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PAPERS AND PROCEEDINGS

OF THE

Seventy-fifth Annual Meeting

OF THE

AMERICAN ECONOMIC ASSOCIATION

Pittsburgh, Pennsylvania, December 27-29, 1962

47

Edited by HAROLD F. WILLIAMSON, *Secretary of the Association*
and
GERTRUDE TAIT, *Executive Assistant*

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PROGRAM OF THE SEVENTY-FIFTH ANNUAL MEETING OF THE AMERICAN ECONOMIC ASSOCIATION

Pittsburgh, Pennsylvania, December 27-29, 1962

Wednesday, December 26, 1962

6:00 P.M.

Executive Committee Dinner Meeting

Thursday, December 27, 1962

9:30 A.M.

Principles of Economic Policy, Consistent and Inconsistent: I. Public Policies with Respect to Private Business
Chairman: R. B. Heflebower, Northwestern University
Papers: Ewald T. Grether, University of California, Berkeley; Ben W. Lewis, Oberlin College
Discussants: Robert F. Lanzillotti, Michigan State University; Jesse W. Markham, Princeton University; Harry M. Trebing, Indiana University; Harvey J. Levin, Hofstra College

Topics in Economic Theory (Joint session with Econometric Society)
Chairman: Oskar Morgenstern, Princeton University
Papers: Robert Eisner, Northwestern University; Dale Jorgenson, University of California, Berkeley; Edwin Kuh, Massachusetts Institute of Technology
Discussants: Carl Christ, Johns Hopkins University; Edwin Mansfield, Carnegie Institute of Technology; Karl Borch, Norwegian School of Economics and Princeton University

Economic Development and the Population Problem
Chairman: Stephen Enke, Duke University
Papers: John Habakkuk, Oxford University; Gustav Ranis, Yale University
Discussants: William N. Parker, Yale University; Paul Demeny, Princeton University; Douglass C. North, University of Washington; Irma Adelman, Johns Hopkins University

2:30 P.M.

Principles of Economic Policy, Consistent and Inconsistent: II. Economics of Our Present Farm Price Support Policy (Joint session with the American Farm Economic Association)[1]
Chairman: Theodore W. Schultz, University of Chicago
Papers: D. Gale Johnson, University of Chicago; Walter W. Wilcox, Library of Congress
Discussants: Hendrik S. Houthakker, Harvard University; George S. Tolley, North Carolina State College; John A. Schnittker, U.S. Department of Agriculture

Industrial Capacity
Chairman: Daniel Creamer, National Industrial Conference Board
Papers: Almarin Phillips, University of Virginia; Charles L. Schultze, Bureau of the Budget
Discussants: Solomon Fabricant, New York University; David Lusher,[2] Council of Economic Advisers; Morris Cohen, St. Johns University

Economic Education
Chairman: Arthur F. Burns, National Bureau of Economic Research
Papers: John R. Coleman, Carnegie Institute of Technology; George J. Stigler, University of Chicago; Lewis E. Wagner, University of Illinois
Discussants: Floyd A. Bond, University of Michigan; Campbell R. McConnell, University of Nebraska

8:00 P.M.

Presidential Address[3]
Chairman: Morris A. Copeland, Cornell University
Address: Edward S. Mason, Harvard University

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[1] To be published in the May, 1963, issue of the *J. of Farm Econ.*
[2] No manuscript received.
[3] Published in the March, 1963, issue of the *A.E.R.*

Friday, December 28, 1962

9:30 A.M.

Principles of Economic Policy, Consistent and Inconsistent, III: International Commodity Stabilization

Chairman: HERBERT STEIN, Committee for Economic Development

Papers: BORIS C. SWERLING, Board of Governors of the Federal Reserve System; RAYMOND F. MIKESELL, University of Oregon; CLARK REYNOLDS, Yale University

Discussants: ROBERT Z. ALIBER, Committee for Economic Development; CHARLES P. KINDLEBERGER, Massachusetts Institute of Technology; GERTRUD LOVASY, International Monetary Fund; DAVID H. POLLOCK, Economic Commission for Latin America

Tax Reform

Chairman: WILLIAM J. FELLNER, Yale University

Papers: DAN T. SMITH, Harvard University; RICHARD A. MUSGRAVE, Princeton University; NORMAN TURE, National Bureau of Economic Research

Discussants: JOHN DUE, University of Illinois; EARL ROLPH, University of California, Berkeley; CHALLIS A. HALL, JR., Yale University

Problems of Regional Integration

Chairman: IRVING B. KRAVIS, University of Pennsylvania

Papers: ERIK THORBECKE, Iowa State University; BELA BALASSA, Yale University; LAWRENCE B. KRAUSE, Yale University

Discussants: RANDALL HINSHAW, Claremont Graduate School; ROBERT C. BALDWIN, University of California, Los Angeles; RICHARD N. COOPER, Council of Economic Advisers

Pricing and Resource Allocation in Transportation and Public Utilities

Chairman: MARTIN L. LINDAHL, Dartmouth College

Papers: WILLIAM S. VICKREY, Columbia University; DAVID ALEXANDER and LEON N. MOSES, Northwestern University; JAMES R. NELSON, Amherst College

Discussants: ELI W. CLEMENS, University of Maryland; JULIAN S. DUNCAN, University of New Mexico; ROY J. SAMPSON, University of Oregon

12:30 P.M.

Joint Luncheon with the American Finance Association[4]

Chairman: EDWARD S. MASON, Harvard University

Speaker: WILLIAM McC. MARTIN, JR., Board of Governors of the Federal Reserve System

2:30 P.M.

Economic and Marketing Aspects of the New Trade Policy (Joint session with the American Marketing Association)[5]

Chairman: WILLARD L. THORP, Amherst College

Papers: RAYMOND VERNON, Harvard University; ROLAND L. KRAMER, University of Pennsylvania

Discussants: HALE A. NEWCOMER, JR., Kent State University; JACK BEHRMAN, U.S. Department of Commerce; DON HUMPHREY, Fletcher School for International Law and Diplomacy; FRANK MEISSNER, Stanford Research Institute

Financial Institutions and Monetary Policy: A Re-examination of Their Interrelationship

Chairman: WARREN L. SMITH, Council of Economic Advisers

Papers: JOSEPH ASCHHEIM, Johns Hopkins University; KARL BRUNNER, University of California, Los Angeles, and ALLAN H. MELTZER, Carnegie Institute of Technology; JAMES TOBIN and WILLIAM BRAINARD, Yale University

Discussants: JAMES S. DUESENBERRY,[3] Harvard University; ABBA P. LERNER, Michigan State University; ARTHUR BENAVIE, Princeton University; HYMAN P. MINSKY, University of California, Berkeley

Principles of Economic Policy, Consistent and Inconsistent: IV. Public Policy Towards the Labor Market (Joint session with the Industrial Relations Research Association)[6]

Chairman: GEORGE H. HILDEBRAND, Cornell University

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[4] Published in the March, 1963, issue of the *J. of Fin.*

[5] Published in the *Winter Conference Proceedings* of the American Marketing Association.

[6] To be published in July, 1963, in the *Fifteenth Annual Proceedings* of the I.R.R.A.

Papers: Simon Rottenberg, University of Buffalo; Frank C. Pierson, Swarthmore College
Discussants: Walter Froelich, Marquette University; William H. Peterson, New York University; John T. Dunlop, Harvard University

8:00 P.M.
Richard T. Ely Lecture
Chairman: Paul A. Samuelson, Massachusetts Institute of Technology
Paper: Jacob Viner, Princeton University
Discussants: Alexander Gerschenkron,[3] Harvard University; George J. Stigler, University of Chicago

Saturday, December 29, 1962
9:30 A.M.
Postwar Growth in the United States in the Light of the Long-swing Hypothesis
Chairman: Richard A. Easterlin, University of Pennsylvania
Papers: Bert G. Hickman, Brookings Institution; Burnham O. Campbell, University of California, Los Angeles; Jeffrey G. Williamson, Vanderbilt University
Discussants: Edward F. Denison, Brookings Institution; Rendigs Fels, Vanderbilt University; Ilse Mintz, Columbia University; R. A. Gordon, University of California, Berkeley
Economic Trends and Prospects in the U.S.S.R. and Eastern Europe
Chairman: Abram Bergson, Harvard University
Papers: Alec Nove, London School of Economics and Political Science; Jaroslav Vanek, Harvard University; J. M. Montias, Yale University
Discussants: Warren Nutter, University of Virginia; Nicolas Spulber, Indiana University
Japanese Economic Development
Chairman: William W. Lockwood, Princeton University
Papers: Henry Rosovsky, University of California, Berkeley, and Kazushi Ohkawa, Hitotsubashi University; Philip H. Trezise, Department of State
Discussants: Martin Bronfenbrenner, Carnegie Institute of Technology; Warren S. Hunsberger, Johns Hopkins University; Hugh T. Patrick, Yale University
Economic Research by International Agencies: A Progress Report
Chairman: Bert F. Hoselitz, University of Chicago
Papers: Caroline Miles and Hans W. Singer, United Nations; John DeWilde, International Bank for Reconstruction and Development; J. Hochfeld, UNESCO, Paris

2:30 P.M.
Conditions of International Monetary Equilibrium
Chairman: James W. Angell, Columbia University
Papers: Harry G. Johnson, University of Chicago; Richard E. Caves, Harvard University; Peter B. Kenen, Columbia University
Discussants: J. Marcus Fleming, International Monetary Fund; Harry C. Eastman, University of Toronto; J. Herbert Furth, Federal Reserve Board
Defense and Disarmament
Chairman: T. C. Schelling, Harvard University
Papers: Alain C. Enthoven, U.S. Department of Defense; Kenneth E. Boulding, University of Michigan; Richard R. Nelson, Council of Economic Advisers
Discussants: Richard U. Sherman, Jr., Ohio State University; Harold J. Barnett, Wayne State University
Problems of Methodology
Chairman: Fritz Machlup, Princeton University
Papers: Andreas G. Papandreou, University of California, Berkeley; Ernest Nagel, Columbia University; Sherman Krupp, Lehigh University
Discussants: G. Christopher Archibald, London School of Economics and Political Science; Herbert A. Simon, Carnegie Institute of Technology; Paul A. Samuelson, Massachusetts Institute of Technology

5:00 P.M.
Business Meeting

6:00 P.M.
Executive Committee Dinner Meeting

THE purpose of the American Economic Association, according to its charter, is the encouragement of economic research, the issue of publications on economic subjects, and the encouragement of perfect freedom of economic discussion. The Association as such takes no partisan attitude, nor does it commit its members to any position on practical economic questions. It is the organ of no party, sect, or institution. Persons of all shades of economic opinion are found among its members, and widely different issues are given a hearing in its annual meetings and through its publications. The Association, therefore, assumes no responsibility for the opinions expressed by those who participate in its meetings. Needless to say, the papers presented are the personal opinions of the authors and do not commit the organizations or institutions with which they are associated.

HAROLD F. WILLIAMSON
Secretary

RICHARD T. ELY LECTURE

THE ECONOMIST IN HISTORY

By JACOB VINER
Princeton University

I feel it prudent to begin by explaining the title of my paper. It is not, except indirectly, a paper on the history of economic thought, although I must confess that several times in the past I have been guilty of smuggling some history of thought into our Association's meetings, and may possibly do so again here. Nor is my paper an account of the influence the economist and his ideas have had on the actual course of history. Keynes may have been right when he said that "the ideas of economists and political philosophers . . . are the chief rulers of the world; . . . it is ideas, not vested interests, which are powerful for good or evil." The techniques, however, by which it is discovered what are the ruling causes in history fall outside the limits of my acquaintance, let alone my capacity to use them effectively. If I may judge by the most conspicuous attempts to demonstrate the dominance of ideas over passions and affections and interests, the techniques involve more dependence on imagination and conjecture than on crude fact laboriously collected. I am also impressed by the extent to which vested interests and going institutions seem to have the power to generate ideas congruent with themselves. Napoleon in his student days read the *Wealth of Nations*. It has been suggested that this is why he later supported sound money, severe economy in public expenditures, and other economic virtues more cherished by Adam Smith than practiced by statesmen. But Napoleon also read economic treatises which opposed these particular virtues. Late in life, moreover, he claimed that he had always believed that if an empire were made of granite the ideas of economists, if listened to, would suffice to reduce it to dust. Even the sector of the world which is reported to have been most influenced by the ideas of economists—the realm of business—seems to have been in the past little disposed to listen to economists, even to those whom it hired or subsidized. If it did absorb ideas from the economists, it must have done so mainly by some process of osmosis not involving deliberate reading or listening. As recently as the late 1920's a leading businessman, important in political and international affairs as well as in business, was reported to have said that of economics he read not at all, as it was "critical, opinionated matter," and that he preferred to form his own opinions from the facts because he felt that "there is always great danger of having your mind all cluttered up with other folk's opinions." This is certainly a real hazard inherent in wide reading, and one that I may not have sufficiently surmounted myself.

If it is not the history of economic thought, nor the history of the causal influence of economists and their ideas on the course of social history which is the subject of my paper, what then can it be? I had considered giving my paper the title: "Why has economics always had a bad press?" This would have described with adequate accuracy the intent of the paper; I had misgivings, however, as to my capacity to make the content of the paper justify this title, and I had in mind the sad fate that befell the author of a book, published in the 1850's, with too revelatory a title: "Comets considered as Volcanoes, and the Cause of their Velocity and other Phenomena thereby explained." A reviewer, Augustus de Morgan, disposed of the book in one sentence: "The title explains the book better than the book explains the title." I have therefore chosen a title which promises nothing in particular. What I try to do in the paper, however, is to sketch the image which economists and their intellectual goings-on have presented over the years to the vision of the lay public, including in the latter the members of other learned disciplines. With internal controversies within the ranks of the economic profession, I am concerned in this paper only so far as there was an outside public which was interested in these controversies and even participated in them.

Since general recognition of economics as a distinguishable discipline or art and of economists as specialized practitioners of it came only late in history, I find little ancient material that is relevant to my topic. I will begin, therefore, with the mercantilists.

It has become somewhat of a fashion to deny that there ever existed a body of doctrine sufficiently distinct and sufficiently self-consistent to justify attaching to it an "ism" label such as "mercantilism." I acknowledge that most abstract terms ending in "ism," including, for instance, capitalism, socialism, liberalism, individualism, conservatism, puritanism, Americanism, inevitably accumulate about them a haze of uncertainty and imprecision. But that is the characteristic of most abstract terms when they acquire mental associations with concrete things or with groups of things which are not in all relevant respects absolutely homogeneous. One can of course attempt to make do with words which have narrowly restricted connotation, but only at the cost of a verbosity aesthetically intolerable both to lecturer and audience and extravagantly consumptive of time. That mercantilism as a term designating a distinguishable body of doctrines and governmental practices is more vulnerable to the charge of imprecision than the general run of terms used by social scientists for such purposes I cannot see. If there is lack of coherence in the doctrine subsumed under the term, the fault, assuming it to be a fault, should be charged against the mercantilists and not against the term. Incoherence is itself a legitimate abstract label for a regrettably pervasive intellectual phenomenon, and if German scholars have not already invented it, I feel sure that they would welcome *Incoherentismus* as an addition to their vocabulary. It is sometimes the same economic historians who object to the term "mercantilism" as empty of content in the absence of a matching body of doctrine who insist that the body of doctrine and of practices to which the label has been applied had in its prime a degree of suitability approaching the miraculous to the contemporary needs of mankind and far exceeding the claims made, as far as I have observed, for any other body of doctrine and practices recognized to be of purely terrestrial origin.

I feel free, therefore, to deal with mercantilism as if it once existed. I will nevertheless say little about it because it had one characteristic which reduces its relevance for the central theme of my paper. It was essentially not a scholar's doctrine, but a folk-doctrine, and I know of no scholar of even third-rate rank who made any contribution to it except that of lopping off one or more of its more obviously diseased branches. As a folk-doctrine it had in the days of its prime no organized opposition, although a variety of vested interests were always pulling at it to make it serve a special group or pecking at it to win a special freedom from its impact. In England, at least, it was not until mercantilism as a body of practices was entering into a coma from internal causes that there began to emerge any systematic and general attack on it as doctrine, with David Hume and Adam Smith as the sole prominent leaders of the attack; and it was not until the English classical school of the nineteenth century made its surviving relics a major target that mercantilism in England also had to endure in increasing measure a partially bad press. It was not until long after the repeal of the Corn Laws that it had a predominantly hostile press, and by that time it was a corpse that was being reviled in the justified fear that there was under way a conspiracy to revive it. I need not, therefore, for present purposes, spend any further time on mercantilism in its days of English glory.

The physiocrats had many of the characteristics of a sect and were so regarded by many of their contemporaries. They had a "master," Quesnay, whose texts they tended to treat as having something of the oracular about them. Although on several issues they never reached unanimity, if one considers how many writers made contributions to the literature of the school and how voluminous their writings were, the degree of unanimity they did attain was remarkable even for a sect.

How wide the following of the physiocrats among the general public was it is hard to say, but the evidence is abundant that they won many disciples and obtained a selective acceptance of their ideas from many others. Among their disciples was a scattering of writers of some prominence in most of the countries of Europe, as well as a few ruling princes. But they also had many critics. All their major doctrines, their general doctrine of laissez faire, their exposition of competition as basically a means of organizing mutual co-operation on a voluntary basis instead of as a field of exercise for egoism, their doctrine of free trade as being useful chiefly as a means of maintaining a "good price" for the products of agriculture, their doctrine of the "sterility" of all nonagricultural activity, their doctrine of "legal despotism" based on the Hobbesian paradox that the most practicable way of assuring the essential freedoms to the people was to establish a complete monopoly of power to an enlightened monarch to be sparingly exercised by him—all of these doctrines underwent severe criticism from many quarters. Among the critics were writers notable in the general intellectual history of France (for example, Linguet, Mably, and Voltaire) and writers of stature in the history of economic thought (for example, Graslin, Forbonnais, and the Abbé Galiani). Voltaire exercised his satiric brilliance on their paradoxical doctrine that since in the last analysis all taxes fell on land rent, with additional costs and inconveniences in the process of shifting from the initial taxpayer to the landlord, they should, in the interest of the landlord himself, be concentrated into a single tax on land rent. Voltaire satirized this in his *L'Homme aux quarante écus,* where a rich banker who is tax exempt makes sport of the poor peasant who pays taxes for both of them. The physiocrats at times fell back on their doctrine of "evidence," or irresistible truth, in expounding what seemed to them to be axiomatic, and they used it also to explain why an absolute monarch, if advised by physiocrats who were ready to display to him the full range of the available irresistible evidence, could be depended on to be a benevolent despot. Galiani disposed in two sentences of the whole doctrine of irresistible evidence, as applied by the physiocrats in support of their assertion that if France removed all restrictions on the export of grain all Europe would follow its example: "It has appeared evident to the economists that the evidence of their evidence would render evident to all nations the evident advantage of free exportation of grain and that all nations would adopt it. Not a single nation has done so." All France laughed with Galiani as it had laughed with Voltaire before. The most damaging satire is that which makes even the victims laugh. From that fate the physiocrats were rendered immune by their constitutional lack of a sense of humor. They were not amused, and they recruited a battalion of disciples to reply. The solemnly serious tone of the replies brought only additional peals of laughter. It seems always to have been true that the only effective rejoinder to clever satire which hits a target at a vulnerable spot is comparably clever countersatire, but it is not characteristic of sects to recruit wits to their ranks, or long to retain any of them that might have blundered their way in.

The single-tax proposals of the physiocrats and their opposition to the feudal privileges of the great landlords turned the aristocracy against them. Their free-trade views antagonized the manufacturers and the privileged merchants. On the two occasions on which restrictions on the export of grain were removed under physiocratic counsel, bad harvests and local famines followed, and this intensified the hostility of the humanitarians to them. Much of the advice of the physiocrats seems to me to have been sound advice for times less politically unstable. Their valuable and lasting contributions to economic analysis as distinguished from policy are on the record. But as has often been the way of economists, the physiocrats too often stepped on the toes of the powerful at wrong times to have been politically successful. To this failure their bad press made a sizable contribution.

From this point on, I will concentrate on the developments in England and will digress to other countries chiefly to point out contrasts with and parallelisms to English experience.

In 1776, Adam Smith published the *Wealth of Nations*. Its subsequent great influence on ideas and even on the course of policy, abroad as well as in England, few would deny. Its influence was slow in becoming manifest, however. The next twenty-five years were perhaps the leanest period in the history of at least English economic thought from the 1690's to the present day. There was not in this period a single general critique of the *Wealth of Nations* in England of any consequence because of the prominence of its author or the quality of its content. In Scotland the record was only a little better. Of all of Smith's students at Glasgow, I can identify only John Millar as a scholar who made any contribution of consequence to economics either as thinker or as expositor. Adam Smith, however, set the pattern of approach and launched the central ideas for a wide range of issues which were after 1800 to be emphasized by the classical school, most notably in the areas of value and distribution theory and of taxation, and above all of laissez faire in general and free trade in particular.

I turn now to the reception of the English classical school as a whole at the hands of the English public. Much of this reception was hostile, even bitterly hostile. Only a few noneconomists praised the school. It nevertheless gradually converted at least the ruling segments of the English population. Part of this conversion can be explained as the consequence of a decline in relative numbers of the most hostile groups as well as a decline of their influence and of their control over publication media. Many of the converts, moreover, were more fully converted in their minds than in their hearts, and adopted classical doctrine with qualms of conscience. In many respects, also, the economists in the classical tradition modified or abandoned some of the classical tenets. The difference, for instance, between the general tone of James Mill's and Alfred Marshall's handling of the economic gospel was greater than that between Marshall and the most sentimental opponent of the rigor and austerity of the classical school in its early days. James Mill, however, was so extremely rigid, austere, pontifical, narrow, belligerent, as to be almost a caricature of the classical school in its less endearing aspects, while Marshall, on the other hand, shrank from participation in open controversy, was so irenic as upon occasion to praise to the skies an economist, notably Schmoller, with whom he had almost nothing in common, strove constantly to blur rather than to sharpen the differences between contending schools and doctrines, and by persistent practice became the great virtuoso in economics of the art of comfortably maintaining a sitting posture between two stools without visible recourse to any substitute means of support. This, I need not tell you, was, however, one of the lesser of his long list of achievements. In any case, when I speak of the English classical school, please keep in mind, even when I fail to do so, its fairly steady progress, from, say, James Mill's approach to problems, to the moderate, the balanced, the disciplined eclecticism and flexibility of that part of present-day economics which still remains substantially respectful of the original classical school, although it has been charged with substituting flabbiness for bigotry. Also, with the compulsions to brevity to which external forces hopefully expect me to submit, I will refrain from bringing to your attention the range of internal dissension, the departures from rigidity of doctrine, the concessions to critics, to be found even within the ranks of the original members of the classical school.

For a school charged with being excessively abstract and removed from reality, it is noteworthy how little there was in the conclusions and the wide range of policy recommendations of the classical school which was not highly relevant to current practical issues, beliefs, convictions, and vested interests. It was only in its choice of premises and in its mode of reaching its conclusions that the school was vulnerable to the charge of undue and arbitrary abstraction. Outside theology, no lay public is normally concerned with the manner in which intellectuals reach their conclusions unless it objects strongly to the conclusions. But almost every one of the major conclusions and the concrete policy recommendations of the classical school touched on the sensitive nerves, ideological or financial, of some important sectors of the population, especially the substantially-allied group of the agrarian landlords and the Church of England as well as the sentimental humanitarians of both conservative and radical species: Laissez faire, free trade, radical reform or abolition of the poor laws, currency reform—on which the school had diverse views—the treatment of rent as unearned income subtracted from the product of labor and capital, the opposition to tithes, taxation reform, the condemnation of extravagance and high-level corruption in government finance, the condemnation of legal monopolies such as the East India Company, the condemnation of the class-restricted electoral suffrage, and so on. From Adam Smith to Lord Sherbrooke in the 1870's, the opposition of one sector of classical economics to withdrawal of religion and higher education from the market test of their proper scope was especially provocative to Anglicans and intellectuals.

The incorporation into the classical economics of Malthusian population theory contributed especially heavily to the dislike of and hostility to the school in important quarters. First, there was the proposition that there was a tendency for population to increase at a rate faster than was consistent with increase or even maintenance of the existing level of living of the working classes. This proposition was held to be one of the premises contrary to fact on which the classical school built its doctrines and its acceptance by the school was interpreted as a gratuitous reflection on the working of providence: "either man has been made too prolific, or the earth too sterile." Second, there was the proposition that to counteract this tendency, the pressure on the poor to exercise prudence in breeding children should be strengthened by withholding from them relief of any kind except in very extreme circumstances, and even then under conditions so burdensome and unpleasant that the poor would have every possible incentive to keep from having more children than they could provide for by their own industry and thrift. This was objected to both as harsh and unfeeling, and as neglecting the fact that a minimum level of comfort and ease was a prerequisite for a large portion of the poor if instead of giving way to demoralizing despair they were to adopt a prudent and self-disciplined pattern of life. The third proposition, as presented by Malthus himself, was that the best available way to keep population in check was deferment of marriage. Malthus, of course, held that such deferment should, morally speaking, be accompanied by chastity prior to marriage. He frankly admitted, however, that by many men this would not be achieved, but claimed that the sexual promiscuity that would accompany deferred marriage was a lesser evil than the misery and consequent vice which early marriages and the consequent unrestricted growth of population would be likely to result in. Bentham and the two Mills, although with less frankness, chose birth control as the remedy. Malthus opposed this, both as morally inacceptable and because he thought that if the sexual instinct could easily be satisfied without involving procreation there would not be population enough. Like their modern successors almost without exception, most of the critics failed to read Malthus carefully enough. They generally failed to see that the classical economists differed in their choice of remedies between two alternative modes of "prudential restraint," birth control within marriage, and deferred marriage with its inevitably attendant sexual promiscuity, although all of them rejected "moral restraint" as impracticable though ideal. However sincere the horrified rejection by the critics of either birth control within marriage or sexual promiscuity outside marriage, their horror at the spectacle of such proposals being presented for public discussion seems to have been completely genuine.

The nature and range of the hostility to the classical school will perhaps be made clearer if I cite a few specific examples of indignation made vocal. The utilitarianism on which the classical economists rested consisted essentially of the propositions that the merit of an act was to be appraised in terms of its consequences, and that "good" consequences were those that served the actor well without injuring others, while for the legislator the sole criterion for judgment of legislation and institutions should be their consequences for the community in general. To the critics this was an ethics of "expediency," of "calculation," instead of one of subjection to the mandates of conscience or of revealed or long-established or self-evident moral rules. Edmund Burke had in his *Reflections on the French Revolution,* in 1790, already exclaimed: "The age of chivalry is gone. That of sophisters [that is, theorists], economists and calculators has succeeded; and the glory of Europe is extinguished for ever." In the same spirit, Lucy Aikin, the biographer, wrote, in 1830: "There is a pseudo science called political economy which dries up the hearts and imaginations of most who meddle with it . . . [it is] a grovelling thing; and . . . a lofty philosophy would act, I believe, as a counterpoise of great value." And Coleridge: "It is this accursed practice of ever considering *only* what seems *expedient* for the occasion, disjoined from all principle or enlarged systems of action, of never listening to the true and unerring impulses of our better nature, which has led the colder-hearted men to the study of political economy." And Carlyle: "Of all the quacks that ever quacked, political economists are the loudest. Instead of telling us what is meant by one's country, by what causes men are happy, moral, religious, or the contrary, they tell us how flannel jackets are exchanged for pork hams, and speak much of the land last taken into cultivation." "May Heaven in its mercy," prayed the Earl of Stanhope in 1826, "preserve us from all the professors of that pretended science, who are more to be dreaded than the Plagues of Egypt." M. T. Sadler, factory reformer and anti-Malthusian, in 1831 called the economists "the pests of society and the persecutors of the poor." I spare you the equally harsh words with which our intellectual ancestors were greeted by Wordsworth and Southey, later by Thomas Arnold and his son, Matthew Arnold, by Charles Dickens, and Charles Kingsley, by John Ruskin, by Benjamin Jowett, and by many others. All of these were persons of considerable eminence, and even if some of them were, to my mind, given to hysteria at times, or were sentimentalists, romantics, of diverse species, it is surely of some significance that they should have reacted in this way even if it is debatable what that significance was, although as I grow older I become increasingly sensitive to the possibility that there are times when sentimentalism and even hysteria may temporarily be socially useful even though always intellectually reprehensible.

Unsentimental politicians did find in the classical economics much that they could usefully adapt to their purposes, and especially the removal of the remaining inheritance of obsolete and class-biased restraints on individual initiative. But even then there was complaint of the lack of agreement among the economists, the variability of their positions from year to year, and their lack of realism. Lord John Russell, in 1821, at an early stage of his political career, opened an essay on political economy, with these rather innocent defects of economics in mind, with the sentence, "Political economy is an awful thing." At about the same time Daniel Webster in the United States wrote in a letter that "I give up what is called the 'science of political economy.' . . . If I were to pick out with one hand all the mere truisms, and with the other all the doubtful propositions, little would be left."

The criticism of the classical school was made to turn largely on their methodology, on the way in which they appeared to reach their conclusions and the logical form in which they presented them to the public for acceptance. Were it not for the widespread dislike for the conclusions of the classical economists it would be hard to explain why so much critical interest was shown in their methodology. Some of their more astute and sophisticated critics apparently saw that their methodology was more vulnerable to attack than their conclusions and that by attacking the methodology they could lessen the appeal of the conclusions without opening themselves as much to charges of self-interested partisanship. It is not true of course that a correct conclusion cannot be reached from faulty premises or from faulty manipulation of undisputed premises. But that is the way for a prudent man to bet, and the critics made the most of this principle in their effort to discredit the conclusions and recommendations of the classical economists. Following the lead of the critics, I will concentrate on the character of the premises used by the classical economists, on what led them to make the particular choice of premises to use, and on what the critics had to say about the quality of this choice. Pure theorists tend to concentrate on what the methodological sophisticates call immanent criticism, or the appraisal of the logical rigor and the elegance with which an economist moves from his premises, whatever they may be, to his conclusions. The critics had some objections to make against the classical economics even in this area, but to engage in immanent criticism effectively one must accept at least provisionally one's opponent's premises, and one must be at least as skillful an abstract theorist as he is. For the most part the critics were unwilling to grant even provisionally the validity of the classical school premises, and were unwilling to undergo the effort needed or were lacking in the capacity to acquire sufficient skill to be able to engage in battle with the classical economists on the field of abstract theory, especially as such battle would have to be fought on ground which the classical economists had chosen. The critics therefore relied mainly on transcendental criticism, although most of them would no doubt have been surprised to learn that they were engaged in so esoteric an activity.

The classical economists worked in the main from categorical premises, treated as axiomatic, or as derived from introspection, or from their own general observation of the external world, or from their impression of recorded historical experience. With a few notable exceptions—for example, Ricardo in the field of money, Malthus in his demographic work—they did not check inductively for validity. They also much exaggerated the extent to which serviceable and genuine axioms are available.

The classical economists conceded that theirs was a "hypothetical" science, increasingly so through the years in response to the weight of criticism. They differed, however, as to what they meant by this. They increasingly conceded that they operated on two levels, the level of "science," and the level of "art," or application, and it was often only the latter which they admitted to be "hypothetical," because the necessary validity of its conclusions was dependent on their conformity to unexplored facts, or on the absence of "disturbing factors," or on their consistency with noneconomic norms or values which the statesman must take into consideration but from which they had abstracted in their analysis. The critics maintained, however, that even the "science" or pure theory sector of the classical economics was "hypothetical" since its premises were not genuinely categorical, but were "iffy" or "speculative" or doubtful, or counter to known fact, or insofar as they were normative in character were arbitrary dogma, were "ipsedixitism," which there was no obligation on anyone's part to accept, and which they believed they had valid grounds to reject outright. They further objected that to be able to distinguish, as the classical economists took for granted that they could do, between fundamental causes and disturbing causes, or between permanent and stable causes and occasional and unstable causes, called for a degree of factual information which one could not come by without a degree of systematic effort which the classical economists made no claim to have engaged in, and that the economists wrongly took for granted that because a factor is a "disturbing" one, or less powerful or less stable than the principal ones, it is *ipso facto* permissible to give no consideration to it in policy making.

The critics still further objected, with special reference to the readiness of the classical economists, from a "higher compassion," to tolerate present misery in the interest of the improvement of the condition of the poor in the distant future, that they were giving too little weight to short-run considerations when these differed from long-run ones. The critics insisted that as a moral principle, as well as because of the uncertainties of life, to say nothing of those of economic analysis, it was wrong to put strong reins on compassion for suffering in the here and now.

This is too favorable an account of the quality of the methodological criticism directed against the classical school by its English critics before, say, 1850. It omits the criticisms made on foolish methodological grounds. It undervalues the contribution which good judgment resting on general information can make to the formulation of a usable set of premises even for policy guidance. It presents the criticisms in my terms, which are more sophisticated, or so I hope, than the naïve gropings of many of the critics. But I believe I can document each one of these criticisms as having been made or implied or approximated by at least one critic, and Whewell in particular stated a number of them with clarity and precision. To the best of my knowledge and judgment, they are all legitimate in principle. They are open to rebuttal only by inductive refutation or by demonstration or judgment that they are properly counsels of perfection and cannot be fully lived up to in practice. If due consideration is given to this, the critics were no doubt often guilty of gross exaggeration of the extent to which the classical school violated them. In the course of the controversy the classical school conceded the validity of much of the criticism in principle. In their best methodological statements they incorporated portions of it. But they made no reply to some of it, and too rarely supplied inductive refutation or made any confession of past or continuing sin in their methodological practice. Of their laissez faire recommendations, Arnold Toynbee, the elder, wrote in 1884: "The bitter argument between economists and human beings has ended in the conversion of the economists." This is not exactly how I would word it, but it correctly reports how much of the world saw it.

Later developments in the controversy over method, although closely watched by members of other disciplines, were of little interest to the more general public. There were now fewer extremists inside or outside the profession than earlier, and, as I have already said, a substantial amount of moderation of doctrine on both sides occurred to make possible, if not a happy marriage, a greater willingness to live in neighborliness without pretensions on either side that they could by themselves adequately cover the whole ground of abstract theory, of systematic induction, of application of theory, and of formulation of social policy, and emerge with definitive answers to all the legitimate questions. To Plato's question, "Granted that there are means of reasoning from premises to conclusions, who has the privilege of choosing the premises?" the correct answer, I presume, is that anyone has this privilege who wishes to exercise it, but that everyone else has the privilege of deciding for himself what significance to attach to the conclusions, and that somewhere there lies the responsibility, through the choice of the appropriate premises, to see to it that judgment, information, and perhaps even faith, hope, and charity, wield their due influence on the nature of economic thought. If a professional group regards itself as having a message to deliver to others than its own members and makes any public claims in that respect, it thereby gives others the right to scrutinize the methods whereby that message was discovered, including the principles, or possibly prejudices, followed in choosing premises. The critics, early and late, of the classical school, and of its neoclassical successors, centered their attack on the classical school premises. They continue to do so. Cunningham in 1891 remarked that in their choice of premises "it is not always easy to tell when a professor of the dismal science is making a joke" and I suspect that Cunningham meant that if the professor was not joking, then he was making a fool of himself. More recently a distinguished economic historian, Postan, complained that "the economists, like the theoretical sociologists of old, only more so, tried to solve the largest possible problems from the least possible knowledge." I have no disposition to concede that the economic historians, or for that matter any other social disciplines, have solved their methodological problems even as well as have the neoclassical economists. In my opinion none has in this respect been reasonably successful, none has given sufficient mental effort to the task, and the chances of real progress in this respect would be maximized if they and we and expert methodologists were to attack the problem together.

The classical economists, as I have already mentioned, met the criticism that their premises were too narrow to lead to usable conclusions by drawing a distinction between the "science" and the "art" of economics, and claiming that the criticism was relevant, if at all, for the latter, but was irrelevant for the "scientific" or "pure" aspect of the economist's operation. Senior, with "pure" economics in mind, said that "political economy is not greedy of facts; it is independent of facts," but he also said that on practical problems political economy has not a syllable of advice to give. What in essence Senior was saying was that it was possible to make honest fictions out of what previously, although without benefit of tradition or revelation, had been treated as authoritative dogma. Walter Bagehot, not always given to using kind words in his references to classical economics and its expounders, even remarked once that "no real Englishman in his secret soul was ever sorry for the death of a political economist; he is much more likely to be sorry for his life." On one occasion, however, he came to the rescue of the economists, on the same line as that taken by Senior, by claiming that they no longer presumed to be both scientists and practitioners of an associated art: "Modern economists know their own limitations; they would no more undertake to prescribe for the real world, than a man in green spectacles would undertake to describe the colors of a landscape." Except for the small but important and esteemed group of economists who today live a completely happy life devoted to dalliance with elegant models of comely shape but without vital organs, how many present-day economists are there for whom Bagehot's statement is even approximately true?

With the growth in numbers of the economists from, say, the 1880's on, and the emergence of a variety of kinds of economists, so that no layman needed to be at a loss in finding some school of economics which provided him with professional justification for whatever policies he was attached to, methodological controversy has now largely ceased to be one between economists and the outside public and has become almost wholly a private dispute within our professional ranks, except that our sister disciplines continue to take pleasure in demonstrating that the motes in our eyes are of larger dimensions than the beams in their own. I will therefore confine my further remarks on the methodological issue, except for a later digression on statistics, to the so-called *Methodenstreit,* in the 1880's and later between the economic historians and the economic theorists. My fellow theorists tell me that the theorists won a definitive victory in this battle when Carl Menger, in the 1880's, demolished Gustav Schmoller. I cannot agree. I believe that the battle was mostly a sham one, and that while Schmoller certainly carried off no laurels, the ones that have ever since been bestowed on Menger for his victory in this battle are tinsel ones.

Schmoller denied to economic theory, at least in its then shape, any usefulness. He claimed that the task for the economist, at least for another twenty years or so, was to accumulate historical data, after which would come a stage when the accumulations of factual data, having then presumably reached the critical size, would reveal, to the economist contemplating them, the pattern of the historical and other laws which express the causal relations between social phenomena.

Menger succeeded in exposing to ridicule the notions that disciplined thought could proceed without any recourse to abstraction, and that the mechanical accumulation of data, historical or contemporaneous, could in some spontaneous way contribute to understanding without the aid of selection, classification, and analysis of these facts under the guidance of some kind of theoretical, that is, abstract, reasoning. But Schmoller was not a worth-while target for methodological attack. His own practice was superior to his doctrine; even his close associates regarded his doctrine as too extreme. I have not been able to find any economic historian whose practice was nearly as vulnerable as Schmoller's doctrine. Menger, in his repeated attacks on Schmoller, admittedly made under great provocation, "thrice routed his feeble foe, and thrice slew the slain."

The real challenge which Menger should have faced was not that of justifying in principle recourse to abstraction by economists, but of justifying the particular extent and manner in which he and his fellow theorists practiced it. As justification for the economic theorist's practice, Menger appealed to the theoretical chemist who mentally separates water into pure hydrogen and pure oxygen. Schmoller pointed out that the laboratory chemist was there to take up where the speculative chemist left off. He could have replied also that the speculative chemist had himself taken off from a body of well-authenticated fact, and that between even the laboratory chemist and final application of the analysis there were the pilot plant, and the artificial controls against disturbing factors in the commercial-scale plant itself, which had been installed with the aid of scientific experiment and of trial and error by experts. To Schmoller's reference to the chemical laboratory, Menger replied that Schmoller misunderstood the role of abstraction in economics, which consisted not in random abstraction, but in the separation of the "essential" from the "accidental" causes. Elsewhere he spoke of economic theory as concentrating on the "ruling" causes and of "causes" as distinguished from "conditions." It seems to me that this begs most of the important questions because the real issue here turns on how at the pure-theory stage one can distinguish an "essential" from an "accidental" cause, a "ruling" from a non-ruling cause, a "cause" from a "condition." It is at least a plausible conjecture that successful achievement of these results requires, among other things, a methodical pursuit of factual knowledge with the aid of tools of art whose use calls for special skills with which the pure theorist as such is not endowed by nature and which he must either borrow from other disciplines or acquire by painful effort.

A major one of these tools is, of course, statistical method, and it is one of the major glories of our profession that it has made important contributions to its progress to its present magnificent status. Its first stage, political arithmetic, in the seventeenth century, had William Petty, an economist, as one of its founders. In the eighteenth century came moral arithmetic, whose link with political arithmetic is not that it makes use of statistical method, but that it also was concerned with measurement of a kind and made use of mathematics of a kind. Francis Hutcheson, Adam Smith's teacher, made an unsuccessful attempt to apply it to psychological entities, but abandoned it when it met with ridicule. Bentham tried to develop a "felicific calculus" by means of it. Much later, Jevons built on Bentham, for the first time, to develop utility theory. Early in the seventeenth century in England there had been a wave of intellectual enthusiasm for measurement, although without any immediate product outside the natural sciences in actual measurement, as far as I know. The term "pantometry" was then coined to signify belief in the possibility of and zeal for extending measurement to all phenomena. The term did not take hold, but it would have fitted very well the fad for measurement in the form of counting, or of statistics as consisting of the computation of aggregates and averages, which in the 1820's on spread like a contagion over Europe and the United States and led to the foundation of a large number of statistical societies. With most of these developments economists in the Smithian tradition had relatively little to do until the advent of Edgeworth and Bowley. Adam Smith had declared in the *Wealth of Nations* that "he had no great faith in political arithmetic" and although some of the classical economists made use of statistical data, they did not discuss statistics as science or art. Since there was, however, some participation by economists in the early statistical societies, and since the compiling of data in statistical form occurred largely in the area of economic data and of closely associated demographic data, a critical outside public which found in the operations of the statisticians food both for indignation and for laughter tended to lump statisticians and economists together as a common group of counters and of calculators to be scolded or laughed at as a unit. This was not the first nor the last time that economists have been charged with guilt by association, but one expert on the period has remarked that the only change he could see as the result of the shift of interest from theoretical polemics to statistical fact gathering was a progress "from Humbug to Humdrum." The efforts at quantification were pioneer efforts, pursued with simple and unsophisticated tools, and often premature, and those who were engaged in these efforts were in many cases simple amateurs facing better-educated and more philosophical-minded critics. To indicate the character of the criticism, I will give some specific examples, although I must acknowledge that I have made my selection from the large body of available material partly with a view to its possible entertainment value.

Jonathan Swift, Daniel Defoe, Bernard Mandeville—a formidable team of satirists indeed—had already in the eighteenth century taken their swipes at political arithmetic, for the unreliability of its results and the presumed unmeasurability of some of the data it measured. They discovered early that a fool can be a bigger fool with the aid of mathematics than without. Edmund Burke later, in perfect dead-pan, presented an exercise in political arithmetic, in which he proves that there had been seventy-five times as many persons killed as a result of wars as there were persons alive at the time of writing. He guesses almost at random the number of dead in various wars, adds up these guesses by means of erroneous arithmetic, and then multiplies this sum by 1,000 to allow for possible error. This may be carrying parody of honest and pioneering effort a little too far. But it has been more than matched by a statement made in 1948 by a distinguished Princeton atomic scientist: "The present experiments indicate that the negative mesons are absorbed by atomic nuclei only one billionth as rapidly as calculated by the theoretical physicists. This would be a major error even for an economist."

Later, the travels of the nineteenth-century statisticians and economists to different congresses and annual meetings were a special irritant to some. Section F of the British Association for the Promotion of Science, first called the Statistics Section and later the Economic Science and Statistics Section, was in this respect a popular target. The annual meetings of the Association were held in a different town each year, and they even met in Montreal one year. There was always much publicity and much crowding, but Section F tended to outdraw the other sections both in numbers in the audience and in press publicity, perhaps because the intellectual level of the lectures was closer to that of the general public than when the topic was in the field of it had among its membership adherents of almost every school of economic doctrine and almost every political party except outright Marxian socialism. A few of its prominent members embraced Nazism, and Nazi intellectuals discovered in Adolph Wagner, with some but not gross exaggeration, an anticipator of Nazi doctrine with respect to authoritarianism, racialism, militarism, expansionism, and state socialism, and paid due reverence to his memory. The *Verein,* unwilling to submit unconditionally to the stringent terms which the Nazi regime made a condition of its continued existence, in December, 1936, liquidated itself.

Because German economists generally found themselves predominantly in harmony with the reigning political tendencies and their organizations were affiliated with powerful political parties, they never seem to have stood out, as did to a large extent the English classical school and the French liberal economists, as either an isolated intellectual group or a group belonging to a political and social minority. There was no shortage in Germany of public criticism by one more-or-less organized group of economists of another such group, but I have not been able to find any trace of a German literature of protest against economics as such or economists as such.

Americans who went to Germany for their graduate training were prominent among the founders of our Association, and some of these undoubtedly looked upon the *Verein für Sozialpolitik* with its ethical, welfare state, and anti-laissez-faire approach to economics as, in these respects at least, a suitable model for the Economic Association. This led to internal struggle within the Association and to some reluctance to accept membership on the part of a few economists who were strongly in the English classical school tradition. Before long, however, as in the *Verein* itself in Germany, and as in England, doctrinal divisions became less sharp among us, and the Association became what it is today: a professional organization of economists with its sole objectives those of promoting the interests of the profession and the discipline, with no desire of and no machinery for committing the Association to any particular social program or economic doctrine. So may it continue.

I have not said anything about the attitude of the American public to the economics profession. We have had our days of bad press in the past. One cause was our free-trade predilections which did not please some powerful sectors of the population. That many economists should be free-traders was, of course, a provocation to right-thinking men in the days before World War I. Theodore Roosevelt wrote to Henry Cabot Lodge in 1895: "... thank God I am not a free-trader. In this country pernicious indulgence in the doctrine of free-trade seems inevitably to produce fatty degeneration of the moral fibre." Until I read this letter, I had always wondered what was wrong with my moral fiber. Now I know. American business organizations and businessmen trustees and regents of colleges and universities, who in the past have at times acted as if they had played hookey in their school days from the classes on civics, have at times made life somewhat miserable, momentarily at least, for worthy members of our profession, including several of our founding members who were laissez faire with respect to foreign trade, but not laissez faire with respect to trusts, monopolies, the trade-union movement, and pauperism. Some of our views on monetary policy and on progressive taxation have also at times failed to endear us in these circles. The addiction of some of us to that mystery of iniquity, aggravated Keynesianism, has attracted the painful attentions of a group of the more solid-headed alumni of Harvard. It was held against some of the early American economists that they opposed slavery, and against some others that they were not opposed to slavery, moral principle then, as always, and perhaps more so in this country than in others, varying with geography.

"On the whole," however—or, with reliance rather on promiscuous averaging than on promiscuous aggregating, I should perhaps say "on the average"—the American economist has been dealt with fairly by the American public. It has laughed at us at times, because we do not always speak with a single voice and because despite many years of sad experience to the contrary some of us persist in operating as if we can forecast. But these are appropriate objects for moderate laughter. In England, in France, and in other countries in the nineteenth century organized religion mildly chastised certain schools of economics, but in our nineteenth-century America much of the teaching of economics was in the hands of the clergymen, and whether it was laissez faire, or antislavery, or free trade that was in issue, or later, the American version of socialism of the chair and the pulpit, religion and economics each pursued the good life, largely side by side, and sometimes hand in hand. So may it continue.

"And here shall be an end."

DISCUSSION

GEORGE J. STIGLER: I became a student of Professor Viner's some twenty-eight years ago—I find this number shocking—and I have never extricated myself from this position. Perhaps that is because I have not tried; but I prefer the more flattering explanation that I have had the good sense not to try. The historians of our science are too small in number to form even a bargaining unit, and too confident of going to the scientific heaven to need one, but if and when we do organize, Viner will be the shop steward.

But I have never envied the person who was asked to discuss a Vinerian paper. His great erudition is not the fundamental problem, for one can always contrive a previously unpublished remark by Adam Smith's mother which places the whole problem in a new light. Nor is his analytical perspicasity the problem, for one can always acknowledge it and pass on to the climate of opinion, which is always changeable and often foggy. It is not quite so easy to handle Vinerian mischief, but when one recognizes that it is fundamentally a serious attempt to cope with intellectual complacency, one can at least try to keep out of its way. What makes Viner hard to discuss is that he is right.

The brilliant survey we have just heard raises a question to which I shall devote my few comments. The question is: is it desirable or undesirable for a science to be unpopular—and I mean unpopular, not merely lacking in popularity?

There are two polar types of economists with respect to the method employed to influence public opinion and action.

At the one extreme is the statesman-economist—I use the term with respect—who never presses an extreme view, who genuinely recognizes the complexity of all major policy problems, who does not find all virtue on one side, and who does not wish to move too quicky even to an indisputably preferable position. This statesman does not abandon his own professional conviction for current popularity, and he does not merely represent a sort of political opinion poll. He exerts a genuine if modest influence on events. Some policy adopted on a certain January 1 is better because of his participation.

This economist-statesman will be a respected figure. People instinctively respect scholars—some of the atmosphere of the medicine man still clings to us—and the statesman reinforces this respect with the breadth of his vision, the tolerance of his wisdom, the practicality of his judgment. Politicians will find him useful even though he is not a pawn. Businessmen will find him comprehensible—since he does not go in for formal analysis at the policy level—and realistic.

If we were all of this type, our craft would be widely admired and respected, just as Christian ministers are if they do not try to change their congregations much, but simply argue for less adultery and more charity. The influence on the long-run course of policy of such behavior is a problem to which I return. At the other extreme is the purist, who wishes to implement his economic reforms directly and not later than Friday. His reforms will be stated in stark, and preferably outrageous, terms, to dramatize their differences from the present situation. His policies are right, and alternative policies are wrong, as can be shown mathematically. The "practical" difficulties in a radical change of policy are to him a euphemism for cowardice and mental confusion.

This bold man is not very widely admired for his wisdom. In fact he is more commonly regarded as a fanatic, and his membership in a commission is a guarantee of sweeping minority opinions. But he is more feared than despised: he works within a well-defined analytical framework, and his position has a clarity and logical rigor that makes the statesman's positions look wooly and slack.

There have been individual economists who have approached these poles. Indeed Wicksell and Marshall are easy to classify on this basis, and a well-informed man might even find living examples. But what concerns us here is that since the time of the physiocrats the economics profession as a whole has preferred the purist role. Of course, the profession has not gone all the way to the pole, but it has not often strayed outside the Arctic Circle of consistent, sweeping policy proposals based upon the discipline's findings. I could support this view by analyzing the policy stances of major economists, including Smith, Malthus, and in some respects even Mill, and of course Ricardo, Marx, and Keynes were built to my specifications.

Or consider the policy views. The nation raises tariffs for a century, after which a thousand economists sign a petition demanding that they be lowered. The community crystallizes centuries of experience with irresponsible rulers into a rigid dogma of financial solvency, and economists then praise big deficits and assert that domestic debt is a much smaller obstacle to national greatness than the Rocky Mountains. The economy distinguished in all history for its large enterprises is told to increase antitrust action manyfold. A society in which advertising has become noticeable even to a careless observer is told by a profession (which is not bad at advertising itself) that advertising is a grossly wasteful activity. And I pass over such modest minority views, not wholly lacking in prominence, as that the Federal Reserve Board should be abolished, the agricultural programs plowed under, the railroads deregulated, labor unions fragmented, industrial prices other than steel set by Senate committees, and the value of the dollar set by a free market. However, you need not accept my characterization of the profession as mostly purist, even though I should consider it statesmanlike of you to do so. The question remains: which approach is more influential?

This is as difficult as any question the history of economics tackles, and I share Viner's misgivings about our present ability to answer it. But I shall briefly state my reason for believing that Keynes was right, and that public men and affairs are, if not the slaves of a defunct economist, as least part-time employees of a purist. What I say may have intuitive appeal; but it remains a task of scholarship to test it.

The statesman seeks to change events slightly, but more or less continuously, and thus to exert a substantial cumulative effect upon the trend of policy. He suffers at least two major handicaps in this role. First, he is meeting all other protagonists of policy on the same ground, the politician's ground, and he has astronomy or of physics. In the criticism of the peripatetic character of the meetings, I am not aware that the objection was raised that it was an impious anticipation of the millennium, for Daniel (XII.4) had promised as one of the millennial blessings that: "Many shall [then] go to and fro, and knowledge shall be increased." But a Dean of York protested at his bishop's acting as chairman at one of the Association's meetings, as it was a gathering of "peripatetic philosophers," of no benefit to science and injurious to religion. Charles Dickens published a parody on the meetings of Section F, entitled, "Full Report of the First Meeting of the Mudfog Association for the Advancement of Everything, Section C . . . Statistics," which included an account of a census of the nursery rhyme books read by London children, of an enumeration of the number of chairs in a Yorkshire town which revealed that there were not enough chairs to give every one a seat simultaneously, and of a careful compilation of the extent to which members of Parliament from a particular section of England evaded the property qualifications for such membership, which showed that both the total and the average amount of such qualifying property held by these members equaled £0.s.0.d.0.

Punch, in its first year, 1841, instituted as an annual feature a report of the annual meetings of the London (later Royal) Statistical Society, which satirized the promiscuity, the triviality, and the futility of the statistical compilations presented at these meetings, but unfortunately with a humor heavier than lead. Perhaps in the fear that the widespread ridicule of the Section F meetings would injure the dignity of the other sections, perhaps because, as the statisticians insinuated, the scientists were slightly jealous of the superior drawing power of Section F, but with the stated ground being that the meetings of Section F did not approximate a scientific character closely enough to warrant having a place on the British Association's program, the Association, in 1877, considered abolishing Section F, but after some negotiations with the statisticians abandoned the idea. Section F, I understand, continues to flourish, without any question now being raised about its scientific credentials.

In France, also, there was apparently readiness in some quarters to have fun at the expense of the statisticians. At the Fiftieth Jubilee meeting, in 1885, of the Royal Statistical Society, A. De Foville, a delegate from the corresponding French Society, read a paper on "Statistics and Its Enemies," which I would have been happy, if time permitted, to quote *in extenso*. De Foville gives an account of a character, an illustrious statistician, in a novel by Goudinet (*Panache*), who finds that in his *Département* there were 17¾ married men per square kilometer but only 16½ married women and points to the need, therefore, to marry 1½ men to 2¾ women per square kilometer if things are to be brought into balance. De Foville reports also on another statistician, a character in a novel by Louis Reybaud (*Jerôme Paturot*), who tries to recruit a retired hosier to the profession. The latter hesitates because of his lack of confidence in his ability to carry out statistical computations. The statistician replies: "It's but a bagatelle, my friend. . . . All that is necessary is a little bit of self-confidence. For instance, you announce: 'There have been harvested in Spain 3,500,300,000 and a half sheaves of wheat. Take note of that *half,* it is essential; it is the touchstone of a meticulous calculation. That *half* will capture the respect of your audience.' 'Look,' they will say, 'what exactitude. These experts count to a fraction.' . . . With your half a sheaf of wheat, you have won more convinced hearers than with your three billion."

One's eyebrows rise at the lightness with which De Foville treated his discipline at what presumably was a very solemn occasion. I tremble slightly at the idea of the consequences of a comparable performance at an annual meeting of our own Association. De Foville, moreover, did not stop here. Such stories, he said, do not bother statisticians. "Instead of recognizing himself in these caricatures each of us has available the resource of seeing in them the portrait of his colleagues, and why not then laugh more heartily than anyone else?" And as if all this were not more than enough, he ended his talk with the comment: "I add that it may even be possible for us to draw some useful lessons from this banter." All things considered, perhaps it was fortunate that De Foville delivered his talk in French.

I should mention two more criticisms of statistics that were occasionally made. The early statisticians regarded their operations as a substitute for, or at least an improvement on, the economic theory approach to the study of economic problems. But Whewell, who criticized classical theory as unduly abstract and as making excessive claims for its conclusions, was not himself antitheoretical in principle or in practice, and at an early stage in the history of statistical societies he urged them to recruit theoretical-minded men and to avoid confining their activities to mere fact gathering and counting. Second, underlying some of the objections to statistics was the fear that the growth in its popularity would lead to an inappropriate application to phenomena for which they had no true relevance of the language if not the substance of measurement and of mathematics. There could not then be foreseen the development of statistics itself into a theoretical discipline and the discovery of ways of making useful servants to economic theory in general not only of statistical theory and of the classical types of mathematics, but also of non-quantitative mathematics, and eventually, perhaps, even of nonmathematical mathematics. What now seem criticisms resting merely on bigotry, or ignorance, or professional jealousy, could then, therefore, have been criticism somewhat justified in the circumstances of the time.

Coleridge's objection, with reference to geometry, of the application of time-concepts and space-concepts to mental phenomena for which he thought they were meaningless, is a case in point.

"There are," said Coleridge, "Acts and forms of Being as alien and heterogeneous to *Time,* as our Thoughts, Affections and Passions are to Space. For example, what should we think of one who said that his Love of his Wife was North West by West of his Passion for Roast Beef?" For Coleridge as a poet, the finding of similarities between things seemingly totally incomparable with each other and the invention of modes of speech indicating these similarities was a standard trick of the trade. We may imagine, therefore, that it was the treatment as similars of love and roast beef, or perhaps the treatment of love in terms of so prosaic a thing as geometry, which Coleridge found objectionable, rather than the use of geometrical ways of comparing things on the same level of prosaicness. I doubt whether Coleridge would have objected, if he had come across it, to Bobby Burns's assertion that "love is like a red, red rose." Coleridge, in any case, could not have foreseen the development of welfare-indifference-curve technique, for which the comparison in geometrical terms, not only for individuals but for communities as a whole, of, say, a pound of roast beef and an hour of felicitous leisure spent with one's love, where these were alternatives, would be accepted by the expert as neither insuperable nor unintelligible.

I will conclude with some comments on the occasional endeavors made by economists in the nineteenth century and since to increase their effectiveness as guides to the public by organizing themselves into groups or societies which should speak to outsiders with an approach to a single voice. In England, the Political Economy Club of London was founded with the promotion of free trade as one of its objectives, although it deliberately maintained a small membership, kept its meetings private, and almost at once discovered that inside and outside its meetings its members would display no disposition to attain a uniform view on anything. The Manchester School was a frankly partisan group. No economist of any distinction, as far as I know, was among its members, and a number of the leading economists disavowed any connection with it and its extreme laissez faire dogmas. In the great battle in the first decade of the twentieth century over "Tariff Reform" the British economists divided publicly and sharply into two groups, and each issued manifestoes to the public, with the prominent theorists on the free-trade side, and a mixture of economic historians and others on the wrong side. I am not aware of any other moves towards the organization of economists on partisan lines in the history of the economics profession in England.

In France, the liberal, free-trade economists throughout most of the nineteenth century had their separate organization and publications and much of the time controlled some of the major teaching posts in Paris. The Liberals, as commonly in Catholic countries, were anticlerical, while the church authorities and those writing under its discipline were anti-individualistic in principle and also anti-laissez-faire. Catholic moral theologians, moreover, then as now, opposed the separation of economics from ethics and religion. Except for a few recent instances in the United States, I know of no works in "pure economics" written under official Catholic auspices, and these American instances present the "pure" theory only on a contingent basis, subject before application to policy to correction for any possible lack of conformance to Catholic ethical and religious doctrine. What this means is that "liberal" or laissez faire economic doctrine has always had a predominantly bad press in Catholic circles, and that "Catholic economics" has throughout the world been presented to the public in a substantially uniform pattern as far as ethical presuppositions and social-policy recommendations are concerned.

In Germany, there was a tendency for "economic" organizations, including those in which academic economists participated heavily, to have a unified doctrine with respect to social policies, and some of these associations were affiliated with political parties. With the Prussian military successes after 1860 and the political unification of Germany in 1871, opinion on the part of the economists moved with public opinion in general. The free-trade and liberal organizations shrank in numbers and influence, and the academic economists became increasingly adherents of the nationalist, expansionist, and antidemocratic doctrines dominant in the German ruling classes—a development for which I see no parallel in the economics profession in other countries.

In 1872 was founded, under the sponsorship, among others, of Gustav Schmoller and Adolph Wagner, of the *Verein für Sozialpolitik,* which almost at once became by far the most influential organization of economists in the world. It had its internal divisions with respect to policy, but throughout most of its history it strove to attain a common policy, and for many years it was substantially unified in support of an "ethical" or anti-laissez-faire program of social legislation, of "socialism of the (academic) chair," of protectionism, and of expansionist nationalism. Later in its history it came closer to being a "scientific" or eclectic organization and at the advent of the Nazi regime no special strengths in this competition. When noneconomic, or special empirical, considerations are dominant, the economist has one small voice of many. Second, his role is based upon the assumption that the process of evolution of social policy is a rational, deliberative process, in which momentum is less important than incremental decision—in fact that momentum is the sum of incremental decisions. This is not a realistic assumption on my view of political life.

The purist abandons all hope of immediate influence and seeks only to dominate the trend. He insists upon fighting on his own ground and usually vanquishes those who are foolish enough to attack him there. He captures the imagination of the young and hopeful and naïve, and many of them lose only youth as they grow older. It is hard to believe that a Ricardo who argued for a two-shilling reduction of the duty on corn, accompanied by an expanded program of rural roads in those parts of England where some parliamentary votes could be picked up, would have aroused the young or influenced the old.

My belief in the greater long-run influence of the purist is not due to the historical evidence on this matter, for this evidence has never been subjected to systematic analysis and at present consists of hand-picked illustrations. Rather it rests on the behavior of a smaller society—that of the economists themselves. Within their discipline they proceed also by consenus, and rational arguments play at least as great a role in scientific consensus as they do in political consensus. Within our discipline, however, the evidence does seem to me strongly in favor of the dominant influence of the purist, as against the statesman. Indeed, if I were to ask you to list great economists of former times who treated theories of their own and theories of other economists with even-handed justice and forbad you to include in your list any names beginning with the letter *M*, you would have your troubles. But I shall not ask this question.

Instead, I shall ask another question: Professor Viner, you have given the first Richard T. Ely Lecture. Are you prepared in future years to honor Fisher, Taussig, Walker, Clark, and possibly Senator Sherman? I pray so.

PRINCIPLES OF ECONOMIC POLICY, CONSISTENT AND INCONSISTENT: PUBLIC POLICIES WITH RESPECT TO PRIVATE BUSINESS

CONSISTENCY IN PUBLIC ECONOMIC POLICY WITH RESPECT TO PRIVATE UNREGULATED INDUSTRIES

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I. *Introduction*

It seems necessary to suggest the significance of two terms in the title of this paper. "Unregulated" is taken to mean the nonpublic utility sectors of the economy; it is not intended to suggest that any sector of our economy is in fact "unregulated." Thus, for example, I shall have some comments about agriculture, which can scarcely be called "unregulated"; yet it is not, as of now, in the public utility sector except, perhaps, in the case of milk marketing and pricing. "Consistency," in the dictionary sense, is taken to mean general harmony or agreement or coherence within the parts of a whole—in this instance, public policy. It is assumed that full or perfect consistency is not and should not be obtainable in an area as broad and elusive as the one under discussion. Rather, our problem is one of reasonable, attainable consistency under the play of numerous, complex, and conflicting forces.

We need to think clearly on this matter. Our democratic federal system, with its mechanism of checks and balances and combination of centralization and decentralization in decision making, tends inherently to allow and even make for some inconsistencies. Furthermore, there is nothing in our constitutional protections that would guarantee full consistency. Our protections go only to basic freedoms and rights and to avoid unreasonable discrimination. Within these broad and elastic boundaries, federal, state, and local legislators may and do enact all sorts of contradictory legislation in the name of freedom and public safety, the public interest and the general welfare. To some extent, the courts set limits—but never in terms of absolutes. Historically, "political balance rather than economic consistency has been the more powerful drive."[1]

Our discussion will focus upon public policy in relation to the functioning of our competitive market system in fulfilling its basic roles of co-ordination and allocation of resources and the resulting "private regulation." The competitive market system is in no sense to be confused with society. We shall be looking at only one aspect of our society, although withal a most important one in terms of the values and goals of a society with a private enterprise base.

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[1] Wm. Letwin, Ed., *American Economic Policy Since 1879* (Chicago: Aldine Pub. Co., 1961), p. xxx.

We shall look first at the recent historical drift or direction of change, second, at our present situation and the divergent opinions of the immediate outlook, and, third, at the crucial decisions that must be faced if we wish to maintain an effectively competitive market system. Unfortunately, the discussion must be highly selective and in terms of global generalizations involving a large element of personal value judgement.

II. *Our Direction of Change*

A. *The Accumulative Drift Away from Generally Effective Competitive Market System*

It is extremely difficult to gain perspective upon the shifting panorama of change.

Transfers into the "Regulated" Sectors and Grants of Partial Exemption. In the last century, we had begun gradually to remove portions of economic activities by statute from the play of full market forces. These political processes have continued into our own period. Markets, while "regulating," are also being "regulated." Piecemeal we have transferred portions of our economy from the unregulated to the regulated public utility sectors or have provided some amount of exemption from the rule of competition in the open sectors. The number and degree of these transfers into the regulated sectors and of the special exemptions now defy precise quantitative portrayal. The degree of transfer or exemption is in itself often an unadjudicated variable as is also the extent to which market valuations are applicable in both internal operations and in rate fixing.[2] Furthermore, surrounding the areas of partial exemption or protection are twilight zones of uncertainty into which the exempted industries and enterprises reach from their protected positions.

Frequently there are conflicts and inconsistencies as between federal, state, and local governmental units. Very few states, for example, have supported the federal government actively in antitrust enforcement; on the contrary, restrictions have been winked at or even promoted or managed by the states. Restrictive licensing procedures intended to limit competition have been rampant under state and municipal authorities or cartel-like self-regulation has been accepted.

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[2] For an endeavor to portray this spectrum, see T. E. Kuhn, *Public Enterprise Economics and Transport Problems* (Univ. of California Press, 1962), p. 23.

But perhaps equally significant and more subtle than the partial exemptions from full market competition have been the open and hidden means of subvention and of overt differential treatment.

Subsidies and Differential treatment. All political jurisdictions have accepted subsidies and subsidy-like differential treatment as part of the way of life. The mere listing of types of federal subsidies, for example, direct and indirect, would require all the time allotted on this program.[3] Practically all major and minor industries and states and local communities are now or have been recently the beneficiaries of federal subventions, aids, grants, and supports in some form. The forms are so diverse and the means often so disguised as to make full recording and quantitative or qualitative appraisal well-nigh impossible. Open, outright subsidies clearly promulgated and controlled are probably less important quantitatively than the indirect, hidden subventions.

Subsidy and subsidy-like procedures should not be condemned out of hand, for there are good reasons for their use in terms of the conservation and development of natural and human resources, education, research and development, pump priming in times of depression, and the like. A recent study of federal subsidies by the Joint Economic Committee concludes as follows: "Subsidies have expanded to the point where few segments of our economy are completely unaffected by them . . . it is unrealistic either to condemn or to praise Federal subsidies as such. Each particular program . . . must be judged independently, taking into account the economic, social, and political conditions prevailing at the time."[4] But this is precisely the nub of the issue so far as market competition is concerned. Case by case, separately, in terms of economic, social, and political considerations, almost any form of subsidy can be rationalized somehow at a given moment of time. Clearly distinguishable among the threads in the weaving, however, are those subsidies intended to protect established interests or bolster and shore up weak economic situations instead of allowing market competition and discipline to run its course. The discipline of the market is subverted or removed entirely and usually without the provision for any other equivalent scrutiny and control. On the contrary, through the processes of financial capitalization[5] there is bred a basis and demand for continuation and even expansion. And in these processes, the chief benefits are most likely to accrue to the established and more prosper-

[3] For such a listing see: *Subsidy and Subsidy-like Programs of the U. S. Government* (Joint Economic Committee Print, G.P.O., 1960).

[4] *Ibid.*, p. 80.

[5] "An acre of tobacco allotment . . . was estimated to add $1,673 to the sale value of tobacco farms in Pittsylvania County, Virginia, during 1957." *Technical Bulletin* No. 156, Agric. Exp. Sta., Virginia Polytechnic Inst., Jan., 1962, p. 2. See also *Technical Bulletin,* No. 148, Apr., 1960.

ous members of the industry or community or to sharp, speculative traders instead of the weaker ones whose alleged plight and needs provided the basis for the original rationalization of the subvention. There will always be marginal and submarginal enterprises, frozen into the industry by the subventions or perhaps lured into it by them, sufficient to justify the continuation or even increase of the artificial supports.

Agricultural Industries. The agricultural industries demonstrate clearly the issues and impasses created by long continued programs of subsidization, direct and indirect, open and hidden. Many believe that we should now follow the logic of accumulative forces to their end by placing major agricultural industries under rigid governmental supply management. Surely no sterner warning need be given as to the accumulative consequences of continuing programs of short-run, piecemeal subsidization. We should soon decide whether to continue agriculture under its own special and overlapping jurisprudence, partially insulated from the market system.

The type of rationalization that would now support the admittedly more consistent supply management approach would seem to fly in the face of the long-run drift of agriculture. Traditional small diversified family-type farming is under the pressures of accumulative forces of change, especially the trends towards specialization and of increase in the scale of operations and of partial subsidization by other sources of family income. The native values associated with classical, family-type diversified farming, the woodshed, the roothouse and miscellaneous outhouses, and sharp demarcation from the city, are also disappearing. Farms are no longer the major seedbeds of our national population increase. Farming increasingly has become a business instead of a way of life, with the size of the business units increasing in order to meet the requirements of economy and efficiency. Much of what used to be called farming is now business activity within the boundaries of metropolitan city governments or closely surrounding them. Except in a few remote areas, the traditional distinctions between urban and rural life and affairs are rapidly breaking down, or have already disappeared. Increasingly, there is less reason for the special treatment of farming more or less outside the competitive market system. There should be relatively little reason for such treatment except on occasion in relation to temporary situations, if agriculture, step by step, were brought into better basic adjustment.

The decisions with respect to agriculture have enormous portents for our entire economic and political system. If agriculture is diverted into a tightly regulated series of industries, then the pattern is set for similar diversions step by step elsewhere in our economy. Inherent in this approach would be the Balkanization of regional and commodity markets in a manner entirely contrary to the trends of modern technologies and our traditional internal free trade.

Governmental Expenditures. Perhaps most important of all, in our immediate situation, are the resounding impacts of governmental expenditures, especially of military procurement, upon the market economy. It is often stated that the Pentagon exercises more influence for or against the policy of active market competition than the Antitrust Division of the Department of Justice and Federal Trade Commission combined. Some believe that the massive defense expenditures amounting to $228.4 billion from 1950-59 have been a tremendous force making for the concentration of economic and market power. The facts are difficult to establish. In a recent report of the Joint Economic Committee, it is stated that "military procurement, to a large extent, is not conducted on a fully competitive free enterprise concept" and hence "it is apparent that the subjective decisions by procurement officers have a heavy impact for good or ill upon the national economy."[6] Much of military acquisition, especially for new weapons and weapon systems, must be through administrative procedures outside the market system, entirely or in part. It was estimated that "during the first six months of the fiscal year 1961 . . . about 40 per cent of our procurement dollars are placed under some form of competition, while 60 per cent are uncompetitive."[7] Admittedly, too, "the non-market character of the relationship between the government and its weapons vendors implies in many ways the arsenalizing or socialization of private firms."[8]

Tax Policies. Running parallel, of course, with expenditure and procurement policies are tax policies. A full appraisal of fiscal policies cannot be made here; only a few general statements are possible. Those who believe strongly that the expenditure policies of the federal government have nurtured unnecessary concentration of economic power also feel that the impacts of tax policies in the main have been in the same direction.[9]

We can all agree that tax and expenditure amounts so substantial, absolutely and relatively, are powerful both as general and particular forces in our economy. Presumably, even with the best of intentions and sets of principles, the hand of the tax collector could not be laid upon all with full equity or with competitive neutrality under present circumstances. Considerations of equity and neutrality, however, have been forsaken increasingly for differential tax treatment. Consequently, tax policy merges into subsidy and aid programs through the use of special depletion allowances, and special accelerated amortization and depreciation rates, and other forms of differential treatment. Such subventions also tend to be rationalized on a case-by-case basis instead of being brought to the touchstone of general principle or of effects upon market competition in general.

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[6] *Economic Aspects of Military Procurement and Supply.* (Report of the Subcommittee on Defense Procurement to the Joint Economic Committee, G.P.O., Oct., 1960, p. viii.

[7] Hearings Before the Subcommittee in Defense Procurement of the Joint Economic Committee, 87th Cong. (June 12, 1961), p. 6.

[8] M. J. Peck and F. M. Scherer, *The Weapons Acquisition Process: An Economic Analysis* (Harvard Univ. Press, 1962), p. 583.

[9] W. Adams and H. M. Gray, *Monopoly in America: the Government as Promoter* (Macmillan, 1955).

Private Enterprise. Paralleling and abetting the accumulative drift away from the competitive market system arising out of public policies and programs have been equivalent, although different, influences springing out of private enterprise. Private enterprises often have been able to soften, deflect, mitigate, or assume command over the forces of market competition to their own individual advantages. The mere mention of concentrated excessive market power, giantism and bigness, of oligopoly, and of product differentiation is in itself sufficient to suggest both the nature of the private forces and tendencies and the types of issues discussed widely in economic analysis.

B. *Important Counter Influences*

But there are important counter influences to our accumulative drift away from a pro-market competition policy.

Internal Free Trade. We have succeeded in maintaining free trade internally in the United States in spite of efforts to the contrary by state and local governmental bodies and private actions. Among the more optimistic developments in our times were the spontaneous and widespread outcries, beginning in the late thirties and continuing more recently, against the growing interstate trade barriers. These outcries appeared to check a rising tendency to use local tax and licensing procedures in a discriminatory manner to restrict the entry of enterprises from other areas or of new types. This is not to say that interstate trade flows are entirely "free" and without any perversion. But it is to say that some major open threats to free movement and access have been checked. In the background, of course, always lurk powerful forces of geographical price discrimination, freight rate distortion, and private authority over prices in marketing channels under fair-trade laws, milk control laws, and marketing orders, as well as some amount of local favoritism in licensing.

Foreign Trade. It is possible, too, to sound an optimistic note with respect to our federal policies in external trade, in spite of the continuing troublesome tariff and quota restrictions. The general direction of tariff movements since the disastrously high level of the Hawley-Smoot Act of 1930 has been downward beginning with the Reciprocal Trade

Agreement Act of 1934, even though (1) progress has often been painfully slow and encumbered by protective clauses and (2) almost any decision can be reached under the elastic criteria employed in recommending tariff rates and adjustments with respect to the some 5,000 separately defined commodities, and (3) outsiders, especially Europeans in the European Economic Community, have had difficulty in relating our liberal world trade professions to our illiberal "Buy American" actions. But we are continuing to move towards greater freedom in international trade, including especially the Trade Expansion Act of 1962. The foundation has been laid for working out relations with the European Common Market in terms of the basic directions of our policy since 1934. We should be able to look forward, hopefully, to linking our internal free-trade markets into freer world markets.

Antitrust Legislation and Enforcement. In spite of confusions and inconsistencies and jurisdictional difficulties in our long record of antitrust enforcement and in the perspective of wide differences of opinion as to efficacy to this point in our history, it is my considered opinion that antitrust is our strongest and most consistent governmental means of maintaining market competition. Without federal antitrust activity, our internal free-trade market might very well have been completely subverted. Antitrust, along with internal free trade, has been one of our truly unique contributions to the world, especially through our per se prohibitions of open cartel-like behavior.

Much of the impact of antitrust enforcement is indirect, voluntary, and *sub rosa.* The mere fact that big corporations feel it necessary to obtain professional legal advice on all strategic aspects of business policy and practice in itself reflects the general nature of the antitrust force. The almost extraordinary nature of this force, in spite of some inconsistencies in policy and lapses and weaknesses in the use of the instrument and the small resources typically devoted to its implementation, bespeaks its inherent soundness. It voices almost alone at times the belief in the basic values of our system of free enterprise and market competition—so much so that all legislation intended to distort the free market and competition must begin with the traditional pledge of allegiance to it in the preamble.

Innovative Dynamism of Private Enterprise. But the most important counterforces to the drift away from active market competition have derived from the inherent dynamism of private enterprise under the compulsions of rapid advances in science and technology. In making this judgment, I am not unaware of the favorable environment of market expansion during the postwar period and of the contributions of governmental programs, especially in research and development.

III. *The Present Status and Outlook for the Future: Pros and Cons*

Where do we stand now and what are the prospects and probabilities of the near term and longer term future? Admittedly, there are good reasons for differences of judgment and opinion even among qualified, objective observers.

Many observers have concluded that the accumulative forces away from an effectively competitive market system are in command and are irreversible. Some have already given up and even look forward hopefully to the increasing central control or to nationalization. Others prefer to live in a world of monopolies, in which government and business co-operate closely. From these particular points of view, the pathway of social progress must lead away from impersonal market allocation and discipline. The market would remain merely as a co-ordinating mechanism when other means were too cumbersome or uneconomical.

Others—and I suspect that these are by far the more numerous group—have accepted this conclusion of irreversibility only regretfully and in a combined spirit of deep pessimism and cynicism. They envisage and predict a gradual, piecemeal movement ending in the emasculation of the traditional competitive market system, through the creation in peacetimes of a federal bureaucratic system of direct regulation comparable to that of wartimes. This regretful conclusion is reached not only out of judgment concerning the broad drift of economic and political forces but also by consideration of numerous basic alleged inconsistencies in our present situation and the numerous cross-currents and confusions in public policies in general.

A proponent of an effectively competitive market system can readily become cynical and depressed by (1) the current inconsistencies and the confusion in governmental policies and actions and (2) the apparent lack of understanding of what is involved and is at stake on the part of many private enterprises. Even within the organic framework of antitrust legislation, some confusion and inconsistencies exist as between the major statutes and in their overlapping enforcement and in judicial interpretation.[10]

The attitudes and beliefs of the pessimists also incorporate the view that we have developed and will increasingly continue to foster such a marked contradiction between acceptable behavior in selling (product) and buying (factor) markets as to make the market system increasingly unworkable. Presently we expect and try to enforce effective market competition primarily in product markets. Increasingly, wage, raw material, energy, freight, and other costs are outside the bargain

[10] For an excellent recent review, see Mark S. Massel, *Competition and Monopoly: Legal and Economic Issues* (Brookings Inst., 1962).

entirely or in large part. And when subject to negotiation, as through collective bargaining, the discussion of factor prices must explicitly or implicitly involve consideration of product prices, and vice versa. Consequently, either the per se prohibition of price agreements in antitrust enforcement must be violated or winked at or price increases to consumers and inflationary pressures negotiated and accepted. In any event, a very basic issue is raised concerning the character of acceptable competitive price behavior in product markets with rigid or semirigid factor prices determined independently or by collective agreements and often supported by long-term contractual arrangements with suppliers and customers.

The pessimism as to the future derives also from the belief that many—perhaps the bulk of private enterprises in reality—may prefer the government as customer and regulator to the rigors and discipline of the competitive market system.

The more optimistic observers do not believe that all is well, but that an acceptably effective market system can be maintained if we capitalize upon (1) the basic elements of strength in the historical counterforces to the drift away from competition and (2) our inherent, innovative dynamism. In our present situation, it is believed that internal free trade supported increasingly by freer world trade provides the setting for making antitrust enforcement acceptably effective. It is believed that antitrust legislation and its judicial interpretation have provided a body of doctrine adequate for effective enforcement, provided we really have the will to do so and provide adequate resources for such action. It is noted, to begin with, that the so-called "per se prohibitions" have kept our economy free of open cartels, except through special statutory exemptions. And even more important, we have developed the legal power and occasionally the will to make structural changes in order to maintain and enhance the role of market competition. It now appears, especially since the unanimous Supreme Court decision in the Brown Shoe case,[11] that the 1950 antimerger law may become the most powerful instrument in our entire history of enforcement so far as future market concentration is concerned. Enforcement agencies are now powerfully equipped with the combined weapons of (1) per se prohibitions, (2) ability to deal with predaceous, unfair, and abusive conduct, (3) the possibility of structural adjustments when needed, and (4) checks upon further increases in the concentration of market power through acquisitions.

Furthermore, the competitive market system is powerfully undergirded by the enormous and still evolving body of law supporting individual transactions, especially under the law of contracts.

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[11] *Brown Shoe Co.* v. *United States*, 370 U. S. 244 (1962).

And, finally, the enormous, innovative forces of modern science and technology will not be denied if they are unleashed in an environment of free trade geographically and an acceptably free and open competitive market system. These competitive pressures are counted upon both to check or offset the rigidities in factor markets and the restraints over competition exercised by private enterprise, including even the powerful giants, through interindustry competition and product line diversification.[12]

IV. *Can We Maintain an Effectively Competitive Market System*

The answer is clearly "No" in our democratic, federal system if we expect full consistency and perfection in the allocation and use of resources. The norms derived from the static economic analysis of perfect and pure competition have placed too heavy a burden upon the actual market system as opposed to the ideal, theoretical one. In actual markets one is concerned with more or less competition, not with an abstract, absolute force. Our more realistic models in economic analysis have begun to portray some, but far from all, of the variability of competitive pressures under diverse market structures and circumstances and at different market levels and stages. Actual competition is always under some amount of mitigation, distortion, and constraint from environmental, governmental, or private sources. Furthermore, in our democratic society, time and again, we will not tolerate market competition so effective as to put continuing severe downward pressure on product and factor prices, especially when numbers of enterprises and of people are large (as in agriculture and small service trades and labor in general).

In the next several years, government and private enterprise should, and in fact must, face up to some crucial decisions requiring heroic measures and high intelligence if we wish to hold the present status or begin to reverse the trend away from the competitive market system. Agreement and action are made difficult by the shifting, multiple,[13] and hierarchical character of goals on both sides. The steel episode of last spring dramatically demonstrated the difficulties, especially when bigness and oligopoly are located so strategically in the economy that private decisions clash head on with current public objectives of high immediate priority.

On the government side, the total framework of goals and of policies and their implementation, short run and longer run, needs to be examined in terms of the requirements of an effectively competitive market system and the maintenance of viable private enterprise. The results of such an over-all view (as by the Joint Economic Committee and the Council of Economic Advisers), if implemented, would not produce full consistency. But it could provide sufficiently greater consistency to reverse the trend away from the competitive market system; especially if we (1) stop adding to the partial exemptions from the antitrust laws, (2) introduce more competition into the regulated sectors when it is "a genuine possibility,"[14] (3) try to keep governmental procurement programs, including weapons, missile and space systems, within the competitive framework of the market insofar as is possible, (4) try to restrict the number and impact of subsidies and subsidy-like differential programs to nonmarket areas, except to correct maladjustments in access to resources and market opportunities or as parts of a well-considered program of economic growth and stability.

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[12] Some leading scholars part company at this point and believe that these results are not obtainable without fundamental revision of the antitrust laws implemented through a special court with expertise. See C. Kaysen and D. F. Turner, *Antitrust Policy, an Economic and Legal Analysis* (Harvard Univ. Press, 1959).

[13] See Ben W. Lewis, "Economics by Admonition," *A.E.R.*, May, 1959, p. 396.

Private enterprise, on its part also, must be prepared to face up to the implications and consequences of persistent noncompetitive behavior in pricing and in the allocation of resources when this occurs. A hard choice at times may have to be made between (1) structural adjustments to guarantee acceptably effective competitive behavior and (2) public utility or quasi-public utility status. In some cases, the latter may be preferred by private enterprise to more effective market discipline, since it would represent less of a departure from established practice. From a community standpoint, however, it may be preferable usually to hold the existing nonpublic utility status with the hope and expectation that the competitive forces will in time become more effective in our dynamic economy or can be made so by structural adjustments, especially if we do succeed in reversing the trend away from effective market competition.

On both sides there is a need for further advancements in economic and managerial analysis more directly relevant to real-life actualities and potentialities and public responsibilities. The driving, dynamic forces of technological change play too remotely outside our formal models, as does much of the institutional and market setting. But we are learning to build models for complex, multiproduct enterprises that relate external and structural constraints to the internal goals and criteria of managerial decision making. Gradually, we are developing a form of operational analysis more appropriate to specific managerial problems and decisions. We are approaching a state of knowledge and of conceptualization that should make it unnecessary to generalize either so freely and loosely or so narrowly and unrealistically concerning bigness and oligopoly. We shall have begun to arrive when we find ways of bringing the dynamic processes of actual competition into our analysis. We will have arrived when we are able to relate the negative prohibitions of antitrust enforcement intended to free markets from unreasonable restraints to the more positive responsibilities placed on the federal government under the Employment Act of 1946.

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[14] G. E. Hale and R. D. Hale, "Competition or Control VI: Application of Antitrust laws to Regulated Industries," *Univ. of Pennsylvania Law Rev.*, Nov., 1962, p. 55.

The current problems with respect to agriculture and steel in the perspective of the emerging issues under the European Economic Community highlight both the analytical difficulties as well as the urgencies for immediate and considered action, especially by the federal government. Europe is moving rapidly in the effectuation of internal free trade. Furthermore, the carefully worded antitrust provisions of the Rome Treaty were implemented by the initial enforcement regulations in February, 1962. The cynics are calling all of this verbiage that will never be implemented by effective enforcement. But the mere presence of such provisions and regulations and of a central enforcement authority have tremendous portent. There were cynics, too, in this country in the nineties who held that the Sherman Act was a meaningless, futile gesture.

A misstep by us could now have tremendous consequences both in example before the world and in the outcome of the tough negotiations ahead. We need to set our house in better order so that our example is as consistent as is reasonable in a pluralistic, democratic society. Hopefully, the outcome then will be not a deadly power struggle between the United States and the European Economic Community but the reinforcement of the competitive market system in both areas.

AMBIVALENCE IN PUBLIC POLICY TOWARD REGULATED INDUSTRIES

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The recent unpleasantness over steel prices has been vigorously and variously interpreted. To some, it was a dramatic but casual incident; others saw the President's action more apoplectically, as a crude assault upon our way of life or as a magnificent defense in the face of an insolent challenge. I suggest that something more and different was involved.

The steel affair did not "just happen"; it was long in the making. It might have erupted a little earlier or a little later, but it was bound to erupt. It was essentially an economizing phenomenon, and it emerged from the working of basic political-economic forces. Its occurrence, hard upon the heels of the publication by the Council of Economic Advisers of its "guideposts" for wage objectives and price decisions in the somewhat freer sectors of our free economy, marks a distinct milestone in our political-economic life. It beckons us to an interesting and important exercise in applied political economy.

Repeatedly since World War II, the White House has urged leaders of labor and industry to be "responsible" in their decisions and demands. The wage and price guideposts cautiously advanced by the Council in its Annual Report of January, 1962, confirmed explicitly the authoritative belief implicit in these admonitions that the mechanism of the free market has come unstuck—that there are important sectors of the economy where there is, "at least in the short run, . . . considerable room for the exercise of private power and a parallel need for the assumption of private responsibility." The Council staked out in terms relevant to economic decision making its own view of the overall limits and broad contours of responsibility.

The Council's venture was modest: it marked boundaries; it did not prescribe decisions. It recognized the possibility of exceptions; it invited discussion. In effect, it also invited departures. It was to be expected that in due course the guidelines would be transgressed, and that, since no sanction other than public opinion was contemplated, the deviant industry would carry its case to the public. Thereafter, the industry's representations would be weighed by an informed public against the general criteria furnished by the Council, in an atmosphere conducive to the responsible exercise of power. As an economizing device, the arrangement was on the loose side, but it was tighter at least than admonitions alone, and we awaited a test with interest, if not with complete confidence.

The test exploded! The Council's formulae proved spacious enough to accommodate the wage settlement in steel, but in the matter of prices, Big Steel chose not to fool around with silly guideposts. It advanced prices and set itself for the whirlwind. Just for the record, what it reaped was no whirlwind! When the industry picked itself up, there was no price advance to debate. Big Steel was back on the reservation, and something other than guideposts had been put to a test.

That something, incidentally, was not market competition. This claim has been made, of course, to the surprise of no one, but it takes a really fanatical dedication to the cause of competition to characterize the industry's staggering and rolling rebound from its price sally as a normal response to a sagging market. Steel was clobbered! The only economizing instrument that was tested in the steel case was Presidential temper, and while no one can doubt that it brought spectacular results, few will contend that unrestrained Presidential temper should be installed as a standard unit in our regular economizing machinery.

Since the capitulation of Big Steel, the President has affirmed almost daily the determination of the Administration to refrain from direct control of prices and wage—provided, of course, that the private decisions reflect a sense of public responsibility and a proper concern for the public interest. Most of us support both the affirmation and the proviso. Few of us can look with a light heart upon the prospect of widely extended direct government controls over the economy. Unfortunately for our peace of mind, however, we find no great solace when, in important sectors of the economy, we look upon the actual performance of the free market as our economizing mechanism. It is this prospect that has driven us, still shrieking our undying faith in free enterprise and the market, into explicit avowal of collective concern and explicit acceptance of ultimate government responsibility for "responsible" decisions on prices and wages. We are slowly becoming aware that economizing is society's business.

We need not be dramatic about this, but neither may we draw the shades and pretend that nothing is happening. There is a limit to even the most compelling self-deception. Let's face it: the government of the United States and the government of our fifty states, representing us as a people, are in the economizing business in some fashion and some degree, involving us in something from surveillance to enterprise, and deeply in results if not always deeply in operations, straight across the board. Let there be no mistake, we are in, and we will be further in—and for keeps.

The problem presented to us by steel is one in a vast array of economizing problems. On the surface and immediately, we are concerned about price and wages; more deeply, we ask: "How shall we determine our wishes—the wishes of society—in the matter of resource use and how shall we translate these wishes into action—by sweet persuasion, by awesome threats, by enticing inducements, by the compassionless compulsions of the market bolstered by a credulous antitrust or by more consciously collective political measures such as legislative enactments, administrative orders and public enterprise?" We employ all of these devices, singly and in combination; our mixed economy is a hodgepodge of arrangements and contraptions calculated to work our social will through and upon each other. We like all of them—selectively employed, of course, and with appropriate exceptions. Whether or not our attitude, over-all, is ambivalent, it certainly provides a soil for the breeding of "ambivali."

In this session, we are taking stock of some of these economizing measures. Of them all, none is more intriguing, in itself, in its relation to the total economy and in its bearing on the business of economizing than the bundle of statutory, administrative, and judicial controls over entry, service, and prices with which, as public utility regulation, we crown specially selected privately-owned industries. There is something quite special about government regulation of the public utility type: this is the way we behave when we are really keyed up about economizing, when we stop acquiescing and "going along," when we feel quite certain that, for reasons we can identify, the processes of the free market cannot be made satisfactorily to perform the economizing job we want done and, hence, that we must perform the economizing functions by specifically designed laws, agencies, and measures.

We have had a good deal of experience in this business—we have been in it, man and boy, for over three-quarters of a century—and have kept a full record of our efforts. No one will pretend that our performance has been brilliantly successful; indeed, our impression of public utility regulation is such that whenever a proposal is made that it be extended beyond its traditional boundaries the almost universal response is, "No, no, not that!" Yet, we have rarely if ever retreated from formal regulation once undertaken, and the course of regulation in the case of individual industries has in general been one of progressive intensification rather than relaxation. We have twisted and turned, and protested and groaned—and turned the screws tighter and tighter. We are not really fond of regulation, but we like it well enough to live with it.

In the case of the so-called "local" public utilities (gas, electric power, telephones, water), we have been convinced virtually from their beginnings that effective point-by-point competition between sellers of the same service is simply not possible, and that even if it were to be attempted, it would fail and in due course disappear. We have insisted on local monopolies under public regulation. There has never been a serious move to supplant regulated monopolies by unregulated competing private companies in these industries. Early regulation by municipal councils through franchises and ordinances has given way, largely although not completely, to regulation by state statutes and by state-wide administrative commissions; and since the thirties state regulation has been supplemented, in the case of interstate commerce in gas, electric power, and telephone service, by federal regulation.

Broadly, but with many exceptions and with varying emphases, public regulation is charged with control over rate levels and structures and the amount and quality of service and over other operating activities which bear on rates and service. The objectives of regulation are deceptively simple: adequate service at reasonable rates. A slightly fuller statement might read: rates which do not discriminate unreasonably between different classes of service and which in the aggregate and under honest and competent management will produce earnings sufficint to cover all reasonable costs and attract enough investment to provide as much and as good service as we want and are willing to pay for.

The history of public utility regulation has been characterized by controversy between regulators and regulated, conflicts between regulatory agencies, ambivalence, gaps and continuous "catching up." Time and space (those grim guardians of the listening and reading public) will not permit me to elaborate this history, but a brief survey may help us in our examination of public regulation as an economizing instrument. Legislatures and commissions have fought a running battle with the Supreme Court throughout the century over the constitutional protections thrown about the regulated industries, particularly by the Fourteenth and Fifth ("Due Process") Amendments. In recent years the Court has materially restrained its earlier proclivity to dictate regulatory policy, but the battle has been taken up by many state courts and fought with as much gusto as that displayed by the Supreme Court in the twenties—the heyday of its conviction that the Court alone knew how to govern.

From 1898 to 1945, the policy and practice of rate regulation were dictated by a very confused Supreme Court, and regulation as an economizing instrument is still paying the price. Regulatory ambivalence in its most pristine state—sheer uncorrupted two-headedness—

was given birth in the pronouncement by the Supreme Court in *Smyth* v. *Ames* (1898) that, to be nonconfiscatory under the Fourteenth Amendment, regulated rates must be calculated to provide a "fair return" on the "fair value" of the regulated property. The Court did not define fair value: after naming a number of possible components (varying in the degree of their respective irrelevancy, and mostly mutually exclusive) and stating that others, still to be dreamed up, might also be introduced, the Court concluded that fair value was to be found by the employment of "reasonable judgment" after giving "due consideration" to the whole indigestible lot. The fair value rule still dominates rate regulation and sets the tone of regulatory proceedings in general; it has made a shambles out of what could have been, and still could be, a straightforward, economical, functional exercise requiring no great outlay in time, nervous energy, or expense.

It has regularly been stated and never seriously denied that fair value is quite different from and is not to be measured by "market value," but in any event, the atmosphere in which fair value has been found for over half a century has been rank with the odor of market value, and the business of rate making has been expanded into an extravagant, mystical ceremony—under dim lights. Protection of the investor has, quite naturally, been the dominant note in the constitutional chant. Protection of the utility owner is by no means inharmonious with, indeed it is an essential feature of, functional rate making designed to attract and hold necessary capital in the industry, but the pitch and quality of the note that has been struck have produced sixty years of discord and dissonance.

With the rise in the price level during and following our two World Wars, great stress has been laid by utility attorneys and their retinues of financial, accounting, and engineering experts upon revaluation of actual dollar investment in calculating rate bases. Here, again, huge sums and endless months have been squandered to obscure the simple propositions that if the public is to look to the market for its utility service, it must expect to pay in rates enough—and no more than enough—to cover the costs of the factors and elements (including capital and enterprise) which necessarily go into the provision of as much and as good utility service as it wants; and that such costs, within quite workable limits, are capable of easy and inexpensive ascertainment. The problem which has disturbed the sincere and more perceptive proponents of the reproduction cost approach to rate regulation is a very real problem; unfortunately it has largely been lost in the ponderous machinery which, quite unnecessarily, has been constructed to fabricate its solution.

We are no longer required by the Supreme Court to punish ourselves with the rate-making extravaganzas of yesteryear. With the announcement of the "end-result" rule in the Hope case (1944), the Court opened the door at last to the use of a simple approach to what, after all, is a relatively simple problem. Unfortunately the door had been closed too long, and large numbers of the Court's disciples in state commissions and courts are, mole-like, still pushing around busily in the dark.

Over the years, regulation of the standard public utilities has encountered many other road blocks and vicissitudes. For a period the Supreme Court, moving behind the prepossessing but wholly unrealistic proposition that "regulation is not management," interposed a constitutional objection to any effective control—through disallowance in rate proceedings—of excessive operating expenditures by public utilities. In due course, the Court retreated from its earlier, hastily assumed position, under circumstances which may have given firmer support to regulation than if the Court had been right-minded from the beginning. Commissions and courts have regularly looked in two directions in their treatment of taxes, and in several directions, to the detriment of effective regulation, in dealing with depreciation. The failure of Congress to act in the face of growing interstate movements of gas, electric power, and telephone communication left an imposing gap for many years in the over-all structure of regulation. Congress acted in the thirties to close the gap, and an extended period of statutory construction and constitutional interpretation ensued. From an early dearth of script and actors, we moved overnight to an excess of both, and it fell to the Supreme Court to sort out the lines and assign the parts. An amusing feature of the shuffling of regulatory powers between the states and the federal government during this period were the efforts of utilities, in the name of high constitutional principle, to seek shelter under what they regarded for the moment as the least oppressive roof—to their later distress in instances where the regulatory climate they had sought became less congenial. The record will show, I believe, that the Court carried out its casting duties deftly and with good effect.

At the moment, regulation is engaged in a rate-tax hassle growing out of the efforts of the federal government to stimulate investment generally in industry by providing special tax allowances for accelerated depreciation. Public utilities, quite naturally, have insisted that regulation must give them the full benefit of this program. Some regulators and courts have gone along; others have dug in their heels. The arguments have been involved and tedious, the technical "evidence" abstruse and deadly, the busy-work most impressive. Here is regulation at its jolliest and silliest. It would be a shame for the fun to come to an end, but before too long it should be brought to the attention of someone in charge that it is slightly incongruous for a remote agency of government to extend, quite fortuitously, to the utility industry an additional income incentive when the industry's income is already being calculated by its very own special government agency to stimulate as much investment as the public wants and is willing to pay for. In the case of handouts, it would make sense for one hand to know what the other hand is handing out.

Let us turn briefly to certain native costs and certain inherent weaknesses in the regulatory process which have made themselves apparent over the years. Regulation costs something in resources; and as it grows more effective, both the open and the hidden costs go up. Public regulation requires regulators and regulations, and as it moves in to fill gaps and to improve its processes it inevitably piles more regulators and more involved regulations upon operators and operations. To the explicit cost of duplication must be added the possible dampening effect upon the spirit of utility operators. Incidentally, it is worth speculating whether regulation has in fact given a new dimension to utility entrepreneurship—whether in many situations there is not more to be gained by outsmarting regulation than by reducing costs and extending markets.

As an economizing instrument, it must be conceded that regulation in practice, like competition in practice, falls short of perfection. Regulatory standards are static, not dynamic; they furnish regulators with no precise continuing measure of "enough" service (hence, "enough" resources). To protect investors, regulation aims to provide a fair, "going" rate of return on a base figure representing existing property: if the return is higher, rates are cut; if lower, rates are raised. In both cases, rates are modified directly without any intermediate change in supply. Excessive or deficient earnings are taken, not as an indication of insufficient or excessive investment, but of defective regulation. Existing investment tends, thus, to validate itself. Regulation pins its judgment on the need for additional investment in specific cases largely on physical criteria. And in calculating profit as a cost (as it must), regulation frequently confuses its specific regulatory *ex antes* with average competitive *ex posts*.

Nor is regulation able, in the face of constitutional protection of property, to move positively to require utilities to measure up to such standards of performance and investment as it has been able to erect. Regulation, under the Constitution, is a great protector: it protects helpless consumers from grasping owners; it also protects lethargic owners from demanding consumers. It does not, by itself, possess the positive, ruthless, driving force which our mythology attaches to competition to compel the best (as distinct from the merely acceptable) in operating efficiency and quantity and quality of service. Herein lies the case for strategically located publicly-owned public utilities (e.g., TVA, REA, and some of our better municipal plants), not as direct competitors or even as "yardsticks," but as threats to complacency—"it could happen to me!"

By and large, public utility regulation arouses protests in about equal volume from owners and consumers; and scholars, of course, are critical as is their role and wont. It is perhaps fair to observe that day in and day out we enjoy quite a lot of pretty good utility service, that we are not conscious of being badly gouged, and that the stock market and the flow of voluntary investment into the industry attest that utility owners are doing rather nicely. If it be argued that regulation is being "carried" by growing demand and advancing technology, it can be argued with equal force that if the public wants the service and if the industry chooses to be alert, the burden of regulation is not intolerable; and surely it is not the obligation of regulation to provide markets and enterprise.

Public utility regulation is demonstrably workable if we really want to make it work. Its most conspicuous shortcoming consists in the confusions with which we have endowed its rationalizations and the unnecessary costs we have heaped upon its processes. We can have better regulation whenever we are ready to recall that regulating is economizing and, in consequence, are prepared to fashion the thrust and processes of regulation to its simple functional role and to rid it of its grandiloquence and its ridiculous trappings.

There are those who believe that in the case of one utility among our select group, natural gas, regulation is overextended. Sales of gas at retail have, of course, been subject to state and local rate control for decades; the Natural Gas Act of 1938 added regulation by the Federal Power Commission over sales for resale by interstate pipeline companies. The same statute also provided FPC regulation of interstate sales of natural gas at the wellhead, but it was not until 1954, and over repeated frantic protests by the Commission, that the Supreme Court in the celebrated Phillips case, finally forced the Commission to accept jurisdiction. The Commission was not happy at the prospect; it has been miserable over the event. Massive efforts by the industry to secure the enactment of legislation to relieve the Commission and restore wellhead pricing to the mercies of the market have so far been unavailing; and the offensive has more recently been directed toward placing the stamp of FPC regulation upon variations of prices reached in the market by negotiations between willing sellers and not unwilling buyers. Patently, in this area, calculated ambivalence is of the essence.

We cannot fight the battle here and probably it is just as well: no one would win. The fact is that in the case of natural gas field pricing we have a bear by the tail. The commodity and the industry are characterized by demand, cost, and supply features, complicated by long-standing practices and institutional arrangements, which cannot be trusted to produce acceptable market prices and which no really satisfactory pattern of public price regulation can be made to fit. Both the market and public regulation are possessed of near-fatal weaknesses; the most plausible case for either is to be made by attacking the other. The Federal Power Commission, after years of agonized soul searching, and contrary to the recommendation of its examiner, has announced an area-pricing approach the principal merit of which seems to lie in the inadequacies, recounted at length by the Commission, of the traditional public utility rate base procedure. The Supreme Court has promised to pass on the Commission's ruling.

Natural gas and we can, of course, live with either market control or public regulation, but with neither will we live fully and happily. Indeed, the prospect of life with either might someday become so unbearable that we will decide to back off, take a fresh over-all look, and, just possibly, undertake an institutional breakthrough. We might come to believe that, to date, we have been principally engaged in trapping ourselves. A long, hard look at the joint-product, competitive link-up between natural gas and petroleum, and the realization that economizing—the rational use—of these limited stock, exhaustible, nonreproducible commodities is really the prime problem, might persuade us in time to attack the problem directly rather than to push it out of the way in our troubled concern over whether to use public regulation or depend upon the market to determine prices. None of this will happen, but we can dream, can't we?

Without moving from the natural gas industry, this time the interstate pipeline sector, we may make contact with still another policy entanglement involving a number of overregulated and semiregulated industries—the running fight between regulation and antitrust over mergers. To take one current example, a staccato account of the five-year, still surging conflict between the Antitrust Division and the Federal Power Commission over the proposed merger of the El Paso Natural Gas Company and the Pacific Northwest Pipeline Corporation would fail completely to do justice to the grand plans and the magnificent maneuverings of the participants; an adequate recital would swamp this paper.

The tugging and hauling and the tortuous path traced by the proceedings in this case reflect something less than a clear purpose and a sure hand in our continuing attempts to structure our economizing efforts. The doctrine of "primary jurisdiction" (who gets first crack?) is the immediate technical legal issue, and one gathers that, as of the moment, the tide of battle on this front is running in favor of Antitrust. So be it. The substantive economizing issue relates to the role of competition in the regulated industries, and more specifically whether in the regulated natural gas pipeline industry our public policy is committed to an antitrust approach to mergers as an absolute value, or whether any adverse effect of mergers on competition is only one factor among many to be weighed by regulation in molding the structure and organization of the industry in the public interest. The policy decision rests with Congress (always, of course, as the decision is construed by the Supreme Court). Congress, presumably, has spoken on the matter in the Natural Gas Act and the Clayton Act, but the message that has come through to the Antitrust Division is quite different from the word that has reached the Federal Power Commission. One can speculate whether Congress was looking both ways at the same time without being aware of the fact (elemental ambivalence) or was knowingly proceeding simultaneously in opposite directions and counting on the Supreme Court, assisted by the Antitrust Division and the FPC, to consolidate its course (provisional ambivalence); or whether the clear single purpose of Congress was somehow lost in the telling.

The same kind of problem is present in the case of other regulated industries—railroads, motor transport, airlines, communications, electric power, banking—with reference to some of which Congress has spoken more explicitly. In all cases, however, there is, necessarily, room for statutory construction. Whenever the Congressional directive employs such limits as "the public interest" (whether the interest is to be "promoted" or just "not injured"), only the Supreme Court can say with finality whether or not the administrative agency has strayed beyond the statutory limit. To advert momentarily to the process of review, it is my own judgment that in cases of this sort great weight should be given by the reviewing court to the findings of administrative commissions. I hold no brief for the brilliant expertise of these bodies. I am quite aware of their imperfections, and I am greatly impressed by the case made by Louis B. Schwartz, in his January, 1954, article, "Legal Restriction of Competition in the Regulated Industries," in the *Harvard Law Review,* pages 436-75, against what he chooses to term "judicial abdication." I remain convinced, however, that where we have been moved by forces and events to remove an industry far enough from the control of the market to warrant its subjection to special regulatory laws and to oversight by a special regulatory tribunal, it does not make sense in the subsequent refinement and application of policy to proceed as though the tribunal were, by hypothesis, corrupt, careless, or, at best, incompetent. If we are going to set up shop, we had better put someone in charge of the store. And I am reminded again that public utility regulation is still walking skewed from the heavy hand laid upon it in its formative years by the courts.

Back on the substantive policy issue, I suggest that in the regulated industries, Clayton Act standards on mergers have relevance, but that they should not be decisive or, in the usual case, even presumptive in determining whether mergers should be permitted or encouraged. To many informed people whose judgment I respect, but whose values I do not share *in toto,* competition, as an expression of human freedom, is an end in itself, as well as a means. I tend to be a means man, myself, but, in any event, in the case of regulated industries we have, by the very institution of regulation, given up on competition as an end, and we have retained it as a means, if at all, only in a supplementary role. To insist, then, that any merger of regulated firms that may substantially lessen competition in any line of commerce in any section of the country must be prohibited is really to embrace ambivalence as a way of life. We institute regulation because in our best judgment the continued presence of effective, economizing competition cannot be counted on. If we are tempted, nonetheless, to retain as much competition as we can, for whatever slight regulatory function it may be able to serve, we will do well to weigh (1) the opportunity cost of supplementary competition—the benefits to be expected from the merger which will be sacrificed if competition (or its semblance) is preserved—and (2) the need for the supplementary competition in an industry already provided with public regulation.

The competition we are talking about is, by our own decision, unreliable at best; and the appearance of competition as an economizing force is quite likely to be illusory. The physical presence of "competing" natural gas pipelines may well signify that competition in a section of the country is a thing of the past—that major long-term contracts have been executed, certification has been granted, pipelines have been constructed to carry out the contracts, and competition has ceased. Consider the unique character of "competition" in banking (where Antitrust is presently taking on all comers): distinctive, partially overlapping markets in which it is just as "natural" for competition (in any degree) to be absent as to be present, a "doctor-patient" relationship between lender and borrower in a generally aseptic atmosphere, a universal dread of competition being carried in any case to its logical conclusion (the failure of any competitor), and a public policy against the unrestricted admission of "competitors" into the business in the first place. In any of these situations, more than the physical presence of two firms is required to bring about a state of competition, and a merger which reduces the number of firms does not lessen competition where no genuine competition existed before. Two horses do not make a horse race unless both horses want to run, hard.

It is possible, intellectually, to find a role for any competition that may chance to rise to the surface in such industries as these—natural gas pipelines, banks, railroads, for example—but if mergers have anything to offer in the way of improved service or lowered costs that can be passed on to consumers—and there is much evidence that they do—and if public regulation or substitute services or both are present, surely it is no part of a purposive program of public economizing to insist on antitrust standards in dealing with intra-industry merger proposals. In any fluid situation mergers should be required to justify themselves positively, not simply to establish that they are not inimical; and antitrust specialists in the Department of Justice should be encouraged to present all the adverse evidence they can muster. But, in the business of economizing, we must be guided in our decisions by net effects, not by absolutes that are not there. As a postscript: to use the antitrust members of our economizing personnel in the capacity I have just suggested strikes me as a much more rewarding deployment of scarce, valuable resources than to use them, as now, as sporadic sharpshooters in an internecine struggle with public regulatory commissions. Antitrust has won its share of the engagements; in the meantime a war that needs to be won is not going too well.

To wind up our survey, admittedly quite incomplete, of public economizing with special attention to our inclination toward double vision in the matter of regulation and competition, let us take the real plunge—into the wilderness of transportation. We will not transform the wilderness at this time; indeed we will be fortunate to emerge alive. One's mind goes back longingly to the good old days when public transportation meant railroads, and railroads meant "natural monopoly," and when, beginning where the centuries-old common law control of common carriers left off, we could tailor regulation with confidence to the clearly visible configuration and easily ascertainable measurements of the industry. Our efforts were not wholly successful, but we had our tapeline, and our needle, and lots of assurance. We really had it made in 1920. Then came the motor transport industry, and now we can't even find a place to park!

Transportation presents us broadside with the full range of economizing issues and details, hopelessly intertwined. To the extent that our public transportation policies are ambivalent, it is fair to say that we have not achieved ambivalence; it has been thrust upon us by an industry which, without warning, took off spectacularly in a new direction, leaving us with a bag of old patterns and old tools. Our approach and policies are really not so much ambivalent as desperately confused or, more generously, experimental. We are quite aware of our inconsistencies, but it will take a little while to sort and straighten them out. The sheer fact is that none (repeat none) of us—legislators, commissioners, judges, lawyers, engineers, economists, or old transport hands—knows what to do. All of us can spot conspicuous oversights and blunders in existing policies (after the event), and all of us are prepared to offer advice in broad, general terms and even, in some instances, to prescribe for particular difficulties. The need, however, is for a clean-cut, over-all program; and only in the sense that we are twenty-five years older and have a greater awareness of the difficulties of the task can we be said to have made progress toward this goal in the past quarter century.

For years everyone has been calling for "co-ordination" in transportation—each carrier in each instance making the happy haul for which, all factors considered, it is best suited. Such contrived efforts as we have made toward co-ordination have not been distinguished, but what other than sheer panic could induce us to hand over the job completely or even largely to the uneven and shifting intramode and intermode competition which characterizes this industry? Look at it: four classes of rivals (each with a sprawling brood of subclasses) of varying ages and strengths, heavy fixed costs and light fixed costs, industry-provided facilities and publicly-provided facilities, massive do-it-yourself private participation, stubbornly sluggish costs, melodramatic finance, subsidized carriers and carriage-subsidized commodities, dynamic exotic technological developments, considerations of national defense and local pride, and widespread legitimate carrier, commodity, and community interests which are not just vested, but imbedded and impacted. To be sure, regulation has made its own unique contribution to this witch's brew. It is the thing today to attach complete blame to regulation for the present plight of the railroads—or at least to offer the failure of regulation to bring peaceful and prosperous coexistence to the railroads and their playmates on land, sea, and in the air, as proof of its blundering ineptitude. It is quite true that regulation has not yet been able to wave into existence the precise pattern of rates that will transform transportation chaos into sweet and gently moving equilibrium. But the cure is not to fire the doctor. Co-ordination in this industry calls for more, a lot more, than antitrust with a mandate to eliminate discrimination.

Our basic uncertainties and disagreements over approach and policy relate largely to degree. It would certainly be comforting if we could hand the task in its entirety over to competition—if we could turn our backs on the trick which fate has played on us, put up our feet, and doze peacefully to the tranquilizing hum of the smoothly meshing market. We cannot believe, however, that the hum will not become a banging, clanging grind, and that we will not have to take our feet down and go to work. It is equally clear to us that the elaborate, cumbersome superstructure of statutory and administrative controls that we have erected over the transportation industry through the years has long outlived any usefulness it may ever have had, and that public regulation is ripe for very considerable dismantling and rebuilding. Interrailroad competition is no more reliable as an economizing instrument than it ever was, but interindustry (rail, water, air, motor transport, and private motor transport) competition is a very lively affair. We cannot and should not ignore it: we should use it (shaping it in the process) and strengthen it. By the same token, we cannot and should not deny the need for public action more openly and deftly purposive than we have known.

There is no single solution for the transportation problem, and most certainly no continuing solution; it will abide with us always. But, we can manage better than we are presently managing. I suggest that we will manage better if we put ourselves in charge consciously and responsibly, and if we give prime attention to the co-ordination of our tools—public regulation (including control over entry and over minimum rates and positive control over mergers), subsidies, public enterprise, and the driving, compelling force of whatever competition can be purposefully drawn into the pattern; and if we set our goal, for a while at least, not as solution, but as workable containment.

Transportation, with its ever shifting pattern of market and public regulatory forces is, in a very real sense, a microcosm of the whole economy. From an explicit confrontation with the problems of economizing in the transportation industry we may learn, if we are not already aware, that public economizing is a tricky business, full of traps, unknowns and unknowables, illusions, disappointments—and great satisfactions. Economizing is unavoidable. Even acquiescence is choice, and it pays out, just as positive decisions pay out, in costs and benefits. Economizing, too, is never ending: economies are built and rebuilt, brick by brick, by the myriad choices made individually in the market and collectively through the machinery of government at all levels. Economies never arrive; they are endlessly on their way. The process, of course, is the thing—the give and take, the play of interests and of reason. In our kind of society, this, as much as the continuing outcome, is dear to us. In this process, our profession has a special stake—opportunity, responsibility, and sheer delight. This is our stamping ground. This is where we work. We have a job ahead of us.

Watchman, what of steel?

DISCUSSION

ROBERT F. LANZILLOTTI: Professor Grether has indicated the nature of the historical drift away from competitive markets and has suggested some of the broader problems resulting from this development. He attributes the accumulative drift to: transfers into the regulated sector; subsidies, special treatment, government expenditures, and tax policies. Against this he arrays the forces of internal free trade, prospects for freer foreign trade, and the antitrust laws; but it would be a gross exaggeration to conclude that he is sanguine about maintaining an effectively competitive market system with this defensive weaponry. In part, I believe this is because Professor Grether really never comes to grips with the central issue of inconsistency in public economic policy toward the unregulated sectors. He deplores the loss of private sectors to government regulation but does not make it clear that what passes for regulation in many cases is really protectionism. How one squares these cases with what Professor Grether considers the competitive system's "basic role of coordination, allocation, or resources, and private regulation"—which I take to mean the same as Professor Lewis' objective of "economizing"—is not indicated. If we take either of these as our basic policy objective, it seems absolutely necessary to indicate in what sense various public policies and practices work in harmony or perversely to the objective and whether the latter can be justified on economic grounds. Both papers reveal a touching loyalty, or peculiar nostalgia, for the goal of efficient resource allocation based on the market mechanism, but both also raise more than proportional doubts about the ability of the market to handle the complex and conflicting forces involved. Put differently, Professors Grether and Lewis ask: "Do we really want, and is it realistic to expect, a set of public economic policies and practices that are consistent with an effectively competitive market system?"

Within the confines of antitrust policy proper, one can find numerous examples of inconsistencies, both with respect to actions taken and failures to impose policy with vigor and determination. Some of us would certainly have serious doubts whether the recent decisions of the Federal Trade Commission in the American Oil, Luria Brothers, Sandura, and Sunshine Biscuit cases, for example, are consistent with the objective of competitive markets. Some would even go further and ask whether under the dictates of Robinson-Patman competitors can really "win," and if so, is it possible for them to price flexibly? Likewise, one wonders about the reported shift in policy emphasis by the Antitrust Division to "the predictable and established lines" of enforcement, and whether in the aftermath of the steel episode the selection of cases and policy emphasis will be circumscribed by larger political considerations, as has been charged in the past.

Desipte such concerns, however, no one seriously doubts that in the main these agencies, with limited staffs and budgets, are attempting to strengthen the market mechanism as our principal economizing device, even though some would prefer a line of enforcement stressing basic structural reorganization through divestitures and dissolutions to reduce concentrations in particular industries.

Timid antitrust enforcement and judicial emasculation, however, shrink by comparison with the persistent undermining by the federal administrative agencies, the Department of Defense, and the Congress. Professor Grether notes, citing the work of Walter Adams and Horace Gray, how government measures, including procurement policies and practices, with hardly a thought to the broader questions of competitive policy, have promoted concentrations of economic power. In fiscal 1961, twenty corporations accounted for nearly 75 per cent of the total research and development contracts awarded by the military. Is it not inconsistent—not to say dangerous—for the federal government to nurture such concentrations in the technically most advanced fields, which can be pre-empted by particular firms selected by military officials? As former Attorney General Brownell prophetically observed in 1956, "the disproportionate share of total industrial research and development in the largest firms may foreshadow a greater concentration of economic power in the future." If Brownell was wrong and the current R and D contract policy is intractable, then advanced research has condemned us to live in a world chosen by government officials and military officers—that brave new world former President Eisenhower calls the "military-industrial complex."

In this connection, even while the President was reaching his difficult decision to meet the dilemma in steel, the Congress was being urged to act swiftly to ensure that we would not run out of future policy dilemmas in communications. Attempts to justify the Communications Satellite bill on the grounds that the Federal Communications Commission (which has long enjoyed jurisdiction, but never regulated international carriers) will regulate satellite operations, have been aptly described as nothing short of humorous. Indeed, our suspicions of the FCC as an inhibiting factor in the innovations process of the communications industry is confirmed in a well-documented doctoral dissertation on vertical access in communications satellites by a young economist at Michigan State University. What our earlier failure to divest Western Electric from the Bell System accomplished in foreclosing on the possibility of competition in the domestic communications equipment market, the satellite bill carried forward into international communications. Here is another instance where those highly respected gales of creative destruction are placed under the control of those who would be destroyed by their blasts. It must rank high in that administrative folklore of making the fox the custodian of the henhouse. I, for one, believe we should permit such fly-by-night companies as General Electric, Lockheed, and Bendix vertical access to this "risky" new field.

Both speakers advise there is an almost inexorable drift of industrial enterprise toward quasi- or fully regulated status, but it is not clear from their papers just what a regulated industry is, what it is supposed to be, or what the conditions for candidacy are. Professor Grether states that such analysis defies quantitative portrayal, while Professor Lewis throws caution to the wind and lumps all regulated industries into one category. Neither paper has indicated, for example, whether banking—which is not without public policy dilemmas in the matter of banking structure—is a regulated or unregulated industry. In the sense that entry is limited, that banks are subject to periodic examination, certain safety requirements, and some regulation of interest rates, one could say that this is a regulated industry. But, in the main, the price and availability of credit is not subject to public regulation—although the manner in which prime rates are established among leading banks raises interesting questions about private regulation!

The need to develop some sort of consistent policy toward the increasing avalanche of bank mergers, acquisitions, and holding company banking have made the meaning of regulated or unregulated status more than an idle issue. The Congress has indicated that antitrust and the principle of amended Section 7 of the Clayton Act must be considered in resolving merger and consolidation issues. However, the Comptroller of the Currency's approval of the Philadelphia National Bank and the Girard Trust Corn Exchange (the city's second and third largest banks) and his recent speeches encouraging bank consolidations suggest that antitrust considerations are being relegated to subordinate position.

Professor Lewis is troubled by the problems in transport, where competition has reared its ugly head. One might be moved to share this concern were it not for two important developments: (1) the declaration that mergers involving motor carriers may be approved if they "are consistent with the public interest" has been interpreted by the Supreme Court to mean that antitrust features need not be given primary or exclusive weight; and (2) while the CAB has endeavored since 1955 to open new routes to competition from other smaller lines, the recent encouragement by CAB Chairman Boyd to airline mergers suggests that the Board will be favorably disposed to consolidations such as proposed by American and Eastern—the second and fourth largest domestic trunk lines.

Professor Grether concludes that our "goals and policies, short and long run, need to be examined in terms of the requirements of an effectively competitive market system" and suggests that the results of such an over-all view (as by the Joint Economic Committee and the Council of Economic Advisers) would produce sufficiently greater consistency to reverse the trend. One could not quarrel with this, if he is willing to overlook Professor Grether's earlier observation that under effectively competitive markets we apparently will not let market prices fall. But the President will not let them rise; so our national policy for the moment, at least, seems to call for rigid prices—since, as Professor Grether cautions, nobody is seriously proposing to reduce factor prices, except possibly for the cost of capital. The Council of Economic Advisers' policy calling for price ceilings seems to be saying: "Other industries may increase prices, but not steel because it is a strategic industry." If it is argued that steel cannot raise its prices in 1962, then one must ask whether they can raise prices in any year, and whether other strategic industries will be permitted to raise prices.

Professor Lewis, however, seems heartened by the Presidential roll-back in steel, since in his view "economizing is society's business." Not all will agree with anything approaching a literal interpretation of this view, nor with his observation of our "explicit acceptance of ultimate government responsibility for 'responsible' decisions on prices and wages." If we are "in," as Professor Lewis believes, then the President and the Council, in their concern for steel, should carefully evaluate Bethlehem's recent decision to invest $250 million in new steel facilities. Under Professor Lewis' ambivalence-free rule, is it wise for us to permit Bethlehem to go ahead with this investment in view of the current excess capacity in steel?

The point is that steel is not just an isolated dilemma for public economic policy posed by jointly-acting oligopolists, with strong market power; over one-half of our national markets are involved. One can agree with Professor Lewis that the steel affair was "long in the making," but sharply disagree that "it was essentially an economizing phenomenon," unless this is a new euphemism for muddled and inconsistent policies. Please note that the President's explanation for the price roll-back rested essentially on the question of "good faith" on the part of certain steel executives. What kind of a market system is this? It sounds very much like the Adolph Berle doctrine of reliance on the corporate conscience, the corporate soul, and corporate morals. In this sense, the President and the Council seem to be following the French approach of bringing in leading businessmen to obtain commitments for a certain amount of business investment and a consistent rate of economic growth. In brief, the price policy enunciated regarding steel represents a candid admission of dangerous levels of concentrations of economic power in certain industries and a tacit unwillingness to unleash the antitrust agencies to handle the problem. It will be interesting to note whether the budgets for the Antitrust Division and the FTC's antimonopoly work in fiscal 1964 will contain increases commensurate with the concern expressed over steel.

In conclusion, the Council of Economic Advisers' recognition in its January, 1962, Report that in important sectors of the economy there is—at least in the short run—considerable room for the exercise of private economic power, underscores the need to re-examine the reasons for the drift from the market mechanism, and the holes that have been made in our statutory machinery to maintain that mechanism. One unmistakable lesson the steel fiasco has taught us is that corporate conscience–corporate responsibility is less than an unambiguous guide to economizing decisions. For, as Professor Lewis himself has observed:

> Responsibility wears many faces; as a standard of choice among competing economic claims, all of which may be entirely reasonable, "responsibility" has no determinate shape or measure. Responsibility simply has nothing to do with economizing. Economizing is not a matter of choosing good rather than evil; it involves choosing among competing goods; and an admonition to be responsible tells no one where in the complex of conflicting responsible actions imposed by the nature of the economic process, the action which society wants, is to be found.

Professor Lewis' blueprint sounds good. However, before we embrace the corporate conscience or its governmental counterparts as our main economizing device, before we rewrite our economic history with reasoned justification for exemptions and insulations from antitrust, protection of regulated industries from competition, subsidy and subsidy-like programs, and the use of federal research and procurement contracts to subvert the market mechanism, we might be well advised to pause and consider whether that revolutionary concept of a system of market competition was ever really given an even chance for survival.

JESSE W. MARKHAM: Professor Grether's assessment of public economic policy toward the unregulated sectors of the United States economy leads him to three related conclusions: (1) policy in this area is riddled with inconsistencies; (2) the competitive market system has undergone, and is now undergoing, substantial erosion; and (3) these two concomitant developments cast considerable doubt on the future prospects for maintaining an effective competitive market system.

There is certainly abundant evidence of inconsistencies in public economic policy: price-fixing privileges and high depletion allowances are extended to the oil industry on grounds of conservation; the government then permits the restriction of oil imports to stimulate current consumption from domestic oil reserves. In 1962 almost as much policy effort was expended on keeping steel prices from rising as on making sure that prices in basic agriculture did rise. Nor are these and many other such policy inconsistencies to be explained away on the grounds that the various arms of government frequently work at cross purposes. In December, 1960, the Department of Justice Antitrust Division won its dramatic victory over the electrical machinery manufacturers who allegedly had conspired by nefarious means to raise prices. By the following April the Assistant Attorney General was seeking through consent decrees to prohibit the same manufacturers from reducing prices to levels that might injure their small competitors. And while Section 7 of the Clayton Act is generally viewed as a prohibition on mergers that tend to lessen competition, in the recent Brown Shoe decision the Court was far more intent on preserving the market position of Brown's smaller competitors.

These and similar anomalies become less obvious, of course, when it is recognized that what we call economic policy is more political than economic in character. In 1962 the business population of the United States numbered 4.8 million; all except the largest several thousand qualify under standards established by the Small Business Administration as small business. Over 40 per cent of the total are in retail trade. An additional 4 million firms are in agriculture. In the absence of public policy, i.e., under unrestrained laissez faire, most of these 8.5 million enterprises would operate either in a market environment reasonably similar to atomistic competition or under a system of loose-knit cartels. Public policy having prohibited cartels by the antitrust laws provides substitutes in such variegated forms as resale price maintenance, parity price supports, the Robinson-Patman Act, and specialized credit and financial institutions. Along the spectrum of market power and far to the right in a highly truncated distribution are several thousand firms known to economists and politicians as the hard core of oligopolistic industry. To economists the natural solution in markets they serve are "administered" prices above—sometimes considerably above—marginal costs. Politicians assess them in less technical terms: these firms not only do not need special protection from other economic sectors or from each other, but rather their suppliers, customers, and small competitors need protection from them. Hence public policy is directed toward the mitigation of the power they possess by vigorous antitrust enforcement and periodic investigations by Senate and House committees. At the far extreme are of course the hopelessly monopolistic utilities where the only feasible policy is outright regulation.

When viewed in these broad terms we may adduce certain policy principles having a higher order of consistency than those Grether has adduced: In political terms the government's willingness to protect business firms from the rigors of competition varies directly with the numerical political strength of those who seek it. And since the effort of those who seek such public protection is likely to vary directly with the degree of competition they confront, an economic consequence of public policy is a narrower range of market power, or Lerner indexes of monopoly, among the nearly 9 million business and agricultural firms than otherwise would prevail. In short, while public policy may not have consistently sought such a high order of competition on all sides that all prices tend toward their respective marginal costs, it may have had the effect of bringing about greater uniformity in the differences between the two.

Even if all this is accepted, however, it does not allay Grether's concern over the apparent gradual erosion of the competition market system. Grether has pointed out a few optimistic notes in this generally pessimistic symphony; e.g., the gradual reduction in intrastate trade barriers, recent international trade liberalization, antitrust policy, and innovative dynamism. There is little doubt that antitrust policy has been greatly strengthened in recent years, especially antimerger policy. In 1920, the United States Steel merger of over 60 per cent of domestic basic steel capacity was tried and found innocent of antitrust violation. Recently, the Bethlehem-Youngstown merger of about 20 per cent and the Brown-Kinny merger of less than 6 per cent of their respective markets were declared in violation of Section 7 of the Clayton Act. More merger cases have been initiated in the decade just ending than in the entire history of antitrust. There is also evidence of increased innovative dynamism. Research and development outlays by business firms are currently running about forty times 1930 levels.

Several blows have recently been struck for the competitive market system which Grether left unmentioned. Fair trade is clearly on the decline; since 1946 approximately a dozen states have either declared their resale price maintenance laws unconstitutional or greatly reduced their scope and effectiveness. The trend is toward an abolition of minimum retail milk prices. The recent American-Eastern Airline merger petition was denied on the grounds that such a merger would be in conflict with current antitrust policy. The Federal Reserve Board has initiated a study of bank mergers, and within the past two years the Justice Department has initiated two antitrust cases against banks—an area considered virtually beyond the reach of the antitrust laws for a decade.

These developments should not make us take less seriously Grether's admonition that the competitive market system needs strengthening and the many inconsistencies in public policy be eliminated. They do suggest the presence of trends counter to those he has noted with concern. Public constraints on the market economy have probably increased, but private constraints have probably diminished. On balance, it would be difficult to demonstrate in quantitative terms that the competitive market system is functioning less effectively than it once did, but I agree with Grether that it surely can be made to function more effectively than it presently does. Since public policy created many of the constraints it can also do away with them. Whether the necessary measures will be taken is essentially a political matter, but the range of choice seems to be narrowing. In the economically more advanced nations I detect more than a slight disenchantment with socialism, the most likely alternative. Britain's Labor Party and the West German Socialist Party no longer clamor for nationalization of industry. The socialists on the Continent generally support the Common Market Treaty provisions for private competitive enterprise. Even Mr. Khrushchev recently observed that Communist countries could learn something from capitalism. If we have even the most remote prospects for such a prominent and influential student, it seems to me the course should be made as stimulating and as logically consistent as possible.

HARRY M. TREBING: Over all, I am in agreement with Professor Lewis' survey of the range of problems associated with regulation, and I have great respect for his general viewpoint. I do believe, however, that two points deserve comment. The first is the implication throughout his paper that regulation can be made into "a straightforward economical, functional exercise requiring no great delay in time, nervous energy or expense" if it can be stripped of its needlessly complex, costly, and often confusing procedures; and further, that "we can have better regulation whenever we are ready to recall that regulating is economizing, and, in consequence, are prepared to fashion the thrust and processes of regulation to its simple functional role, and to rid it of its grandiloquence and its ridiculous trappings."

At the possible risk of misinterpreting Professor Lewis' position, I would suggest that this is too great a simplification of the problems faced in refashioning regulation along the lines of a highly efficient economizing instrument. Even if we are able to correct some of the past examples of ambivalence and inconsistent policy which he cites, there still are a number of bewildering variables that must be faced and far too few guideposts for assistance.

Of course, if we were to confine regulation to simply setting the general level of rates and accept an accounting review of allowable expenditures and actual taxes paid, together with a cost of capital rate of return on an original cost rate base as a measure of this requirement, then Professor Lewis' statement is undoubtedly true. However, in recent years what were once "local utilities" have become regional, national, or even spatial in their operating dimensions. Hence, if price and investment control are to have any meaning as economizing instruments promoting the general welfare, it would seem that regulation should keep abreast of such changes.

To be effective in this capacity, regulation must develop truly dynamic standards whereby it can judge public needs and the attendant resource requirements in advance of actual need. Or, expressed somewhat differently, regulation must be able to formulate long-run policies or plans with respect to the industries under their jurisdiction. Interestingly, regulation's failure to accomplish this objective was one of the principal criticisms made by the Landis

Report several years ago. Whether regulation can in fact, however, move to add a planning dimension to its sphere of operation, one cannot say. Perhaps too many years of living with the adjudicatory process and a dependence upon litigants and intervenors to supply information beyond the capacity of a limited staff have foreclosed this type of action. All that one can note is, that whatever standards do exist today, as Professor Lewis observes, they are far more static than dynamic.

Yet dynamic standards alone are not sufficient to promote the public interest unless they can be carried out. Generally, three avenues are open to regulation in its efforts to realize or effectuate its policies in the traditional utility field. The first is the direction of investment through certification and/or control of security issuance or adjusting the rate of return. The second avenue is the development of incentive regulatory techniques to encourage patterns of performance that are deemed desirable within a policy framework: for example, differential rates of return to reward firms for superior efficiency and innovation, and penalties imposed upon those with inferior performance records. The third means or avenue is the potential threat of public ownership from an active public ownership program-in-being. This appears to be the tool most favored by Professor Lewis to compel obedience on the part of the private companies.

As Professor Lewis rightly observes, regulation seldom, however, is capable of moving positively to enforce whatever standards it may have. In the past, certification and security issuance powers have seldom been used as positive weapons to compel performance. No doubt the commissions have been hesitant because of the fear of judicial reprimand for interfering with the sanctity of managerial prerogatives. Similarly, virtually nothing has been done in the area of incentive regulation since the primitive sliding scale efforts of a generation ago. With respect to the effectiveness of public ownership as a means to induce performance, this avenue would seem to be a function of government's willingness to take a positive stand with a far stronger program than that embodied in the old "partnership" philosophy. Whether the current Administration's recently announced power policy of "competitive co-operation" is adequate or simply another illustration of ambivalence cannot be clearly ascertained as yet.

Taken as a whole, therefore, if regulation is to function efficiently as an economizing instrument, it must have dynamic standards and the means to realize or achieve these goals. It may be possible to refashion the commission system along these lines, but there is no indication that the task will be simple or the product wholly satisfactory within the reasonable future. An illustration may serve to prove this point.

In early 1962, the Federal Power Commission instituted a National Power Survey to lay the basis for a national grid interconnecting major points of electric consumption and generation, thereby hoping to realize significant economies in the utilization of plant. Presumably this plan, when complete, will yield a standard or provide a basis for formulating long-run policies toward this segment of the operations of the electric power industry. However, the Chairman of the FPC has subsequently announced that the FPC has neither the authority nor the desire to enforce performance once the plan is complete, thus eliminating direct compulsion by the Commission. Moreover, the threat of public intervention and construction of the transmission lines had already been sensed for several years by the private companies, and they have taken appropriate steps under the apparent aegis of the Edison Electric Institute. On the one hand, the private companies have undertaken an extensive national advertising campaign aimed at winning public acceptance of a privately-constructed transmission grid (while taking credit for originating the idea of EHV transmission without giving sufficient recognition to Morris L. Cook and other public power pioneers), and on the other hand, they have promoted an increasing number of private interconnection schemes between existing operating private companies. At about the same time that the National Power Survey was begun, the federal government released a plan for a proposed 750 kv., d.c. EHV transmission line 1,000 miles long, between the Pacific Northwest and the Pacific Southwest. Simultaneously, two private utilities in the area, Pacific Gas and Electric and Pacific Power and Light in Oregon, announced plans to build a smaller 230 kv. line for 110 miles with an ultimate capability of 500 kv. to interconnect the two operations and possibly serve for wheeling surplus BPA power to California at a later date, thereby supposedly negating the need for the federal line. The Pacific Power and Light Company then went to the Federal Power Commission to get authorization for a security issuance to finance construction of its portion of the inter-tie. Despite the vigorous dissent of one commissioner, the FPC, with the survey still incomplete, approved the security issuance, making no judgment on the relative desirability of the two separate proposed undertakings. The way was now open for what could be, at the least, a duplication of investment.

In this illustration we see all the ingredients of the actual economizing process at work: the groping by the Commission for some kind of standard; the attempt of the regulated firms to create good will and their struggle to lend the impression that they have the situation well in hand while at the same time creating partial or hastily conceived projects to allay possible federal inroads—projects which the FPC must apparently sanction because it has not been able to develop some final standard in advance of need. Whatever else may be said, this can hardly be considered the optimal performance of an economizing instrument, and it might be remembered that the FPC is an agency recently benefiting from both a fresh infusion of new personnel as well as an internal reorganization. Clearly the course of effective modern regulation will be long and tedious.

The second point deserving further comment deals with Professor Lewis' appraisal of the relative roles of regulation and competition in pipeline mergers. Obviously, if the choice were between a merged but adequately regulated pipeline capable of effecting significant service economies or the maintenance of an ineffectual duopoly or oligopoly, then logic would clearly dictate the former. However, the enigma of corporate power and the inability of regulation to constrain its exercise clouds this choice and apparently tilts the scale in favor of antitrust, or at least lends strong support to those who argue for more fluid market structures.

It may be argued, of course, that the kind of rivalry that results from the maintenance of an additional or potential source of supply is an ineffectual check on corporate power. But those who represent consumer interests apparently put more faith in such an arrangement than in the ability of the FPC to control extortionist prices and pass whatever savings might result from the merger forward in the form of lower wholesale rates. One need only look at the position of the state of California in the El Paso case already mentioned by Professor Lewis as evidence of this type of thinking. In fact, concern or respect for the economic power of the monopolistic utility supplier seems to permeate much of the recent activity in the utility field outside of the natural gas industry. One finds, for example, efforts on the part of REA distributors to integrate vertically through generation and transmission loans to prevent dependence upon private facilities, and also evidence that municipal plants and co-ops are planning to construct their own transmission facilities rather than enter into private utility power pools whose transmission lines would presumably be regulated as common carriers by the FPC. But perhaps the best illustration is in the communications satellite field where the Administration advocated the creation of a rather awkward, mixed corporation rather than permit existing international communications carriers to simply incorporate a pro rata share of the satellite investment in their rate base. While the latter course would appear to be the less costly alternative for the reasonable future, concern again seems to center more on corporate power and its ability to impede the diffusion of benefits rather than cost minimization. The answer to this dilemma would hopefully lie in a restoration of confidence in regulation, but that in turn will probably be a function of the Commission system's ability to pursue policies free of the ambivalences and inconsistencies that Professor Lewis has outlined.

HARVEY J. LEVIN: As he is undoubtedly aware, Professor Lewis' highly stimulating paper is calculated to exasperate. Who else would dare to provoke us by prefacing a skillful review of our regulatory past with comments about a recent pricing episode in the steel industry and then leave us hanging mid-air at the end, desperately trying to see exactly what the last three-quarters of the paper have to do with the first quarter! (I think I have the answer, incidentally, but he might disagree.)

To hold, as Professor Lewis does, that public utility regulation has not really worked that badly after all and that inconsistencies in the transport field cannot always be resolved in favor of more competition is noteworthy enough on this occasion. But to suggest further that we actually have something to learn from our regulatory experience in handling problems in the unregulated sector, is surely a bombshell that must not go unnoticed.

Apparently Lewis is not satisfied with merely recovering territory lost in past years to the critics of commission regulation. He has gone on the offensive, as it were, and crossed the sometimes fuzzy boundary line between regulated and unregulated enterprise. Not that oligopolists have never before been threatened by a proposal that they be required to announce price increases beforehand, or by direct commission-type regulation, or even by outright public ownership. But to see this sort of thing almost as a "way of life" hard upon us—as Lewis does—and one with compensations as well as drawbacks—is surely to guarantee the undivided attention of his audience.

Our speaker says that Big Steel was "clobbered" plainly and publicly for all to see. What happened here, he says, must have happened to someone sooner or later because it was symptomatic of a deep-seated economic-social-political process now part of the facts of life of any sensitive oligopolist.

Well, Lewis is obviously not alone in his conviction that Big Steel's price-setting discretion was reconciled with our national commitment to economic growth without inflation by something more than the mere number and size distribution of rival sellers, the industry's entry conditions or product characteristics, or monetary-fiscal constraints alone. Nor was Steel constrained by any simple antitrust threat to take members of the industry to court when government's offer to switch orders from Big Steel appeared to deter neither the price followers nor their leader. (Undoubtedly, of course, there are those who will feel that this antitrust threat might have won the day if it had been more credible.)

Less controversial, perhaps, is the speaker's apparent implication that we can expect to encounter some such direct governmental participation in the economizing process more frequently in oligopolies (with their indeterminate price-output results) than in atomistic structures; and more frequently also in industries whose price-wage decisions, though not regulated by commission, are truly affected with a public interest (defined in national terms). And surely there is no question that government is a crucial buyer in many markets today, or that, whether we like it or not, its expenditures necessarily affect aggregate employment, prices, and growth, and even the structure and performance of particular industries.

But do economists really have anything directly to learn from the study of commission regulation in their perennial search for better ways to reconcile market efficiency with national economic policies? Even a sympathetic listener may wonder whether we yet know enough about what has really been happening in these regulated industries—enough, that is, to use them as any sort of guide on how to reconcile the irreconcilable elsewhere in the economy. Lewis would probably be the first to agree that these same regulatory commissions which were originally created to correct unwanted market biases have tendencies and biases of their own. Now this may not mean that they can easily be jettisoned, but a serious dilemma does arise as to whether one biased economizing agency can really correct the performance of another. To put it in more general terms: Can regulatory bodies with serious tendencies of their own really serve as neutral mechanisms through which political valuations derived from Congress, the executive and the courts regarding the optimal balance between competition and regulation can be transmitted intact to any regulated industry? Or may they not act—the agencies, that is—as systematic if unintended discriminators for and against different performance norms? Perhaps this much at least can be salvaged from the Schwartz article he mentions, from the book by Adams and Gray, and surely from the barrels of testimony produced by the House Subcommittee on Legislative Oversight several years ago.

Even accepting Professor Lewis' provocative contention that government is in the business of economizing for good, where, as economists, do we go from here? What use can we safely and profitably make of the mass of records and experience accumulated during decades of regulation? To what end can we turn that rich "bundle of statutory, administrative and judicial controls over entry, service, and prices . . ." in which he reaffirms his faith so candidly here? After rousing us up off dead center, perhaps Lewis would agree that we should look carefully before we leap, and that a good place to start might be to study more systematically than hitherto the structure, behavior, and performance of these regulated industries.

This requires a discriminating application of industry study methodology and case studies geared to the distinguishing characteristics of regulated industries. For if regulatory experience leaves us with any useful legacy at all, it may well be its greater variety of techniques to influence business behavior (compared to antitrust alone); related to this, its relatively greater emphasis on conduct than on structure (given legalized entry controls); its far more varied and detailed performance standards (again compared to antitrust); and its wide assortment of still inchoate but potentially valuable data gathered under the regulatory statutes.

There is a real abundance of useful empirical material available today to help us fill the gaping holes in our knowledge about the consistency of different regulatory techniques (subsidies, taxation, rate proceedings, informal negotiations, rate-entry-service controls, etc.), on one hand imposed in varying structural-technological settings, and a number of desired performance dimensions on the other. Yet professional energies have thus far gone in surprising degree to the study of the unregulated oligopolies alone, where the pickings may be slim.

Perhaps it is time for a change, although I suspect that it will take a lot more work tying together the two portions of Lewis' stimulating paper before many of his readers will agree that it is time for a change. The stakes are big, and so, the effort may well be warranted: for if the ideas expounded here are valid, there is just an outside chance that a long, hard look at our regulatory experience may also help us structure and evaluate some of the processes already at work, for better or worse, in the unregulated sector and in the economy as a whole.

PRINCIPLES OF ECONOMIC POLICY, CONSISTENT AND INCONSISTENT: INTERNATIONAL COMMODITY STABILIZATION

PROBLEMS OF INTERNATIONAL COMMODITY STABILIZATION

By BORIS C. SWERLING
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The list of inconsistencies between the domestic and the international features of American commodity policies is a long one. Parity between prices paid and received by farmers remains a standard applied internally, but we resist all pressures to stabilize the commodity terms of trade between primary products and manufactured goods moving in international trade. Historical share of world markets is a rule endorsed for our own exports of raw cotton, but we would be considerably embarrassed if the same rule were extended to cover American overseas shipments of soybeans, corn, or inedible fats. Individual commodity agreements have not been entered into for imported raw materials subject to severe cyclical fluctuation in industrial demand, but we participate in agreements governing trade in wheat and sugar, in considerable degree as a means of buttressing domestic farm programs. At a time when there is some concern over the international competitive position of America's exports, we forego the advantages of cheap raw materials by artificial support of the domestic market for raw cotton, petroleum, and certain of the nonferrous metals. We are disturbed by the level of agricultural protectionism likely to prevail within the European Economic Community but resort to a wide variety of devices to shield our own farm sector from movements in world-market prices.

Commodity policies have a particular relevance to current problems of foreign economic relations with the less developed countries. Petroleum is a dominant factor in the political economy of the Middle East; rubber, tin, copra, and rice are of prime importance to the economic well-being of the countries of Southeast Asia; coffee plays a pivotal role among the exports of the Latin-American republics; while the market fortunes of cotton, copper, and vegetable oilseeds significantly affect the foreign exchange earnings available to individual African nations, both for implementing future developmental programs and for servicing international indebtedness already incurred. The enlarged representation of less developed countries within the United Nations increases the political leverage of commodity interests overseas and with it the risk that the General Assembly will urge precipitous actions and false remedies for complex commodity problems.

Certain guidelines for the operation of intergovernmental commodity agreements, as originally drafted for the abortive Havana Charter for an International Trade Organization, received official sanction by a 1947 resolution of the Economic and Social Council. Flagrantly monopolistic abuses, such as characterized the Stevenson rubber scheme in the twenties, were to be avoided by means of publicity in the form of open conferences and annual reports, as well as by the principle of equal voting power for importing and exporting countries. Economic efficiency was to be promoted by granting a preferred position to low-cost producers and paying due regard to the long-run equilibrium price. Where conditions of substantial surplus prevailed, ameliorative measures were to be taken during the life of the agreement.

Unobjectionable though these commodity principles may appear, a wide gulf separates the stated objectives from the actual attainments of the commodity agreement of longest postwar duration, that governing wheat. Here, as in several other instances, rapid changes in external circumstances have combined with the internal logic of the control mechanism to erode the basic instrument and to suggest that longevity in individual commodity agreements may be something less than a virtue.

The International Wheat Agreement, initiated in 1949, is the prototype of the multilateral long-term contract form of international commodity agreement (I.C.A.). Importers agreed to accept specified quantities if the market price fell to the Agreement minimum, and exporters agreed to supply stated quantities to member countries at the specified maximum market price. Between the floor and the ceiling, prices were expected to move freely and the contractual obligations to become inoperative. The four-year life of the original Wheat Agreement was marked by the currency devaluations of September, 1949, high-level price supports in the U.S., the Korean commodity inflation, and a light world harvest in 1951 succeeded by a bumper crop the following year. I.W.A. prices held at or close to the Agreement maximum throughout the period, though for a succession of reasons and with diverse consequences: (1) In a falling market preceding the outbreak of the Korean conflict, there was some tendency for the I.W.A. ceiling to be regarded also as a floor price that exporting countries were reluctant to undercut. (2) A subsequent run-up in "free" wheat prices during 1950-52, in step with commodities generally, led importers to opt pretty much for their full I.W.A. quantities, stimulating wheat consumption and discouraging its production during a period of acute shortage. (3) Depite the bumper grain harvests of 1952-53, the Canadian Wheat Board held its "free" export price at a high level as a bargaining tactic for negotiating a higher price target in the new Agreement signed in 1953. (4) Importing countries, with the higher 1953-54 price ceiling in prospect, built up stocks by purchasing their remaining quotas before the lower maximum prevailing under the 1949 Agreement had expired. (Farnsworth, *Q.J.E.*, May, 1956, pages 220-28.)

Subsequent developments under successive extensions of the I.W.A. may be summarized more briefly. Actual wheat prices were not permitted to fall to the floor price specified in the 1953 Agreement by joint action on the part of Canadian and American wheat marketing agencies. In any case, while implicit income transfers from the relatively rich wheat exporters to relatively poor importers were tolerable during the earlier period of shortage, transfers in the reverse direction at the floor price would have been less conscionable. This was especially so when the U. K., the major importing country, failed to participate in the 1953 Agreement on the justifiable grounds that the new price range had been set too high. Far from bringing consumption and production into balance, the agreements tolerated the rise of world wheat stocks to record levels. Disposal, particularly of American wheat, under special concessionary terms encompassed an increasingly large share of world trade, requiring the reciprocal commitments between importers and exporters to be restricted to commercial transactions, and for these only up to specified percentages of recent trade volume. The first substantial decline in world wheat stocks did not come until the end of the 1961-62 season, an outcome toward which an important role was played by the enlarged takings of a nonmember country, Mainland China.

Comment on other existing agreements must necessarily be more brief.

1. The International Sugar Agreement of 1953 was of the more traditional export-quota variety. The floor price was under pressure from the beginning and such success as was had in maintaining it was due in large part to Cuba's willingness severely to restrict the size of its domestic crop. Yet existing stock accumulations proved too modest to offset inflationary pressures at the time of the Suez crisis, and world-market prices about doubled within the span of a few months. For this commodity it has been Cuba, the major exporter, that has traditionally been the major advocate of heavier volume even at somewhat lower prices. Ironically though it may now appear, long-sustained membership of the U.S.S.R. was originally heralded in some quarters

as a harbinger of international economic co-operation. More recently, the Cuban-Soviet *rapprochement,* which led to the embargoing of Cuban imports into the U.S., has brought a wholesale rechanneling of international sugar shipments to the point where the crucial export-quota provisions of the Agreement have had to be suspended. Moreover, and for a variety of reasons, world-market prices for sugar have proved stronger since quotas were removed than during the period when they were actually in effect.

2. The International Coffee Agreement, recently negotiated but not yet ratified, suggests interesting contrasts with U.S. sugar policy. Coffee, unlike sugar, has hitherto entered the U.S. market at the world price, duty free, and lacks any competition from domestic producers. But while U.S. sugar imports are allocated strictly on the basis of national legislation, total world marketings of coffee to traditional outlets are to be governed by export quotas specified in a multilateral instrument. Moreover, barter transactions in traditional markets are debarred under Article 54 of the Coffee Agreement, whereas the most recent extension of the U.S. Sugar Act gives a favored position to sugar exporters willing to contract for specific quantities of U.S. agricultural exports. Complications can also be expected to arise for the Coffee Agreement because this is a less homogenous commodity, and regulation of total marketings can serve to distort price differentials prevailing as between *robusta* varieties, grown in Africa, and the Brazils and milds originating in Latin America.

3. The buffer-stock provisions of the International Tin Agreement proved ineffective in holding the floor in the face of Soviet Union exports a few years back, whereas the current ceiling on tin prices is governed not so much by the price range specified in the Agreement itself as by the terms on which the U.S. disposes of tin in excess of the present needs of the strategic stock pile. Indeed, the existence of U.S. stock piles of strategic materials as well as agricultural surpluses provides some assurance against major inflation in commodity prices along a broad front and seriously undermines much of the argument for international buffer-stock accumulations as an instrument of current market support.

One can readily identify various factors that have operated so as to damp the instability of individual commodity prices in the postwar period. Of special importance has been the more moderate character of postwar business cycles to date. Modern production techniques in agriculture make major field crops somewhat less susceptible to fluctuation in yields. The range of intercommodity competition has considerably broadened, not merely because of the rise of synthetics and plastics, but also as between structural steel and reinforced concrete, while soybeans now find extended uses over the general range of fats and oils, and aluminum has invaded traditional markets for steel and copper. But for the emergence of the American synthetic-rubber industry, the full impact of rising postwar demand, while bringing temporarily inflated profits to natural-rubber producers, would likely have induced cyclical overplanting of rubber trees comparable to that which has actually occurred for coffee. In the case of raw cotton, the U.S. price support program has underwritten prices on a world-wide basis but has placed this country in the role of residual supplier, subject to wide year-to-year swings in exports while permitting other countries to increase their share of the total market. For tea, production excessive to export requirements has been rather readily absorbed by domestic consumers in the Orient.

At the same time, new elements of price instability have been emerging. Many of these have been peculiarly political in character, and accordingly all the more difficult to deal with. The rubber and wool markets have witnessed sudden entry and withdrawal of U.S.S.R. purchasing, for motives apparently related as much to cold war strategy as to strictly commercial ends. In aluminum as well as tin, relatively small Soviet exports have on occasion had seriously disruptive effects. Control of Manchurian soybeans by a Communist Chinese regime is a supplementary factor of disturbance, while the Communist threat overhangs the rice bowl, the tin, and the rubber of Southeast Asia. Primary producing countries have also by their own actions set in motion serious commodity disturbances. One need recall only Castro's actions in expropriating sugar properties, the Iranian oil episode, nationalization of Bolivia's tin mines, or the bearing of political crises in the Congo and the Rhodesias on copper supplies.

The minerals are a commodity category that is peculiarly involved in economic relations between the less developed and the advanced countries. That trade is triply hazardous: first, because the changing technology of the industrial system raises or lowers the market value of successive minerals in unpredictable fashion; second, because sources of supply are distributed around the world in haphazard fashion; and, third, because rich portions of the world's mineral resources are subject to arbitrary regulation by newly independent nations jealous of their sovereign prerogatives and prepared to pay a high economic price for a fervent anticolonialism. Even in the less developed countries, and in striking contrast with much agricultural production, mineral operations tend to be conducted on a large scale by heavily capitalized enterprises servicing metropolitan markets. In the interests alike of commodity stability and economic growth, the rules of the game by which private mineral enterprises are to operate in the less developed countries need to be more clearly established. The risks of expropriation and of confiscatory tax burdens create an environment hostile to private savings, whether from domestic or foreign sources, and go far to negate even the best considered programs of foreign aid.

In the context of the European Common Market, broad sectoral problems and not merely those affecting individual commodities become important. While a strong motive for the founding of the European Coal and Steel Community was to militate against future French-German hostility through unification of their heavy industries, that Community cannot be insulated against the effects of the emerging Common Energy Policy. Reconciliation is necessary between a host of conflicting considerations. Some recent events, such as the discovery of a rich field of natural gas in the Netherlands and the opening of oil fields in North Africa, together with the persistently high cost of Belgian coal, buttress protectionist sentiment on the Continent. The heavy outflow of petroleum from the Soviet Union, partly through the intermediary of Italian refining, creates an important strategic issue for the NATO powers. There has been a progressive undermining of the old international regime, based largely on the strong market position of international corporations controlled by American, British, and Dutch capital; the international price structure for petroleum has been seriously disorganized; and new forms of market control are being considered by such an agency as the Organization of Petroleum Exporting Countries. The Soviet Union cultivates the favor of the traditional exporters by backing their criticism of the Western companies while the OPEC countries (with the possible exception of Venezuela) remain surprisingly insensitive to the deterioration in their own position as the result of Soviet competition.

The essential source of difficulty in the standard type of international commodity agreement, which merely sets a relatively high price target and allocates export quotas to enforce that price, is that appropriate canons of economic adjustment, of social equity, and of efficient administration are required to work at cross-purposes. Unsalable current output is overt evidence that a commodity is already overpriced; yet the marginal incentive to expand output continues, while there is, depending on demand elasticities, some tendency to discourage consumption. The enormous advantages of price signals for coaxing rather than coercing needed adjustments are lost. Nor do benefits that are strictly proportional to marketings meet reasonable criteria of income redistribution, domestically or internationally. Through effects on capital values, moreover, better market prices today are built into higher price structures for succeeding generations of producers, with consequent dissipation of intended benefits.

For these reasons, we must recognize a standing challenge to devise new and unorthodox instrumentalities that provide for international transfer of funds without relying so heavily on artificial market support of individual commodity markets. In this connection, several mechanisms are of special interest:

1. The system of variable import levies, which plays so important a role in the EEC's Common Agricultural Policy, arouses exceedingly mixed reactions. Since the levies will tend to rise at times when external supplies are relatively abundant, the instability of world-market prices will tend to be accentuated in the short run, quite apart from long-run effects in increasing European agricultural output. Accommodation of British agricultural policy, with its reliance on a system of direct payments to growers, has proved a major stumbling block in negotiating the U.K.'s accession to the EEC, and for understandable reasons. For Britain, the changeover would involve abandonment of a technically superior system of agricultural support, a considerable rise in food prices to domestic consumers, and, because U.K. imports are heavy, a disproportionately large British contribution to the common Agricultural Fund, part of which would finance export subsidies on agricultural surpluses subsequently generated on the Continent. Yet the Agricultural Fund itself, as a rudimentary system of taxation, has served to initiate important discussions of fiscal burden and outlay that are relevant to a wider range of Common Market problems.

2. In modern tax systems, the contribution of commodity levies is trivial as compared with direct taxation of personal and business income, but any international fiscal system must begin on a more primitive level. The United Nations organization is plagued with serious budgetary problems, the solution of which is much in the interest of the primary producing countries. Might the U.N. perhaps be empowered to impose a levy on petroleum, a commodity peculiarly well suited to provide a broad tax base? It tops the list of internationally-traded products, and national consumption is closely correlated with level of economic advancement. By virtue of the enormous physical quantities involved, the per-unit rate could be held low and the marginal choice between alternative fuels little affected. U.N. revenues would rise in step with higher civilian consumption in the West and the growth of Soviet output. The principle would be established that this key raw material makes a special financial contribution to the international community, whether for technical aid or Congo-type operations, and, similarly, that the international community has a clear interest in the rational management of petroleum resources.

3. Of considerably more current interest is the serious attention now being given to international compensatory arrangements as an alter-

native to market support for the exports of primary producing countries. A considerable variety of technical arrangements are feasible, but the essential features are as follows: (*a*) There would be no direct intervention in commodity markets; (*b*) some estimate would be made of trend, whether in price of individual commodities, or in a nation's export proceeds from a given commodity or from primary commodities as a group; (*c*) if prices or receipts fell below trend by more than some stated margin, exporting countries would be entitled to receive financial compensation. Proposals vary on the extent to which reimbursement would be made in the reverse direction, should price or export proceeds rise above trend.

While an individual-commodity approach to compensation for export shortfalls would have the advantage of permitting maximum accommodation to the economic peculiarities of different primary products, so limited an approach would also suffer from certain serious limitations. Mere specification of "the" price to be taken as the standard runs into serious complications; e.g., there are customarily a number of subcommodities whose price experience may show considerable divergence. A key assumption of the compensatory plan is that price instability is essentially beyond the control of the exporting nation, but for individual commodities the direct impact of such deliberate national policies as export subsidies or marketing controls is by no means inconsequential, and the volume of exports will also reflect changes in the rate of domestic absorption or policies affecting domestic price structures. The larger the number of countries participating, the more complicated becomes the process of decision making. Yet to permit the existence of outsiders would mean that markets might in fact be responding to destabilizing imports or exports of nonparticipating countries. The individual-commodity approach would appear to be less appropriate to foodstuffs than to industrial raw materials, for which cyclical fluctuations in price and quantity can ordinarily be expected to be mutually reinforcing.

If we go beyond single-commodity compensatory schemes, a wide range of alternatives are open. Are shortfalls only in export proceeds, or earnings on current account, or the entire balance of payments of immediate concern, and is the level of reserves also a relevant variable to be taken into account? Are payments to be triggered by the export performance of individual primary producing countries, or the import expenditures of a few industrial nations? Should the scheme be regional or global in scope? If shortfalls in export proceeds are chosen as the variable to be compensated, should the full export list be included or only a select group of primary products?

Consider this last point in more detail. It might appear that an ineligible group could readily be chosen according to various criteria: coal, because it is primarily an export of industrial countries; rayon, or synthetic rubber, both of them highly processed products originating from comparable sources; wheat, bacon, and butter, because of their association with a relatively rich group of exporting countries. Yet these commodity particulars do not stay put. Iron ore has become an increasingly important export of the primary producing countries. Difficulties would arise, similarly, in establishing the stage of processing at which eligibility is lost. Does one include both raw and refined sugar, only crude petroleum or also petroleum products, aluminum as well as bauxite?

The issue of global versus regional coverage raises a different set of equally explicit considerations. Perhaps the U.S. might begin with an arrangement limited to Latin America, a region with which our ties are particularly intimate. But major commodity fluctuations are worldwide in scope. An arrangement with Latin America would be less appropriate as a strictly contracyclical device than one that included the natural rubber economies of Southeast Asia, and would also deny itself the financial contribution from expanding petroleum exports of other regions. Moreover, a heavily Pan-American flavor would push the world further away from multilateralism and in the direction of formalizing economic blocs.

A crucial question is whether compensatory arrangements are really to be regarded as a form of short-term credit, pending reversal of temporarily adverse circumstances, or as foreign aid involving international income transfers of a permanent sort. To the extent that the arrangement relies on loans that are to be fully repaid, operational questions can be intelligently decided; but there is a good deal less appeal to its potential clientele. On the other hand, allocation of foreign aid proportionally to shortfalls in export proceeds has little appeal for a country like India, for which fluctuations as such are not a serious problem. Nor would it be easy to justify the relatively large transfers that would be enjoyed by Malaya, whose export proceeds have been liable to rather wide swings. The choice between credits and grants will also affect the appropriateness of employing the IMF as the administering agency, through liberalization of the terms under which the primary producing countries are entitled to borrow.

For Latin America, of all continental areas, a purely credit arrangement would seem particularly inappropriate. For no region of comparable size has the export performance since the mid-fifties been less satisfactory. There are, of course, national exceptions to that pattern, but there appears to have been a secular decline in the region's relative position in a considerable number of export staples—tin, coffee, wheat, corn, and petroleum being notable examples. One need not engage in that great guessing game—projecting the prospective terms of trade for primary products—and yet be able to recognize that such gross shifts in regional sources of supply involve difficult problems of economic adjustment.

Among those whose allegiance to private enterprise is most solid, intervention in commodity markets is somehow more respectable than direct financial transfers that are less disturbing to the price system—the major organizational mechanism of a free economy. Legislators who endorse liberal political causes simultaneously espouse import quotas or tariffs for individual primary products, whether wool or the nonferrous metals, petroleum or sugar. Those who defend financial orthodoxy, exchange rate stability, and a balanced budget in the interest of a sound dollar are major defectors on the issue of commodity price supports. In taking such positions, they parallel the views of producer groups anxious to conceal the financial costs, and ready to ignore the intangible burdens, of individual commodity controls.

Accordingly, the political pressures are strong to look to commodity agreements as a means of dealing with the unfavorable impact of the EEC Common Agricultural Policy upon the products of the temperate zone. Similarly, at a time when the more direct forms of foreign aid are meeting increasing resistance in the U.S. Congress, it is tempting to fall back upon individual commodity agreements as an alternative means of assisting the less developed countries. Yet a belief that such agreements can do more to alleviate than to perpetuate underlying commodity maladjustments must be based on something other than historical experience.

INTERNATIONAL COMMODITY STABILIZATION SCHEMES AND THE EXPORT PROBLEMS OF DEVELOPING COUNTRIES

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While I was on a lecture tour in South America last spring, the question most frequently raised during discussion periods was, "Why don't the United States government and the international assistance agencies concentrate their efforts on stabilizing the prices of our exports? This would mean more for Latin America's economic development than all of the loans and technical assistance we are likely to receive."

I was never able to give a satisfactory answer to either the question or the assertion, but the purpose of this paper is to suggest a basis for such an answer. I must begin by saying that I regard the problems inherent in the behavior of primary commodity exports as being very serious for developing countries, and my critical remarks concerning various suggested solutions should not be taken as an effort to minimize the problems. My main concern is that in seeking solutions, we do not concentrate on "symptoms" rather than deal with those factors which are basic to the process of economic development.

Inherent in the question quoted above—and indeed in much of the literature on international commodity stabilization in relation to developing countries—are several ambiguities or questionable assumptions, which I would like to identify.

1. There is an ambiguity between stabilization and the maintenance of a fair price. Now, of course, every sophisticated person knows the difference between ironing out the peaks and the troughs in fluctuations above and below a long-term trend, on the one hand, and fixing a price at some desired level, on the other. Moreover, many if not most proposals for dealing with international commodity problems, whether they are concerned primarily with prices or with export proceeds, have as their announced objective stabilization rather than long-term price fixing. Nevertheless, there is a bias in most so-called "stabilization" programs in favor of halting or reversing declines in prices or earnings. Hence, stabilization plans often become in practice price or income maintenance programs.

2. Assuming that stabilization rather than the fixing of a fair price or return is the objective of a commodity program, there is considerable ambiguity in the concept of stabilization itself. The long-term

trend from which deviations in prices or export proceeds are to be minimized can only be determined by an arbitrary formula or some judgment as to what the trend is or ought to be, and there are any number of methods of calculating trends. The amplitude of fluctuations differs substantially depending upon whether you employ a straight-line trend fitted by the least squares method for a given period or whether you calculate the trend by means of, say, a five-year moving average. For example, the average annual fluctuation in export proceeds of primary exporting countries during the 1948-58 period is 12 per cent when calculated by means of a trend-line fitted by the least squares method [1, page 40], and 8 per cent when calculated by means of a five-year moving average [2]. Other approaches to stabilization avoid any formula for calculating trends. For example, under a buffer-stock arrangement an attempt might be made to restore the original composition of the stock of money and commodities, say once every three years.

3. It is generally assumed that primary commodities can be defined unambiguously and that their production tends to be material resource and labor intensive. The fallacy here is, of course, too obvious to dwell on. Not only do many products included in the category of primary commodities involve considerable processing, but their production may be highly capital intensive. Moreover, with the development of synthetics and new methods of production, the relationship between primary products and factor intensity is becoming more and more nebulous [3, pages 151-57].

4. It is assumed that there exists a relatively well-defined group of primary exporting countries which have the common characteristics of being poor and underdeveloped and, therefore, programs of international commodity stabilization or stabilization of export proceeds from primary commodity exports are mainly devices for helping poor countries. However, in 1960, the industrial areas of the free world exported primary products totaling $23.2 billion while the nonindustrial areas of the free world exported $25.3 billion in primary products. Moreover, if we exclude the primary product exports of Australia, New Zealand, and South Africa from the nonindustrialized areas, the exports of primary commodities of the industrialized countries and of the nonindustrialized countries are approximately equal. Also, for a number of relatively high income countries including Denmark, Australia, Ireland, and New Zealand, primary products constitute over half of their total exports. On the other hand, in the case of underdeveloped countries such as India and Israel, primary products constitute less than half of total exports. Thus, any world-wide scheme that seeks to deal with the problem of instability of export proceeds from primary products for

the poor or underdeveloped countries alone, is faced with the problem of arbitrary classification of countries. This is also true of any scheme which seeks to limit its application to primary exporting countries—a classification which is widely used in the United Nations literature.

5. It is generally assumed that balance-of-payments difficulties of poor countries or primary exporting countries are mainly a consequence of fluctuations in the prices of, or export proceeds from, primary products. However, there is considerable evidence that relatively short-term fluctuations in export proceeds are a contributing, but not a major, cause of the balance-of-payments difficulties of most poor countries. Moreover, where export proceeds have been expanding satisfactorily over time and where appropriate financial policies have been followed, balance-of-payments adjustments arising from temporary shortfalls in export proceeds have been handled with relative ease.

6. It is generally assumed that, except for crop failures or natural disasters, fluctuations or declining trends in export proceeds of poor countries are in virtually all cases a consequence of international forces beyond the control of the individual country and, hence, call for an international solution. Although it would be difficult to determine the percentage of cases over the postwar period in which export shortfalls in a particular commodity were a consequence of national policies rather than international factors, there are certainly many examples of cases where internal price and investment policies have been the major factor in the decline in exports. Argentina and Uruguay are, of course, outstanding examples, but there are a number of other cases where decreases in export proceeds are largely, if not entirely, a consequence of national policies.

7. Finally, we are told that fluctuations in export prices and proceeds constitute a major, if not the major, deterrent to economic development. This hypothesis is sometimes defended by correlating short-term fluctuations in the purchasing power of exports with fluctuations in real fixed investment [4, page 11] [1, pages 60-64]. However, aside from the fact that the correlations presented by United Nations documents are not especially impressive and in most cases the statistics on real fixed investments are not reliable, there is a danger of reading too much into such correlations. At most they indicate that when countries are forced to curtail imports they curtail imports of capital goods, and imports constitute a high percentage of fixed investment in less developed countries. More importantly, they tell us little about the character of the investment or its relation to development. Why, for example, has Venezuela, whose exports have had a steady rise and regularly constitute nearly 30 per cent of the total exports of the twenty Latin-American republics (and with only 5 per cent of the population) failed

to achieve a broadly based economic development? Mexico, with five times the population of Venezuela, has had a much better over-all development record, and Mexico's exports have been only a third of that of Venezuela and much less stable. High and stable export earnings may mean little more than stability of consumption and investment in luxury apartments and office buildings in the metropolitan centers or a rapid development in the export sector of an economy with little impact on the remainder.

Individual Commodity Agreements

By and large, individual commodity agreements are of three types: (1) those based on limiting market supply by means of export quotas in order to maintain price at or near the desired level; (2) those employing multilateral long-term contracts; and (3) those which rely upon an international buffer stock to maintain prices within a given range by means of purchases and sales from the buffer stock. Where the basic purpose is the elimination of sharp and reversible fluctuations in world-market prices of storable commodities, the buffer-stock arrangement has important advantages, particularly if the manager of the stock is permitted to operate freely on the basis of his informed judgment regarding longer range demand and supply conditions and if he is provided with sufficient resources in the form of both foreign exchange and commodities to offset the reversible fluctuations peculiar to the demand and supply patterns of the individual commodity. A wise manager will, of course, take into account the effects of this year's prices upon supply, and perhaps demand, in succeeding years. However good his judgments, they may be thwarted by national programs which provide for subsidies to producers to compensate for reduced international prices. In addition, the existence of large national stocks, such as exist today in the case of coffee, would pose a continual threat to the operations of the buffer-stock manager unless there was an agreement with respect to the maximum rate of liquidation of these stocks. In the absence of such national programs or of large national stocks, there is theoretically no reason why a competent buffer-stock manager could not achieve the objectives of a true commodity stabilization program for individual commodities and the result, so far as the proper allocation of resources is concerned, could be more beneficial than that which would occur without a stabilization operation. Unfortunately, these ideal conditions have rarely, if ever, been achieved. Moreover, many commodities cannot be stored except for short periods.

The best example of the multilateral long-term contract arrangement is provided by the series of International Wheat Agreements. Under this type of agreement, each consuming country undertakes to purchase agreed quantities at prices no less than a stipulated minimum while producers agree to supply certain amounts at prices no higher than a stipulated maximum. Even if virtually all producers and consumers are members of the agreements, there will almost inevitably be a free market price outside of the maximum and minimum range. Producers will tend to sell their extraquota output at the free market price, and if this price is well below the minimium contract purchase price, it will tend to undermine the contract price and also create a two-price system in the consuming countries. This may also occur under a straight export-quota system if some producing and consuming countries are not members of the agreement. On the other hand if the individual producers are required to sell extraquota output at low free market prices, this might tend to discourage production.

Much more common has been the export-quota approach under which prices to producers are divorced from realities of the world market, with governments either absorbing the extraquota production or limiting output through national price or direct production controls. Theoretically, it is possible to achieve the same results in terms of world-price stabilization through the export-quota method as might be achieved under a buffer-stock arrangement. Thus, excessive stocks accumulated by governments in years of heavy supply or low world demand would be followed by a liquiditation of these stocks in other years. But, administratively and politically, a true stabilization operation by this means is almost impossible to achieve, since the determination of national quotas must be made through international negotiations as opposed to stabilization operations undertaken in the world market in accordance with the informed judgment of an independent buffer-stock manager. Under the export-quota system, each participating country has two somewhat conflicting objectives when entering into negotiations: to keep the world's total supply as small as possible and to maximize its own share of that limited supply. Moreover, if the country is an important world supplier and has built up a large stock as a consequence of export curtailment, this enhances its bargaining power for obtaining a larger export quota for itself. For example, in recent negotiations on the International Coffee Agreement, Brazil has argued that because it has accumulated stocks in excess of annual world consumption, this fact should be taken into account in the determination of Brazil's export quota.

A true stabilization program, as we have defined it, can only operate successfully if the long-run equilibrium price which the scheme seeks to approximate is permitted to have an unobstructed impact on output in individual countries. Otherwise stocks will be accumulated in certain countries and these will constitute a continual threat to the market.

Alternatively, of course, the agreement can provide for production controls to be imposed by individual members which are consistent with the export quotas established. Indeed, such controls are provided for in the International Coffee Agreement of 1962 (see Articles 48-50). But it must be recognized that the introduction of production controls constitutes a departure from the traditional principles of stabilization operations, and the objective of the scheme becomes one of establishing "fair" or "equitable" prices rather than limiting fluctuations above and below a long-run equilibrium price. Thus, for example, in the 1962 International Coffee Agreement, there is a curious kind of contradiction in the statement of objectives of the Agreement which reads: "To achieve a reasonable balance between supply and demand on a basis which will assure adequate supplies of coffee to consumers and markets for coffee to producers at equitable prices, and which will bring about long-term equilibrium between production and consumption."

What I have been saying in effect is that commodity agreements operated in accordance with true stabilization principles are probably not politically feasible. If the schemes really aimed at establishing a long-run equilibrium price, substantial changes in the relative shares of the export markets of individual countries, as a consequence of differing costs and supply conditions in producing countries, might well take place. Politically, countries are motivated by a desire to maintain or increase their share of the world market, and this has certainly been reflected in the negotiations among Latin-American and African producers for a coffee agreement. It should also be said that in the case of commodities like coffee with long gestation periods, long-run equilibrium prices are at best difficult to determine, and arrangements to maintain them might require an accumulation of large stocks over relatively long periods of time during which both the supply and demand conditions under which the equilibrium price was originally calculated might well have changed to a considerable degree.

Implications for Development

The implications of various international stabilization or price-fixing schemes for economic development depend in considerable measure on the national programs accompanying the international arrangements. For example, if under an international export-quota system domestic output is maintained and above-quota surpluses purchased and accumulated by the government, there is an investment in stocks of commodities which is not only unproductive but which in later years may have to be destroyed or dumped at prices far below the purchase price. In addition to involving an unproductive governmental investment, such a policy would continue to promote, or at least not discourage, investment in the primary-product industry and delay the movement of resources out of the industry.

What would be highly desirable for an individual producer country is a modernization of its primary-product industry, which will enable output to be maintained or expanded at a lower cost. This might be desirable even under an international scheme which seeks to limit exports to the world market. Ideally, what is required then is increased productivity achieved through modernization and the elimination of high-cost producers, while at the same time keeping output from rising so fast as to depress world prices. But simply letting prices to domestic producers fall will not necessarily achieve either of these objectives in the short run. Increased investment for modernization may be discouraged by low prices and, in many countries, for various institutional reasons plus the existence of plentiful low-wage labor, investment for increased productivity in primary production has not been encouraged by high prices either. On the other hand, low prices alone may not greatly affect output or the transfer of resources in the short run. What may be needed, therefore, is a government program designed to take marginal land (or other resources) out of production of the commodity in question while at the same time encouraging investment for higher productivity in the remaining area. This also means that productive employment will need to be found for the displaced resources, including labor.

It is very doubtful whether any international export-quota scheme can maintain world prices above long-run equilibrium levels for any substantial period of time. What schemes like the International Coffee Agreement might be able to do is provide time for countries to adjust output and costs to the long-run realities of world-market conditions. By and large, however, such adjustments, if they are to be made at all, must be done on a national basis, possibly with assistance from external financing and technical assistance agencies. In the absence of an adjustment program, it can well be argued that the very existence of international commodity stabilization schemes will remove the urgency for making the necessary adjustments and that it would be better simply to let market forces run their course. On the other hand, sharp decreases in export proceeds may hinder the process of long-run adjustment.

Although I tend to be skeptical regarding the possibility of successful individual commodity stabilization schemes in modifying price fluctuations, it is not my intention to take a position against them. Perhaps it is worth experimenting more with them, particularly where an effort is made to include both consumer and producer countries in the agreements and where countries are encouraged to intensify their efforts to increase productivity and to diversify their economies, and are discouraged from relying on such schemes to maintain or increase their foreign exchange earnings from the export of two or three primary commodities in the face of uncertain or possibly adverse long-run demand and supply conditions.

Even under the most ideal administration of buffer-stock or quota schemes, there are many commodities that simply do not lend themselves well to operations of this type. This is true, for example, in the case of commodities which compete closely with synthetics or with other natural commodities which are close substitutes and in the case of commodities such as cotton, sugar, and wheat, which are produced by highly industrialized countries for their own use and for export, and where price and output policies are largely determined by domestic political forces. This does not mean, of course, that commodity study groups with power to make recommendations to governments on export and import policies with respect to commodities such as cotton, wool, wheat, sugar, and rubber cannot be of value in achieving more orderly world markets for these commodities, but I seriously doubt the feasibility of international stabilization agreements with respect to more than a few international commodities such as perhaps coffee and cocoa. Certainly any idea that the whole problem of stabilizing the export proceeds of the vast majority of less developed countries by means of a series of international commodity agreements is highly unrealistic.

Schemes for Stabilizing Total Export Proceeds from Primary Commodities

The realization that individual stabilization arrangements are at best suitable for only a handful of commodities and that, in addition, such schemes are likely to have only limited success in achieving their objectives, has led to a consideration of devices designed to stabilize the total export earnings of primary exporting countries. It is well beyond the scope of this paper to comment on all of the numerous proposals which have been put forth in the past for the compensation of reductions in export proceeds from primary products [5]. I shall, therefore, confine my discussion to the major schemes for stabilizing export proceeds that have been formulated over the past two years by United Nations or Pan American Union groups [4, 6, 7, 8, 9] and have as their principal objective the compensation for reductions in export earnings from primary products. None of these proposals currently under discussion within the United Nations or the Organization of American States is designed to maintain a particular level of export proceeds over long periods of time or to limit the volume of exports as a means of maintaining prices. While the terms "stabilization of export proceeds" and "development insurance fund" are employed in the official statements of these proposals, in general the schemes do not purport to be self-liquidating in the sense that compensations for decreases in export proceeds will always be balanced at some time in the future by equivalent repayments on the part of countries receiving compensations. Their principal aim is to reduce the impact of substantial declines in export proceeds from the level of the immediately preceding years rather than to maintain a given level of export earnings over time.

The U. N. Experts' Proposal

The proposal by the U. N. Committee of Experts entitled, *International Compensation for Fluctuations in Commodity Trade* (1961) [4], provides for a development insurance fund to which both developed and underdeveloped countries would contribute, and compensations would be made to both low-income and high-income producing countries for shortfalls in export proceeds in accordance with an automatic formula. Shortfalls would be measured from a moving average of the three preceding years, but alternative suggestions are made in the U. N. Experts' plan with respect to both the minimum deduction before compensation could be requested and the amount of the compensation as a percentage of the shortfall. Compensations would take the form of either grants or contingent loans, but the Committee appears to favor a combination of the two types of benefits, at least for the low-income countries. The development insurance fund would be financed by an initial subscribed capital fund and annual contributions, plus repayments of contingent loans, the repayment obligations being related to any increases in proceeds over a three-year moving average. However, capital subscriptions and contributions to the development insurance fund would be established in a manner which would assure that benefits would substantially exceed contributions and repayments for the low-income countries, while just the opposite would be true for the high-income, industrialized members.

The solution to the problem put forth by the U. N. Experts' Committee in terms of an insurance program is surely a curious one. First, the degree of instability of export proceeds, i.e., the degree of risk, varies greatly from country to country depending upon the commodities which it exports; yet a common insurance scheme is provided for all, at least for the low-income members. This is a little like an insurance program with more or less common premiums and benefits, which covers damage to automobiles from accidents, damage to homes from fires, and accidental death from drowning, regardless of whether the insured owns or drives an automobile, owns a home, or ever goes swimming! Second, the so-called "insurance" program is deliberately designed so that the expected benefits to the low-income countries will exceed by a substantial margin the premiums paid by this group, while for the high-income countries, expected benefits will fall substantially short of the premiums paid. Finally, the insurance scheme moves considerably away from the rationale presented in the statement of the problem, by providing compensation for all commodity export proceeds rather than limiting them to the highly vulnerable primary products.

It might be mentioned at this point that the more recent report of an experts group appointed by the Secretary-General of the Organization of American States, while proposing a compensation scheme which is similar in many respects to the U. N. Experts' report, avoids many of the anomalies of the U. N. Experts' proposal. For example, under the OAS Experts' scheme, the concept of insurance is absent and, although both high-income and low-income countries would contribute resources for the operation of the compensation program, benefits would be limited to the low-income countries. In addition, all compensations would take the form of loans which would be repayable from increases in export proceeds over a three-year moving average within a period of three years, or if export proceeds do not rise sufficiently to repay the loan obligations over a three-year period, there would be an obligation to repay within five years except under special circumstances and upon a vote of the compensation institution. Thus, the OAS Experts' scheme, while providing for partial compensation for shortfalls in export proceeds from a three-year moving average, approaches more closely the principles of a stabilization operation [9, pages 7-13].

The Relationship Between Commodity Stabilization and Export Proceeds Stability

The justification for compensation schemes for dealing with shortfalls in exchange proceeds of so-called "primary exporting" countries is that fluctuations in world demand and supply conditions over which individual countries have little or no control, affect the prices and export volume of primary commodities. Nevertheless, each primary commodity which is subject to marked fluctuations in trade has its own pattern of fluctuations. In some cases fluctuations arise very largely from the demand side. This is the case with industrial raw materials that are greatly affected even by moderate movements in business conditions in the industrialized countries. In other cases, such as coffee and cocoa, relatively long-run supply cycles resulting from the fact of long gestation periods of new plantings, which may have been engendered by high prices for two or three years but do not affect the market supply until six or seven years later, are responsible for a great variety of fluctuation patterns. Thus, depending upon the major primary commodities exported by the individual country, the pattern of fluctuation in export proceeds and hence the problem of stabilizing them differs markedly. Moreover, as has already been pointed out, the changes in the volume of export proceeds for many commodities and/or countries is not simply a matter of world price and demand movements over which they have no control, but is in considerable degree a function of national policies. Thus, if people in Argentina consume large amounts of their excellent beef every year while at the same time national policies are not designed to expand the total output of beef, their exports are bound to fall irrespective of what happens to the world demand for beef. While it is possible, or at least conceivable, to devise a scheme for reducing fluctuations in prices or even export proceeds, after eliminating long-term trends, for certain individual commodities, it is not possible to apply this concept of stabilization to a compensation program which lumps together total export proceeds from all commodities which have markedly differing trends and short-term fluctuation patterns. There is simply no basis for determining a trend for export proceeds which would be applicable to all commodities and to all countries. Moreover, any formula adopted for the so-called "stabilization" of export proceeds may be rather irrelevant to the problems facing a particular country. For example, let us assume that the normal cyclical pattern for a given commodity, say coffee, is one in which there is a seven-year period from the peak to the trough and the long-run trend is downward, while for another commodity, say copper, the cyclical period is three years and there is no observable long-term trend. The formula for meeting the stabilization problem of the country whose principal export is coffee is quite different from that of the country whose principal export is copper. Therefore, if stabilization of export proceeds is the objective, a scheme for stabilizing export proceeds from single commodities is more relevant than a general export proceeds stabilization scheme.

We may conclude that a total export proceeds compensation scheme which provides for a formula common to all primary exporting countries is rather far removed from the traditional concept of stabilization or the ironing out of fluctuations in prices or proceeds above and below long-term trends. It is perhaps for this reason that the proposal of the U. N. Experts is referred to as an insurance scheme, and what primary exporting countries are insured against is not fluctuations in prices or proceeds in relation to a trend, however calculated, but against a decline in export proceeds in accordance with a highly arbitrary formula; namely, one based on a short fall from an average of the previous three years.

Significance of the U.N. Experts' Formula for a Shortfall in Export Proceeds

Not only is the suggested formula of the U. N. Experts' committee for the compensation of shortfalls in exports rather arbitrary and unrelated to the concept of stabilization, but it may have little relevance for the problem of a particular country which experiences a sudden decline in its export proceeds. Assuming the operation of a compensation scheme for shortfalls in exports from a three-year moving average, let us suppose that for a given country in year One export proceeds are 100, in year Two, 115, and in year Three, 125, giving us an average for the three years of 113. But in the fourth year, this country's exports fall from 125 back to 110. Assuming a minimum reduction from the three-year moving average of 5 per cent before any compensation is paid, this particular country would realize no compensation from a nearly 15 per cent decline in export proceeds. On the other hand, if the country's export proceeds had remained reasonably stable over the previous three-year period before the decline, a 15 per cent decrease in export proceeds would have entitled the country to a compensation equivalent to 10 per cent of its total exports, or roughly two-thirds of the decline, assuming a 5 per cent minimum shortfall.

We are certainly entitled to ask whether the basic problem of fluctuations in export proceeds is best dealt with by compensations for annual shortfalls from the previous year, from a three-year moving average, from a five-year moving average, or on the basis of a weighted average of deviations of prices of export commodities from long-term price trends of the individual commodities exported by the particular country. Once we get away from the basic idea of stabilization, the most appropriate means of compensating countries for shortfalls in their total exports can only be determined in the light of the problem of adjustment for the particular country or, in other words, from the standpoint of the balance of payments as a whole. For example, the most useful formula at a particular time for an individual country might be compensation for shortfalls in export proceeds from the previous year.

Automatic Versus Managed Programs of Assistance for Shortfalls in Export Proceeds

The compensation or so-called "insurance" fund approach to the problem of fluctuations in commodity trade shifts the emphasis from the commodity trade problem as such, to the problem of the balance of payments as a whole. Because of my misgivings regarding the effectiveness of individual commodity stabilization programs, I am inclined to favor the balance-of-payments approach. However, once the problem is clearly conceived of as a balance-of-payments problem, I believe the solution should be based on principles and considerations which take into account the balance of payments as a whole and not simply one aspect of it; namely, fluctuations in world trade in primary commodities. In this connection it is worth noting that in recent years the balance-of-payments difficulties of most low-income countries have been caused by either a rise in imports or capital flight, or a combination of both. In many cases, substantial increases in imports have been financed by large supplier credits and a crisis occurs when a large volume of these credits becomes due over a relatively short period of time. In 1959, 1960, and 1961, there was a rise each year in total export proceeds of primary producing countries; yet balance-of-payments difficulties occurred with considerable frequency. For example, between 1959 and 1960 there was an increase in export receipts of primary exporting countries of about $1.6 billion but there occurred a deterioration of the trade balance of these same countries of $1.7 billion [10, pages 170, 186-88]. Moreover, sixty out of a total eighty-seven countries listed as primary exporting countries increased their export proceeds between 1959 and 1960. Out of this group of eighty-seven countries, sixty-five experienced a deterioration of their trade balance and in the vast majority of cases this deterioration was entirely or mainly a result of increased imports.

The balance-of-payments approach raises several questions, including (1) whether balance-of-payments assistance for shortfalls in export proceeds should be provided on an automatic formula basis or under a managed loan program; (2) whether the problem can be handled by the International Monetary Fund, perhaps in co-operation with other existing international assistance agencies; and (3) whether there is a need for new national and international approaches to the problem of fluctuating or stagnating export proceeds of less developed countries, which are more directly related to the promotion of economic growth and development.

Obviously, all three of these questions are very closely related. The question of automatic access to the IMF by members experiencing balance-of-payments deficits has a long history beginning with the discussions prior to the Bretton Woods Conference of 1944. To a considerable degree, automatic compensation in whole or in part for shortfalls in export proceeds involves some of the same considerations which characterized the controversy with respect to the operations of the IMF. Those who favor a managed as opposed to an automatic fund have taken the position that forces responsible for balance-of-payments difficulties, whether from the payments or the receipts side, are not necessarily beyond the control of the deficit country nor are they always automatically reversible, and that a country receiving assistance should be required to adopt appropriate policies and measures directed toward a correction of its balance-of-payments deficit as a condition for receiving assistance. Although export proceeds compensation schemes do not deal with balance-of-payments deficits arising directly from increased imports or capital outflow, the various elements which determine the balance of payments are closely interrelated, and if capital funds are made available to a country, they should not be simply for the purpose of maintaining a given level of consumption but should make a contribution both to the economic growth of a country and to the correction of the forces which have given rise to its balance-of-payments difficulties. To my mind, at least, this would seem to call for a program of managed assistance as against automatic compensation for shortfalls.

The Role of the IMF

We now come to the question of whether balance-of-payments deficits arising out of fluctuating export proceeds of primary producing countries can be handled by the IMF or whether it is necessary to set up a specialized institution. Two arguments have been made against leaving the job to the IMF. First, it has been pointed out that IMF assistance to the less developed countries has fallen far short of the amount of the aggregate decreases in export proceeds of primary exporting countries during certain periods, and, second, that the Articles of Agreement of the IMF preclude the Fund from providing the kind of compensation for shortfalls in export proceeds which is believed to be essential. As to the first objection, it may very well be that in times past the IMF has been too conservative in providing assistance for fluctuations in export proceeds of less developed countries. If the IMF quotas for certain countries are too small to permit drawings commensurate with the amplitude of their fluctuations in exchange earnings, the quotas should be adjusted, or above-quota drawings can be authorized by the IMF. On the other hand, there is little evidence that the IMF lacks the resources for dealing with the problem of fluctuating export proceeds of the less developed countries, especially since the general increase in IMF quotas in 1959 and the more recent arrangements for borrowing from the major creditor countries to deal with special balance-of-payments problems of large industrialized countries. For example, in 1961 drawings from the Fund by so-called "primary producing" countries totaled $903 million while repayments by these countries totaled $300 million, thus providing net benefits of over $600 million. This sum exceeds by a considerable margin the average annual net benefits for low-income countries contemplated under the U. N. Experts' compensation scheme based on the experience of the postwar period, even under the most generous formula for determining benefits which has been suggested.

The second objection to relying on the IMF for providing compensation for shortfalls in export proceeds relates to the automatic nature of the proposed compensation schemes, and also to the feature of the U. N. Experts' proposal which provides that repayments for compensations for shortfalls would never be required of the recipient countries unless there was a rise in the recipient country's proceeds in accordance with the formula. In fact, under one version of the U. N. Experts' scheme, compensations would take the form of grants rather than of loans and hence compensations would not be repaid since they would be regarded as insurance benefits. The Articles of Agreement of the IMF require repayment of drawings as a country's reserve position improves, and, in addition, that countries shall not remain indefinitely in debt to the Fund. In other words, the purpose of the Fund's assistance is to provide short- or intermediate-term financing for reversible deficits in the country's balance of payments. It might well be argued that this same principle should be followed by any compensation scheme, if indeed the fundamental purpose of the compensation scheme is to deal with reversible fluctuations in export proceeds from primary products. This is, in fact, the principle followed by the OAS Experts' proposal.

This brings us to the question of whether compensations for export proceeds shortfalls which are not reversed within a reasonable period of time should take the form of long-term loans or grants. In my own view, long-term loans or grants may well be desirable, and indeed necessary, to enable a country to make the structural adjustments for dealing with the problem of declining export proceeds. Where I would disagree with the proponents of certain types of compensation schemes, however, is that such capital assistance should be made available on an automatic basis and without regard to how these funds are employed. Hence, what I am saying is that so far as truly reversible fluctuations in export proceeds are concerned and taking into account the factors affecting the balance of payments as a whole, both the resources and the rules of the IMF are quite adequate to deal with the problem. But the IMF is not in a position to provide long-term capital loans or grants for assisting countries in making structural adjustments. This is a job for other institutions such as the World Bank and its subsidiary, the International Development Association, and AID.

Contribution to Economic Development

There is certainly no doubt that instability of export earnings can have an adverse effect upon the level of investment and upon planning for economic development. However, I am of the opinion that short-term fluctuations in export earnings can and should be dealt with as a balance-of-payments problem by the International Monetary Fund. I would say further that the Monetary Fund might consider a revision of its regulations which would provide for more liberal drawings on the part of members, both for meeting temporary shortfalls in export proceeds arising from changes in international commodity market conditions and even for sharp declines in export proceeds which appear to call for more permanent adjustments in a country's balance-of-payments structure. However, I am convinced that the most serious problem arising out of international commodity trade which faces most less developed countries is not that of reversible fluctuations, but rather long-run declining or stagnating export proceeds, or a slow rate of growth in export proceeds relative to the country's foreign exchange requirements for imports and debt service necessary for sustained growth. These are structural problems which concern the volume and direction of investment in developing countries, and they cannot be dealt with simply by compensating for all or a portion of the shortfalls in export proceeds in accordance with some automatic formula. Rather than providing income transfers under automatic compensation schemes, such as that contemplated by the U. N. Experts' report, I would far rather see the same or larger funds made available by international development financing institutions expressly for the purpose of broadening the export base and in other ways of improving the export capacity of low-income countries.

Projections of the world demand for primary products from the low-income countries over the next couple of decades indicate low rates of growth in export proceeds relative to their foreign exchange requirements, especially for countries whose principal exports consist of tropical foods, beverages, and fibers [11]. Yet, far too little attention is being paid to this problem, either in the national economic plans of the individual countries or by the various development assistance organizations [12]. In fact, one of the dangers of concentrating on schemes for dealing with shortfalls in over-all export proceeds or for maintaining the prices of individual commodities such as coffee and cocoa is that the longer run problem of changing the entire export base of the developing countries will not receive adequate attention. Most developing countries cannot continue to grow without broadening their export base to include not only additional primary products but also an increasing volume of manufactures. This is, of course, not simply a problem of the direction of investment and development policies, but also involves world-trade policies [11] [13].

There have been some suggestions that instead of making automatic compensation payments for export shortfalls, these payments should be made in the form of loans for development projects so that they would contribute directly to economic development. One difficulty with this approach is that development assistance in the form of project loans cannot be turned on and off like water from a spigot. It takes time to prepare projects suitable for international financing and time for the negotiation of loans. More and more we are seeing that development assistance agencies need to look beyond specific projects and involve themselves in long-run development plans for the purpose of providing a continuous flow of assistance in accordance with priorities, timing, and complementarity of projects [14]. However, if international development assistance organizations are maintaining a close and continuing relationship to the development process of a country, including both externally financed investments and those financed from internal sources, such institutions might be able to move rather quickly in making up shortfalls in investment programs necessitated by a decline in export proceeds. Thus, for example, institutions such as the International Development Association or AID might be prepared to provide supplementary financing for investment projects which were in danger of being delayed or eliminated by sudden shortfalls in export proceeds. This approach might well be institutionalized by the creation of a special committee consisting of representatives from the IMF and the various development financing institutions which would consider applications for special development assistance from countries experiencing shortfalls in export proceeds which endangered their investment programs. However, in providing assistance in such cases, it would be highly desirable for both the applying countries and the assistance institutions to give special emphasis to projects which would make the countries less vulnerable to such shortfalls.

Finally, when we come to consider individual commodity agreements, such as, for example, the International Coffee Agreement, it is surely a mistake to assist producing countries either by helping them to enforce their export quotas or by more direct financial assistance, without insisting that the schemes provide for long-run structural adjustments [15]. If the American consumer is asked to pay more for his coffee or his cocoa because of an import quota established by the United States government, he certainly ought to know why he is being subjected to this indirect tax on his consumption. And, if this tax simply perpetuates the existing situation in the producing countries without making a contribution to their development, including a reallocation of resources in a manner which would both limit production of the primary commodity in relation to the growth of demand and increase productivity in the industry, he ought to write a letter of protest to his Congressman. If the United States becomes a party to a new international commodity agreement, it should be one which meets the test of a defensible international commodity accord set forth in a recent study by Dr. Boris Swerling [16, page 237]; namely, "promotion of commercial marketings, definite incentives to reallocate productive resources, and mechanisms for gradually superseding the basic agreement."

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DOMESTIC CONSEQUENCES OF EXPORT INSTABILITY

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At a time when general discussions of export instability abound, in terms of its nature, cause, effect, and possible correctives, it may be of interest to take an empirical look at the case of one country for the insights which it will provide. The example of Chile and its copper industry between 1925 and 1959 is presented to reveal some key relationships between stability of export earnings and economic growth as seen through the structural ties between the rest of the economy and its major industry.

The problem to be analyzed is the relationship between fluctuations in export earnings and the stability and growth of the export economy. Fluctuations are confined to short-run (one year) phenomena which are determined primarily by shifts in demand. If changes in the value of exports affect growth, it is mainly through their unstabilizing effect on the capacity to import and domestic demand of the export economy. Therefore the key to the relationship between export instability and economic growth lies in the relationship between the fluctuating value of export earnings and the changing level and composition of its domestic share.

This paper endeavors to show how such a relationship may be analyzed in order to determine the effect of instability in export earnings in the economy. To do so, it traces the following reaction path between the change in the value of export earnings and the change in the level of income: (1) change in the value of export earnings, (2) change in the value of export earnings retained in the economy, (3) change in the value of domestic expenditures for inventory accumulation in the export sector, (4) change in the distributive shares in the export industry, (5) change in the value of local purchases on capital account by the export sector, (6) change in the value of domestic foreign exchange earnings derived from the export sector (arising from 1 to 5 above), (7) change in domestic income, (8) implications of the foregoing for economic growth.

The results of this analysis indicate that stabilization policy which deals only with compensation for changes in the value of exports will tend to destabilize the system unless very special conditions hold for relationships 2 through 6 above. Stabilization policy, it will be shown, must first isolate the differential effects of changes in the value of exports on different income recipients. Some of these, including the government, may have greater destablizing effects on income than others. For policy to be effective, it must respond to different sectoral impacts of export fluctuations to insure maximum growth with stability. Therefore so-called "automatic" financing schemes designed to compensate countries for shortfalls in export earnings, if they are truly to promote stability and growth, cannot afford to be automatic either in terms of amount of financing or conditions under which financing would be made available.[1] In the process of reviewing the Chilean case the argument upon which these conclusions rest will become clear.

The Chilean Case 1925-59

Chile is a well-known example of an export economy, depending upon the proceeds of its copper industry for over 50 per cent of its foreign exchange and 8 per cent of its gross geographic product. Data on the value of copper exports may be disaggregated into the component retained in Chile and the share expatriated in the form of purchases of intermediate goods and services, profits and depreciation. Figures on local expenditures in the export sector for creation of stocks and capital formation are also available owing to recent research.[2] Without going into a detailed discussion of the industry and its impact on Chilean stability and growth, some relevant magnitudes will be presented to illustrate points mentioned above.

Copper is an export industry of fairly inelastic supply, due to the high ratio of fixed to variable cost and remoteness from population centers which tend to fix factor supplies in the short run. This makes the chosen example representative of a class of export economies for which analysis and policy must focus upon certain rigidities in the factor markets. Other export economies undoubtedly have relatively elastic supply functions in the short run due to availability of alternative employment for factors of production (e.g., the case of Indonesia where labor may shift between the production of rubber and rice).[3] Furthermore, large-scale copper mining in Chile is entirely foreign owned, permitting us to assume that the entire share of profits and depreciation in the value of export earnings is remitted abroad while local gross investment expenditures of the industry are treated as exogenous. Some of the implications of relaxing these special conditions will be examined below.

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[1] Automatic financing schemes to compensate for fluctuations in export earnings have been urged among others by the Committee of Experts of the U. N., *International Compensation for Fluctuations in Commodity Trade* (U. N., New York, 1961), Chap. 4, and Joseph D. Coppock, *International Economic Instability* (New York, 1962), Chap. 7.

[2] This paper is based primarily on information presented in a doctoral dissertation submitted by the author to the University of California at Berkeley in 1962: "Development Problems of an Export Economy: The Historical and Developmental Relationship of the Copper Industry to the Economy of Chile." Research for this study including residence in Chile was financed by a Doherty Foundation Fellowship.

[3] R. Nurkse, "The Quest for a Stabilization Policy in Primary Producing Countries," *Kyklos*, Vol. XI, 1958, Fasc. 2.

Instability of Export Earnings

Chilean export earnings as measured by the value of copper sales have shown an average annual fluctuation of 22.1 per cent between 1926 and 1959.[4] This is the result of well-known instability in demand for the metal[5] plus inelasticity of supply (gross of inventory change) in the short run. Instability in export earnings affects the economy through payments to local owners of factors of production plus the government. Here the inventory policy of the export industry will play an important role in the degree of instability which is transferred into the domestic income stream from a given change in export earnings. Thus the first qualification to be made in attempting to assess the impact of a fluctuation in sales on the domestic economy is that the value of sales may not be the relevant determining variable. For that reason we use a figure representing the portion of export earnings accruing to the Chilean economy plus payment to factors of production employed in the creation of stocks.

Instability of Net Returned Value

The share of sales plus value of net inventory change retained in the economy may be termed "net returned value." This figure is net because it does not include payments to owners of local factors of production employed in capital formation by the export industry. This figure showed an annual fluctuation of 20.2 per cent during the same period—somewhat less than fluctuations in the value of sales. Moreover, in six of the thirty-four years the direction of change in returned value was of opposite sign from the value of sales. This illustrates that the instability of retained earnings in the export sector was less than the total value of sales owing to two important factors: (1) the profit share in the value of sales fluctuated more widely than any other component, and profits are not included in retained earnings because of foreign ownership; (2) the inventory policy of the export industry was essentially a stabilizing one for copper. The foregoing gains in importance when translated into absolute terms, since net returned value was less than total value of sales for every year except 1953 (during which large stocks were accumulated).

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[4] Instability is measured in terms of the change in the value of the relevant variable between the present year and the past year divided by the value of the variable which is the greatest for the two years. An average is then taken of all percentage changes over the period considered, disregarding sign, and this gives the "average degree of instability." The measure used gives a downward bias to fluctuation and does not correct for trend. Nevertheless, it is the relative amount of instability which concerns us; so the measure is adequate for this purpose, particularly when the values of all variables show proportional secular changes. The figures for all relevant series fail to show significant serial correlation; percentage changes were negative in twelve of the thirty-four years for total sales, and in fourteen of the thirty-four years for net returned value, gross returned value and government revenue from the copper industry.

[5] United Nations Department of Economic Affairs, *Instability in Export Markets of Underdeveloped Countries* (New York, 1952); R. W. Ballmer, "Copper Market Fluctuations—an Industrial Dynamics Study," M. S. dissertation, M.I.T., 1960; O. C. Herfindahl, *Copper Costs and Prices: 1870-1957* (Johns Hopkins, 1959).

The importance of inventory policy as a stabilizer of incomes of local suppliers of labor, intermediate goods, and raw materials is revealed by looking at fluctuations in local operating expenditures. This important component of returned value (taxes form the remainder) showed smaller variations averaging 15.1 per cent over the period. Since operating expenditures including wages and salaries represented from 83 per cent of returned value (1925-31) to 45 per cent (1956-59), the stabilizing influence of inventory policy cannot be overlooked.

Furthermore, in ten of the thirty-four years for which data are available, the direction of change in operating expenditures was of opposite sign from the change in the value of exports. This illustrates that fluctuations in the value of exports are unreliable as a predictor of fluctuations in domestic factor returns.

Stability of Distributive Shares in Net Returned Value

While it is not possible fully to disaggregate returned value into returns to local land, labor, and capital, it is possible to divide returned value into its tax and nontax components. In this respect, operating expenditures represent the private share of the returned value of export earnings, while the public share includes direct taxes plus import duties and miscellaneous charges. In earlier years (1925-31) the government share of returned value was only 17 per cent while operating expenditures accounted for 83 per cent of the total. In recent years, however, the balance has shifted to 55 per cent taxes and 45 per cent operating expenditures (1956-59).

Under these circumstances, the transfer of instability of export earnings to instability in domestic income depends to a large extent on the instability of government income from the export sector and resulting government expenditure policy. Total government revenue from the copper industry (measured by year of payment) fluctuated by 32.8 per cent per year over the period! Furthermore, in ten of the thirty-four years, percentage changes in government receipts were of opposite sign from changes in the value of exports. That is because income taxes constitute the bulk of copper tax revenues and tax legislation is subject to frequent change, altering the incidence of direct taxation independent of changes in the value of sales. There was also an approximate one-year lag between incidence and payment of taxation prior to 1946.

Since copper tax revenues fluctuate much more widely than operating expenditures in the export industry, one might expect to find the main source of instability in income to be the government. This would be particularly true if government expenditure policy were such that expenditures moved in parallel with taxation over the course of the export cycle or expenditures equaled tax receipts in boom years with deficits being run during declines in export earnings. For this to be true, of course, other tax receipts must remain constant or fluctuate in the same direction as copper taxes. In the first case, instability would be transmitted directly from the export sector to the domestic economy via destabilizing government expenditure policy—rising with full capacity and declining with excess capacity over the course of the trade cycle. In the second case, there would be an indirect transmission of instability via the foreign trade sector as government failed to maintain buffer funds of foreign exchange during peak years which could be drawn upon to support a sustained demand for imports during periods of decline in exports—this import demand resulting from sustained high levels of government spending through creation of a deficit.

To determine whether fluctuations in export earnings might have affected the stability of Chilean income and product via taxes and expenditure policy, let us examine (1) the relationship between fluctuations in taxation and fluctuations in gross geographic product and (2) the relationship between changes in copper revenues and changes in government expenditure. In only nine of the thirty-five years (1925-59) did Chilean gross product (measured in U.S. dollars) change in the opposite direction from changes in copper tax revenues, and in only two of these years (1927 and 1942) did copper tax receipts increase as gross product declined tending to stabilize the system. This information strongly suggests that there may be a relationship between the stability of copper revenues and Chilean income via government expenditure policy. The other relationship, between government copper revenues and government expenditure, reveals that in twenty of the thirty-one years for which estimates are available (1928-58) government expenditures moved in the same direction as copper revenues transmitting instability to the rest of the system, especially as revenues moved in the same direction as operating expenditures for all but five of those years. For eight of the other eleven years government expenditures rose while copper revenues declined.

Had the government engaged in a conscious stabilization policy, it would have attempted to offset fluctuations in domestic income created by changes in operating expenditures of the export sector by running a surplus during periods of increasing revenue and a deficit during declines (this would amount, in effect, to a "buffer fund" of copper revenues in the form of foreign exchange). Yet in only eight of the thirty-one years do the statistics suggest such a policy.

Stability of Capital Formation in the Export Sector

Nevertheless, we have still not arrived at a figure which adequately represents the impact of the export sector on the domestic economy. To do this we must add to net returned value all local purchases by the export sector on capital account. This represents the total foreign exchange made available to the system from the export industry during a given year and is termed "gross returned value." It is, therefore, important to determine whether or not capital formation in the export sector tends to further destabilize the system.

The relationship between capital expenditures in the export sector (exclusive of capital goods imports) and value of exports depends upon the role of profits and sales in capital formation, including relevant lags. It is recognized that in many cases lags may be influenced by expectations which are directly related to political factors. This has been particularly true in the Chilean case in recent years.

In case where investment in the export industry is a function of profits and/or sales (the latter showed fair results in the Chilean case), one would expect capital expenditures to increase instability in income resulting from fluctuations in sales. This would be particularly true if local expenditures on capital account were a stable proportion of total investment expenditures in the export industry (a case which does not hold for the Chilean copper industry).

Nevertheless, fluctuations in gross returned value did not significantly exceed those of net returned value in our example, varying on an average of only 20.9 per cent per year. Moreover, in only two of the thirty-four years did gross returned value fluctuate in a different direction from net returned value (1927 and 1928). We see, then, that capital expenditures did not serve to further destabilize over-all domestic demand. Neither did they operate to increase stability. This is partly because local capital expenditures represented only a small percentage of net returned value (6.9 per cent between 1950 and 1959). One must not disregard the role of capital formation in the export sector, however, when weighing its potential unstabilizing effect on income. As economies develop, increasing their producers' goods production, the share of local expenditure in total capital formation of the export industry will tend to rise, increasing the effect of this factor on stability and growth.

Stability of Foreign Exchange Receipts

The best measure of foreign exchange provided by the export sector is not the value of sales or net returned value but gross returned value. This has been shown to depend upon sales plus inventory change plus local capital expenditures. It has been shown that gross returned value varies with net returned value in the Chilean case, but that the latter does not closely follow the value of sales because of stabilizing inventory policy. In cases in which governments desire to stabilize the foreign exchange available to importers through the creation of buffer funds during the boom periods which may be drawn upon during downswings in export earnings, the relevant variable should be gross returned value—and government revenues should be set aside to offset swings in this variable. Indeed, the government may even attempt to encourage stabilizing investment policy in the export sector for the foreign exchange which it would provide by encouraging capital formation during downswings in export demand. It should be noted here that the important variable is domestic expenditure on capital account, which could include construction of new housing for workers and other social overhead facilities which have a low import coefficient and employ labor displaced by cutbacks in production.[6]

Stabilization and Growth

The question of whether or not a stable domestic income stream and stable imports will produce an optimal rate of growth is yet unsettled. The answer depends upon the effect of instability in domestic and foreign demand on investment by the export industry, other industry and agriculture, and the government, and the effect of instability in the capacity to import on the price level, import requirements for growth, and the ability of the government and business to engage in long-range development plans which require a steady rate of capital formation.

In the export sector, some feel that a steady income stream will increase the share of income devoted to productive capital formation; others argue that short periods of very high profits will induce a greater share of income invested and that downswings will not cause this share to fall proportionally.[7] This would imply a ratchet mechanism which would insure a higher long-run rate of investment for the economy with cyclical behavior in its export sector than for a stable system. It is not possible to argue this point in terms of the Chilean case, since there has not been a period of stable export earnings of sufficient duration to draw any conclusions. It is, however, possible to point out that a high rate of reinvestment of profits from the sector has taken place consistent with very wide fluctuations in profits and sales. The largest investments were commenced during periods of rising profits and favorable market outlooks (1911-13, 1926-27, 1941, 1948-52, 1955-56). This implies that investors in the export industry regard upswings as beginnings of trends rather than as temporary phenomena and behave accordingly; downswings, on the other hand, are apparently considered short-run phenomena, since no major disinvestment occurred for any of the companies even during the severe depression of the thirties.[8] The result of this behavior is that net investment in the Chilean copper industry has represented 52 per cent of net profits between 1930 and 1959.

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[6] It is important to note here that foreign exchange earnings should be deflated by an index of import prices to provide a capacity to import measure similar to the "income terms of trade." For a stable import stream, the buffer fund should in fact be related to fluctuations in this variable since it more accurately represents real purchasing power of export earnings.

[7] Sir Sidney Caine, "Instability of Primary Product Prices: A Protest and a Proposal," *Econ. J.*, Sept., 1954, and comments, *Kyklos*, 1958, *op. cit.*

In the Chilean case there is no clear relationship between the stability of export earnings and growth. Although these did fluctuate widely between 1925 and 1959 and although the rate of growth of per capita income was very modest (cumulative annual rate of 1 per cent or less), this was perhaps less the result of instability in export earnings than of failure by the government to invest a steady or growing share of copper revenues in productive capital formation. Although copper revenues rose by 255 per cent over the period 1940-59, direct and indirect government investment expenditures increased by only 116 per cent. Furthermore, the share of total government revenue devoted to investment fell from 21 per cent in 1940 to 17 per cent in 1957. Annual government support of its major development institution, the Corporacion de Fomento de la Produccion (CORFO), only increased by 28 per cent in real terms between 1940 and 1958.

Policy Implications of the Chilean Case

One learns from the Chilean case that if the goal of commodity stabilization policy is domestic stability and maximum growth, it is neither appropriate nor sufficient to stabilize the value of export earnings (regardless of the difficulty of such an undertaking). That is because the net returned value and gross returned value derived from the export sector, which have a direct effect on domestic stability, do not necessarily depend directly upon the value of export earnings. Moreover, fluctuations are transferred to the rest of the economy through changes in revenue accompanied by parallel changes in government expenditure (destabilizing domestic demand) or steady expenditure equal to peak year revenues (creating balance-of-payments problems and tending to destabilize real income).

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[8] During the period 1932-37, the average annual net investment was minus $1 million. Since the depreciated value of assets approximated $235 million during that period, this is not considered major disinvestment.

It is suggested that it is this kind of behavior by government which introduces instability into the export economy, particularly where profits are remitted abroad either because the industry is foreign owned or because domestic owners have a high marginal propensity to import.[9] The destabilizing behavior of the government sector may well result from political pressures of powerful interest groups which have no direct concern for domestic stability or growth. Under these circumstances the government may be powerless by itself to engage in the type of stabilization policy necessary to achieve these ends. It is here that international stabilization policy may operate in conjunction with the government to permit compensatory fiscal policy in the following manner: (1) an international lending agency (such as the IMF) could provide funds during the decline in export tax revenues equal to a percentage of the deviation around the long-run trend, (2) the agency would require an agreement by the government that the share of government investment expenditures not be cut during a decline in export revenues, (3) the international agency would obtain an agreement from the government to repay its borrowings during an upswing in export revenues. The proposal would, in effect, permit the government to engage in the kind of compensatory fiscal policy which is most consistent with stability and growth while transferring the domestic political pressure against such policy from the government to the international agency.

This proposal differs from current policy of the IMF in that (1) it is designed to meet the problem of instability at its source (fluctuation in government revenue and expenditure) rather than at the point of decline in over-all export earnings or when balance-of-payments deficits arise, (2) it is designed to be used as a tool of the government to transfer political responsibility for stabilization policy from itself to the international lending agency, (3) it is to be used explicitly as a tool of economic development as well as stabilization by permitting the government to engage in a steady rate of capital formation.

Coupled with this policy, which deals with instability produced by fluctuations in government spending, could be a policy to cope with instability arising from private payments in the export sector. Here financing could be made available to permit the export industry which has inelastic factor supplies to engage in the formation of buffer stocks in cases in which this policy is not already being undertaken privately.

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[9] For a similar example of destabilizing government expenditure policy in Malaya, see Sir Sidney Caine references, *op. cit.*

Conclusion

Instability is primarily transferred from export earnings to the export economy in the Chilean case by way of destabilizing tax and expenditure policy of the government. The relative destabilizing effect of government tax and expenditure policy in the export economy is likely to increase as one or more of the following factors gain in importance: (1) the export industry is foreign owned, (2) the share of profits of the export industry which are expatriated rises, (3) the profits share of the export sector grows, (4) the government shifts from indirect to direct taxation of the export industry, (5) the rate and degree of progressiveness of direct taxation rises in the export industry, (6) the elasticity of substitution between factors employed by the export sector and factors employed by the rest of the economy declines, (7) the natural resource intensity of the export industry rises, (8) government expenditure tends to equal government revenue, (9) government investment expenditure is a fixed share of government revenue from the export industry, (10) the local expenditures share of export industry investment declines, (11) the local investment expenditures of the export industry show a greater lag as a function of profits and/or sales.

Whenever the main source of instability is traced to the government sector, international stabilization policy should be directed toward the encouragement of compensatory fiscal policy related to fluctuations in export revenues and designed to insure not only stability but growth. Here the international agency should be consciously designed to bear the responsibility for such policy when it engenders political friction.

DISCUSSION

ROBERT Z. ALIBER: Much of the motivation of recent proposals for international commodity arrangements has been to provide for international transfer of income from industrial countries to low-income, primary producing countries. Such transfers raise inevitable problems of allocation of income, not only between extending and receiving countries, but also among the various receiving countries. One of the central policy issues is whether to make these transfers through commodity arrangements or by alternative means.

The authors of the several papers stress some of the difficulties inherent in some of these proposed commodity arrangements: Mikesell has given us an impressive list of technical difficulties that might be encountered in their operation; Reynolds has suggested that it may be very difficult to stabilize both export proceeds and foreign exchange receipts at the same time; and Swerling has indicated that the historical experience does not suggest that the agreements in the past have alleviated the underlying problems.

Even if these technical difficulties could be surmounted or were judged inconsequential, it is not obvious that these proposed commodity arrangements, or even arrangements less subject to technical difficulties, would be the most advantageous way to assist the low-income countries. Transferring income and allocating resources in accordance with formulae based on previous market performance or the guesses of the experts may not provide the best set of answers as to who produces what primary products, in which low-income countries, and how much they are paid.

The central issue is whether the most beneficial method for transferring aid or assistance from the richer, more developed countries to the poor, less developed countries, judged both in terms of the volume of aid and its effectiveness, is likely to be provided under the formula approach of a commodity arrangement, or under bilateral and multilateral aid programs. The need for an assured minimum flow of foreign exchange toward the less developed countries to assist in the forward planning for development appears pressing. A commodity agreement is one way of steadying the flow of foreign exchange. An alternative is for a direct flow of development assistance under a set of commitments or obligations which also provide these countries with some assurance of continuity. A perfect commodity scheme which meets all of Mikesell's objections might still be judged inferior to a direct assistance, or imperfect commodity arrangements might be better than the direct approach.

If a choice must be made between these broad alternatives, then some basis is necessary for selecting where policy emphasis should be placed. Several possible bases for choice are mentioned below, less in an attempt to be exhaustive than to point toward the types of considerations which might affect the choice. These include the merits of strings on direct assistance, the differences in the size of the aggregate volume of financial assistance from developed countries, and the role of the private sector.

Commodity programs are attractive for those who favor automaticity and who wish to avoid, by reference to a formula, the continually complicated and trying decisions otherwise associated with international transfers of income. But selectivity is also a virtue—for distinguishing among the low-income countries on the basis of the shortfall of their export price or export proceeds below some previous norm, for encouraging the shift from one-crop agriculture into other types of primary production as well as into industrial undertakings, for providing the stimuli toward more effective systems of taxes and more effective tax collections, etc.

This choice, therefore, is related to the use of strings on the transfer of funds under the direct assistance alternative. The developed, assistance-giving countries and the established international aid agencies have their own set of rules of how best to facilitate development; price stability is good, high foreign debt ratios are bad, etc. But the economic planners in the recipient countries face a different set of problems, for they are ultimately responsible to their own electorates for their tenure in office. Indeed they are caught between the constituency in the rich countries which has the money, and their domestic constituency, which has the votes. Are the finance ministers more likely to provide better ways of using the monies than those in Washington, are Washington's rules too harsh and self-defeating, or does Washington really know best. If it is clear that Washington knows the appropriate measures for development better than do planners in their own countries, and that Washington's adherence to established truths is necessary to give the planners more effective power in their own countries, then the case for commodity agreements appears weak.

It may be that it would be difficult, if not impossible, to generate as large a volume of financial transfers to low-income countries through direct assistance programs as through commodity agreements. Commodity agreements, after all, both the domestic and the international varieties, provide a way of indirectly taxing U.S. and European consumers through higher prices. Only Great Britain, as Swerling notes with considerable approval, appears to give direct income subsidies to the farmers; every other government appears to engage in the deceptive effort to keep its own direct financial burden small by subjecting consumers to artificially high prices. The U.S. government—and perhaps to a greater extent governments in the industrial countries of Europe—appear less willing to tax their citizens directly as a means of providing a comparable amount of assistance to less developed countries on a direct basis than through a commodity agreement. To the extent this is true and a larger volume of assistance is deemed more useful than a smaller volume, the case for commodity agreements is stronger.

The third consideration centers on the role of the private sector. There appears to be general agreement that a large, thriving, private sector encourages development. If this is the case, then it would be helpful to know whether furnishing assistance through commodity arrangement or through direct transfers best strengthens the private sector. The greater the favorable impact through commodity agreements the stronger the case for commodity agreements.

The important point is that the case for commodity agreements does not stand or fall on the measure of their nearness to perfection. Instead, the central issue is how commodity agreements compare with alternative arrangements for the effective channeling of financial assistance from the more advanced countries to the less developed countries. Such a comparison should be based on a variety of criteria, including the volume of assistance and the effectiveness of strings in economic assistance. The papers presented here, by emphasizing the difficulties that proposed commodity arrangements might encounter, slight the central issue.

CHARLES P. KINDLEBERGER: Professor Mikesell has informed us of many aspects of international price commodity stabilization schemes as they affect the export problems of developing countries, but I must confess I am still left wanting to have the answers to the question put to him by his friends in Latin America last spring: "Why don't the United States government and the international assistance agencies concentrate their efforts on stabilizing the prices of our exports?" The purpose of his paper was to suggest a basis for an answer to this question, but the answer itself was not forthcoming. I suggest that the answer is essentially that we, as economists, or they, the United States government and the international agencies, do not know how. We lack techniques for determining and applying the equilibrium price in commodity markets. Intervention in the price-formation process then leads directly to the disequilibrium system in which incentives are created to undermine the pegged price, stocks pile up (or shortages develop), and ultimately the support price collapses or explodes.

This is not exactly Professor Mikesell's answer in his statesmanlike paper. He says he is not against international commodity agreements, though he cogently states the reasons that these agreements have failed. He opposes insurance schemes to compensate for swings in national terms of trade on the ground that they are a disguised form of intergovernmental aid. He favors a balance-of-payments approach to swings in capacity to import of developing countries affected by cyclical movements in commodity terms of trade. In short, he is for aid—not fuzzy trade—but aid resting on a discretionary basis operated by administrators who view the whole economy rather than on the basis of some automatic procedure which spills out foreign exchange when two curves intersect.

Since I am not disposed to cavil at the broad lines of Professor Mikesell's approach, let me turn my attention to a suggestion implicit in his statement that the problem of stabilization is soluble in economic terms. The only informed, independent, and presumably benevolent manager, but that they cannot be carried out by present administrative procedures for arriving at national quotas by international negotiation. I think I detect in this passage the view that the problem of stabilization is soluable in economic terms. The only difficulty is that the answer gets corrupted by politics. If this implication is in fact present, permit me to enter a dissent.

There may be something to the proposition that a world monopoly, like the International Nickel Company or the De Beers diamond syndicate, is able to fix prices better than a consortium of national states, but the reason, in my judgment, is unrelated to the administrative forms. To maximize a single variable is easier than to reconcile the conflicting interests of a number of separate entities. Monopoly produces a unique solution in contrast to oligopoly. It can happen that oligopolists decide that their interests are identical with those of a dominant firm. It is suggestive that the De Beers diamond interests are a syndicate and not a company and that the De Beers syndicate markets diamonds for the Soviet Union. But in oligopoly under dynamic conditions there will be a necessary change in the relative positions of the various participants. A change in favor of the one party will adversely affect others, but no change at all, when underlying conditions of supply and demand have altered, will also result in adverse effects on some. To the extent that the benevolent and omnipotent manager of say a buffer stock provided nothing more than a more smoothly working market mechanism, forecasting correctly the long-run equilibrium price and trading in open markets to stabilize them at this level without the use of export or import quotas or production controls, Mikesell may be right. But any of the normal controls which accompany commodity stabilization agreements introduce elements of conflict between economic interests. The problem of reconciliation is not merely political. The omnipotent manager is in politico-economics whether he likes it or not, concerned with propping up the senior high-cost producers and restraining the chiseling new entrants who want a larger share of the market, if not troubled by the usual problem of trying to enforce stabilization rather than engineer price increases.

It might be that the countries concerned took such a long-run view of their economic interests that the new producers would hold back and the old ones give ground readily for the benefit of the total producer group. They might even themselves attempt to approximate the long-run equilibrium price in the belief that this both limited new entry and reduced consumer incentive to explore substitutes produced under competitive conditions. But these standards of sophistication and restraint seem beyond the reach of most developing countries. The temptation to maximize in the short run is overwhelming. The price gets pushed up. Long-run elasticities of supply and demand bring in new competitors and drive out old customers. The inability of governments to behave like a market of competitors may be set down as a political phenomenon; its essence seems to me economic.

I judge then, that we don't stabilize prices because we don't know how, just as we keep on giving foreign aid because we don't know how to stop. There are numerous signs—in coffee, wheat, sugar, and tin, and perhaps in the pressures inevitably arising from the U.N. Conference of Trade called for next September—that we will start stabilizing world commodity prices, even though we don't know how, exactly as we have done in domestic agricultural produce, on the same intellectual base.

One must of course recognize the political elements in commodity price stabilization, at home and abroad. The question, however, is whether economists acting as czars could improve on the system of open markets and open prices, imperfect as we recognize them to be. If Mikesell thinks so, I would be interested to know it. On the record known to me in forecasting of terms of trade, balances of payments, stock markets, business conditions, and many other variables, the evidence is less than convincing.

GERTRUD LOVASY: I shall not attempt to take up the wide range of commodity problems raised in Mr. Swerling's comprehensive survey. I want to address myself briefly to one aspect; namely, the choice between stabilization measures—commodity agreements on one side and compensatory financing on the other. What I have in mind when using the term stabilization is the smoothening of short-run fluctuations around the longer run trend.

Mr. Swerling has pointed to the poor performance of existing international commodity agreements; in spite of the fact that their structure, compared to those in the interwar period, has improved through the inclusion of importing and consuming countries, none of them can be considered a success. Government interference on the domestic market, which hampered operations of the agreements, structural maladjustment, as well as political factors have been mainly responsible for the failure of these agreements to achieve their purposes. Mr. Swerling, therefore, is joining those who look for alternative instruments; compensatory financing appears most promising. Various proposals have been made, visualizing compensation in the form of either loans or grants or a mixture of the two. These proposals are under active discussion but have not been applied so far. As Mr. Swerling points out, compensatory financing, in whatever form, would avoid interference with prices and marketing and thus would not affect structural adjustment if required. I have some doubts in that respect. Much would depend on the use recipient governments would make of compensatory loans or grants. If passed on to producers of "sick" commodities, the effect might be similar to that of commodity agreements.

In discussing the choice of stabilization measures, varying assumptions are made with respect to prevailing market conditions for primary products.

Under ideal conditions, i.e., in the absence of structural problems and of national government controls which affect international prices and trade, the choice should mainly be based on the purpose of stabilization: Is it intended to stabilize the incomes of individual producers and exporters of certain primary products or do we wish to stabilize the foreign exchange income of exporting countries?

In the first case, it seems to me that commodity agreements, preferably of the buffer-stock type or possible long-term contracts would be the obvious choice. An agreement based on international buffer stocks has the merit of merely correcting the adverse effects arising from short-run inelasticities in demand and supply for most primary products: buffer-stock operations, by supplementing market demand in periods of temporary slack and market supply in periods of temporary shortages, could go a long way in reducing sharp price fluctuations. (Such operations, in order to be effective, obviously require wise management and sufficient funds to continue purchases as long as necessary to tie over troughs; stocks would have to be large enough to permit sales as long as necessary to tie over booms.) Long-term contracts would assure minimum incomes in years of slackening demand. Either of these two devices would also maintain foreign exchange earnings for the exports of the particular commodities concerned. Export quotas, however, cannot be relied upon to maintain incomes (or exchange earnings) on a desired level, as price stabilization through quotas may be at the expense of considerable restrictions in volume exported.

The alternative of compensatory financing—in this case on an individual commodity basis—could achieve a similar result, though in a more roundabout fashion. The fact that compensatory financing would not affect price movements cannot be considered as an advantage under conditions of long-term equilibrium here assumed; there is no merit in preserving sharp price fluctuations around a satisfactory trend.

If, however, stabilization of the primary exporting country's foreign exchange earnings is the main purpose, compensatory financing seems the best method. In that case it would appear more logical to compensate for shortfalls in total exports or preferably the balance of payments as a whole rather than for declines in prices or receipts on individual commodities.

Under more realistic conditions of structural difficulties—chronic surpluses, declining price trends (e.g., coffee), or prices controlled through action of large individual exporting countries (e.g., wheat and cotton through U.S. action) —the choice of devices intended to cope with fluctuations is considerably narrowed down. Buffer stocks are not applicable; long-term contracts, though theoretically workable, have little chance of emerging, as importing countries would be reluctant to make commitments under these circumstances. Yet the pressure for stabilization measures is usually greatest in periods of structural difficulties; but the only type of commodity agreements which one could hope for under such conditions is the one based on export quotas. The severe drawbacks of this type of agreement are generally recognized and have also been referred to in Mr. Swerling's paper.

Will the alternative method of compensatory financing—either applied to individual commodities if direct relief to producers is desired, or to total exports (as visualized in the proposed schemes) if stabilization of exchange earnings is the purpose—offer a workable solution? I doubt that it will.

If it takes the form of loans which are repayable within a reasonably short period (as under the scheme proposed by the OAS experts), it would, at best, provide a temporary palliative. A declining or stagnating trend will make it difficult for producers or countries, as the case may be, to make repayments when they fall due and they may find themselves worse off than in the absence of a compensation scheme.

If compensation takes the form of partial grants (unpaid balances after a specified period to be written off), such difficulties would not arise. But, as pointed out by Mr. Swerling and others, this would simply mean that aid is being extended and allocated on the basis of export shortfalls. That raises the question whether this would be an efficient way of extending aid. Mr. Swerling has himself expressed doubts in that respect; export shortfalls can hardly be considered as a sound criterion for aid allocation, particularly under conditions where total aid that might be supplied remains rather limited.

These considerations suggest that adverse longer run trends or structural maladjustment are serious obstacles to the satisfactory working of any device intended to cope with short-run fluctuations in the trade of primary producers.

DAVID H. POLLOCK: Dr. Reynolds finds that export fluctuations inadequately indicated the direction or magnitude of domestic income changes in Chile between 1925 and 1959. Hence he urges stabilizing action upon all sectoral income paths rather than upon the external fluctuations alone. This leads him to a basic criticism of the new automatic international compensatory mechanisms which seek to dampen only the originating export shock. He concludes that unless specific national preconditions are tied to the new automatic mechanisms (especially in the field of countercyclical fiscal policies), a comprehensive approach to domestic income stabilization cannot be achieved.

His paper is one of the very few among the growing literature on the subject of export instability that disaggregates and then measures in detail the backward-to-forward income linkages for any Latin-American country (see, however, the ECLA *Economic Bulletin for Latin America,* March, 1960, Venezuelan article). By combining these two approaches, Reynolds provides a valuable methodology for quantitative analyses of, and suggesting multiple solutions to, the problem of domestic income imbalance. His emphasis upon sources rather than symptoms of instability and upon major as versus minor contributors to it, are all most pertinent and highlight the importance of integrating international with national stabilization programs.

Nonetheless, I feel that his main conclusion has been overstated. Many policy tools already exist to stabilize the domestic income stream, but the new international compensatory schemes have not been designed for that complex task. Rather their goal is a very different one; namely, to dampen down short-term trade account oscillations so as to stabilize the import capacity and hence to provide a more continuous access to international resources. This alone would substantially assist recipient countries in undertaking the longer run structural domestic measures with which Reynolds is so properly concerned. But to make certain domestic measures a strict precondition of the international proposals would needlessly postpone a solution to the limited but important problem of an erratic import capacity.

Some other and secondary points were unclear or raised some doubts in my mind. A few statistical tables would have helped considerably in interpreting certain findings. Thus in his paper the destabilizing influence of Chilean fiscal policy rests substantially upon the high proportion of copper revenues to net returned values—a ratio that is not given for the years 1931 to 1956. Similarily, the stabilizing effect of inventory changes is difficult to judge from the text. Another point is the lack of any information about the level and trend of Chile's capacity to import. Finally, Reynolds implies that the so-called "ratchet mechanism" approach to capital formation in the export sector is at least supported by the Chilean example, but his evidence is inconclusive. In any event, this thesis (that investments are magnified by windfall profits during the upward phase of an export cycle and somehow muddle through on the down side) has always struck me as doubtful—if only because of the adverse impact upon both public and private forward planning, including the encouragement of a speculative investment mentality.

These few differences are not intended to detract from Reynolds' useful paper, and I hope his "reaction-path" technique will be applied to future studies on export fluctuations and their effect upon domestic income changes. I did, however, want to distinguish between the latter and the quite different objective being sought by the automatic international proposals.

Professor Mikesell holds that existing financial institutions should continue being the principal international mechanisms utilized for action on both short- and long-term primary export problems. He concludes that very few individual commodity agreements are useful, and that automatic schemes for stabilizing total export proceeds would be undesirable and in fact "not possible" to implement. I share his general view about the single-item agreements, but not his rejection of the broader compensatory proposals.

His approach is to present a detailed list of statistical and operative difficulties surrounding the workings of the new OAS and especially the U.N. proposals. One wonders, however, whether these technical objections warranted such a lengthy presentation. Take only a few examples: determining base periods and income trends; defining primary goods or countries; and eliminating perversities in certain compensation-repayment schedules. While serious, such problems need surely not be considered insuperable. It may be impossible to satisfy all parties and criteria, but solutions have been found before on even more complex international arrangements, provided there was agreement on objectives and some urgency in the need for action. I wish, therefore, that Professor Mikesell had devoted greater coverage to such matters as (1) how seriously he views the two principal export problems facing developing countries (cyclical or shorter term instability and inadequate long-run growth), and (2) how effectively he believes existing mechanisms have in fact demonstrated a capability for resolving them. It is my view that international action is now overdue on both the problems noted in (1), and while I would place major emphasis upon the matter of secular stagnation, I do not feel that either the short- or long-run problems can be fully met through actions of the IMF, developmental lending agencies, or individual commodity schemes. These should all play a continuing and hopefully an expanded role in the future, but judging from the postwar record, additional measures are both desirable and feasible. Note, for instance, the official U.S. position that, in dealing with short-term instability of total foreign exchange earnings, automatic compensatory financing "is perhaps the principal device to be considered" and could be "a decidedly useful supplement to . . . action on individual commodities and to the assistance which the IMF gives . . ." (statement presented by Mr. Blumenthal, Deputy Assistant Secretary of State to the U.N. Commission on International Commodity Trade, Rome, May 16, 1962). It is precisely because so many economists and government officials have been urging a re-examination of standard approaches and a search for some experimental or unorthodox ones that Professor Mikesell's reliance on the *status quo* seemed disappointing. The U.N. and OAS proposals are new, intriguing, and have had long and careful scrutiny at both the technical and political levels. Whether they need additional revision is one thing: within a month, for instance, a special intergovernmental group will report on just such matters. But to reject them now because their two key features (substantial automaticity, and a formula-based system of credit transfers) diverge from the principle of managed assistance—which I take to be Mikesell's position—seems premature.

Dr. Swerling recognizes the "standing challenge to devise new and unorothodox instrumentalities that provide for international transfer of funds" and the related need to look beyond the approach of single-commodity arrangements. If additional steps are taken, however, he raises the "crucial question" of whether compensatory financing schemes should be regarded as pools of short-term revolving credits or of long-term financial assistance. This is the key point of difference between the new U.N. and OAS proposals. Thus the first provides short-term stabilization credits, but over time also effects net permanent financial transfers to primary exporting members. The second proposal conversely is designed to be self-liquidating during a normal export cycle. Which approach might be considered preferable clearly involves foreign aid considerations. Look, for example, at Latin America, where declining terms of trade between 1950-54 and 1955-60 resulted in a $7.3 billion loss of real income; i.e., almost exactly equal to its net foreign capital inflows during the same period—mostly loans requiring future repayment (speech by Dr. Prebisch, ECLA Executive Secretary, Brussels, November 21, 1962: data in 1950-54 prices). Moreover, based upon the growing number of ever gloomier commodity projections, there will undoubtedly be an increased need for long-term loans or grants to offset the discouraging trade-account outlook throughout the sixties for primary producing areas generally. Whether the amount and geographic distribution of foreign aid should in any way be linked (through compensatory financing schemes) to export shortfalls has, of course, obvious political as well as economic implications. Hence I fully agree that Swerling's "crucial question" should be given special emphasis.

CONDITIONS OF INTERNATIONAL MONETARY EQUILIBRIUM

EQUILIBRIUM UNDER FIXED EXCHANGES

By Harry G. Johnson
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I shall attempt in this paper to present the familiar analytical problems of equilibrium under fixed exchanges in a slightly novel way. I shall treat fixed exchange rates as a device for achieving on a world scale the advantages of a unified currency secured at the national level by a national currency; and I shall relate the problem of equilibrium with fixed exchanges on the one hand to the inherent problems of a unified currency and on the other to the defects of fixed exchange rates between national currencies as contrasted with a single international currency. This formulation of the problem has of course been employed by others; I am particularly indebted to a stimulating paper by R. A. Mundell recently published in the *American Economic Review* [3] and to a survey of the issues presented to the University of Chicago Money and Banking Workshop by William Poole [4]. I have attempted to extend the analysis by a more explicit recognition of the dynamic requirements of monetary equilibrium in a growing economy and of the behavior and preference systems of central banks.

The advantages of a unified currency, very briefly, are those of a measure of value and standard for deferred payments commonly used by and thoroughly familiar to everyone in the economy; a medium of exchange acceptable throughout the economy, whose use for effecting payments over distance incurs only the minimum of banking costs; and a store of value subject only to the risks of fluctuations in the general price level. These advantages are interrelated with and dependent on the freedom of competition in goods and factor markets in the economy, which tends to equalize the purchasing power of money throughout the economy. Precisely for this reason, however, participation of a particular region in the national currency area involves avoidable risks of being adversely affected by real or monetary disturbances emanating from other regions in the area.

Real disturbances include adverse shifts in demand or supply conditions elsewhere in the economy, consequent on economic growth, and the recession phases of the fluctuations characteristic of a growing economy. Their effects are avoidable to the extent that a combination of wage and price rigidity and factor immobility makes them manifest themselves in "involuntary" unemployment, which unemployment could be remedied by a general reduction of real wages and output prices brought about by devaluation or depreciation. In the modern national economy, the exclusion of local devaluation implicit in a unified currency is compensated for by the negative remedy of unemployment insurance; recently there has been a trend toward increased use of positive remedies in the form of government assistance for geographical and occupational transfers of labor and capital, directed at depressed areas and industries.

Monetary disturbances include both changes in the demand for money, of which a liquidity crisis and a spending spree are the extreme examples, and changes in the supply or rate of change of supply of money. The modern economy relies on the control of the money supply by the central bank to prevent monetary disturbances and maintain a stable monetary environment. This reliance is generally expressed in the listing of price stability as one—and in both listing and practice, usually the first—objective of monetary policy.

It may be remarked that the belief that stability of the general price level is a potent contributor to economic welfare and growth, though sedulously propagated by central banks, rests on extremely weak theoretical and empirical foundations. Theoretically, what matters for monetary stability is stability of the trend of prices; a zero trend has no special virtue. In fact, earlier generations of economists seriously debated the question of what price trend was most desirable, and generally concluded that productivity increases should be reflected in falling prices, on grounds of equity to savers. More recent theorizing suggests that this conclusion is mistaken, since any stable price trend would be incorporated in market yields on fixed interest securities; but that some falling price trend would be desirable for a different reason, to correct the distortion introduced by the fact that though the creation of money costs virtually no real resources, the holding of it involves a sacrifice of interest. The conversion of the majority of present-day economists to stability of the prices of goods rather than of factors as an objective is largely the outcome of the reluctant recognition that wages are sticky in the downward direction and that a rising average wage level stimulates mobility. There is no convincing evidence that a rate of wage increase exactly consistent with stable prices is optimal from the point of view of the other objectives of high employment and growth, and there is contrary evidence that the pursuit of price stability by central banks in recent years has had destabilizing economic effects. Furthermore, the clinching cliché of the conventional argument for price stability, that it is essential to the balancing of the balance of payments, is manifestly and demonstrably wrong [1].

Be that as it may, a region in a national currency area is dependent on the skillfulness of the central bank in managing the national monetary system and may suffer unnecessarily from inappropriately motivated or ineptly executed monetary policy. In addition, since the central bank conducts its operations in the light of the aggregate performance of the national economy, the effects on a particular region of even an ideal national monetary policy may well fall short, at certain times or on the average, of what could be attained by an independent regional monetary policy, operated with the freedom permitted by a floating exchange rate. This possibility again depends on wage and price rigidity and factor immobility, and again is partly compensated for in the modern nation by other policies, notably the provision of special credit facilities for industries or agricultural activities on which certain regions are heavily dependent, and the channeling of government funds into regional or depressed-area development projects.

The foregoing analysis is intended to demonstrate that there are disadvantages to participation in a national currency area, which disadvantages are offset in part by the advantages of a unified currency itself, but to a much more important extent by the freedom of movement of goods and factors in a common market, and the freedom to legislate and finance various remedies for economic distress, that a nation state entails. Because the currency is unified, the consequences of economic change and growth do not appear explicitly as problems of balance-of-payments disequilibrium between regions, though they are reflected in flows of money and capital between regions as well as in changes in interregional flows of goods.

A system of fixed exchange rates is intended to secure the advantages of a unified currency on a world scale. To the extent that it does so, it also involves the same sorts of disadvantages to the participating nations as does a unified national currency to the regions of the nation. But a system of fixed exchange rates differs from a unified currency in certain important respects that either reduce the purely monetary advantages or reduce the compensations for the disadvantages or involve new problems, by comparison with a unified currency.

In the first place, fixity of international exchange rates does not carry with it either the freedom of competition over the market area that is necessary to the realization of the full benefits of a unified national currency, or the use of the taxing power to assist the sufferers from economic change that compensates members of a nation for the disadvantages of participating in the national currency area. "Freedom of competition" in this context is a very broad term, embracing not only free movement of goods but free movement of factors and standardization of the laws governing competition and the taxation of income and economic activity. For various reasons to be mentioned shortly, a fixed exchange rate system tends to reinforce tendencies toward governmental interferences with the freedom of international competition, and, more important, to the use of variations in the level of barriers to international competition as a means of preserving equilibrium. So far as assistance to the sufferers is concerned, it can be argued that the postwar development of massive use of intergovernmental and intercentral bank loans to offset balance-of-payments deficits represents a rudimentary development of a sense of international responsibility; but the prevailing spirit of such arrangements is as yet more like that of the New Poor Law than of twentieth-century social legislation.

In the second place, a fixed exchange rate system does not in fact provide the same technical convenience as a unified currency. For one thing, there are presumably some costs of converting one currency into another, additional to the costs of moving money over distance. More important, the present International Monetary Fund system is not really a fixed rate system. Countries are bound only to keep their exchange rates within 1 per cent of par value. The variability of rates within these margins introduces a certain risk into international transactions, the cost-equivalent of the risk being greater the shorter the period from initiation to completion of a contract: an exporter selling for payment in three months and not hedging, for example, could incur a maximum loss at the rate of 8 per cent per annum. The alternative of hedging itself involves a cost, and may be unavailable, or expensive, especially if there is speculation on a devaluation in the forward exchange market. Finally, under the present fixed rate system there is still the risk of devaluation of a currency, which may act as a significant deterrent to international transactions.

The magnitude of the additional costs of international as contrasted with intranational payments under the IMF system and the deterrent effects of those costs on international trade and finance have not been seriously studied. The debate on fixed versus floating rates has instead focused attention on the assumed extra costs of a floating rate system—which costs have not been seriously studied either. In its recent statement of the case against floating rates [6]—one of the most solemn exercises in "primrose pathos" yet produced by a central bank—the Federal Reserve System regards a floating rate system as so clearly more expensive than a fixed rate system that it finds the presentation of empirical evidence on this point (or indeed on any other) unnecessary. This assumption is somewhat difficult to reconcile with the fact that the world's fifth largest trading nation was on a floating rate for twelve years, during which time no serious protest was raised against the system on these grounds. While a full statement of the argument is impossible here, there are some cogent theoretical reasons for believing that the difference in deterrence to international transactions between the IMF and the floating rate systems may be small by comparison with the deterrence inherent in the existence of separate national currencies not absolutely rigidly pegged to one another.

The third and most important difference between a fixed exchange rate system and a unified national currency—a difference which is responsible for the appearance of international equilibrium as a specific problem and the balance of payments as an object of economic policy—concerns the nature of monetary reserves. In both monetary systems, money actually in use consists predominantly of fiduciary money, pyramided by the banking system on a base of reserve money. But there are two important differences in the character of reserves between the two systems.

First, in the national system, any individual bank is obliged to maintain one-to-one convertibility between its monetary liabilities and the reserve money, whereas in the international system any nation has the ultimate right and power to alter the exchange ratio between its national money and the international reserve money. The obligation to maintain convertibility at a fixed rate imposed on banks in a national banking system is expressed in legal reserve requirements and supported by the central bank's obligation to act as a lender of last resort. In the international monetary system there are no legal obligations of this kind: countries are free to manage their international reserve ratios as they see fit, and there are no clearly defined internationally accepted standards of sound practice; and the lender of last resort function is entrusted to formal or informal international monetary co-operation—through the IMF, or by arrangements between governments or central banks. Ultimate national autonomy with respect to the exchange rate, the absence of precise rules of monetary behavior with respect to international reserves, and the uncertainty of the lender of last resort function, both permit countries to get into situations of potentially serious disequilibrium before they are forced to take corrective action and complicate the pursuit of corrective action by allowing legitimate grounds for speculation based on the expectation that corrective action will include a change in exchange rates. The effects of speculation on international short-term capital movements, in turn, reinforce dependence on central bank and governmental co-operation.

Such dependence has its dangers, not only because it tends to give central banking opinion—inherently a conservative and orthodox force—a controlling influence over domestic economic policy, but also because it introduces into the equilibrating process extraneous considerations of national prestige and rivalry. A world in which national economic policies have to be sanctioned by international banking opinion is likely to fall short of satisfactory performance with respect to the objectives of high employment and economic growth; nor, as Friedrich Lutz has recently pointed out [2], is equilibration by prestige borrowing and *noblesse oblige* lending among the leading nations likely to contribute to the efficient international allocation of capital. In addition, conflict between the need to balance the balance of payments and the domestic objectives of economic policy is likely, as already mentioned, to induce nations to resort to variations in barriers to international trade and payments as a means of bridging the conflict. The inconsistency of balance-of-payments interventions in the competitive system with the liberal objectives of a fixed rate system is disguised but not altered by certain conventional fictions, of which the most recently popular are the notion that discrimination in favor of home producers exercised in the spending of governmentally-provided funds is ethically different from tariff protection, and the belief that tax credits and other governmental aid to exports are not equivalent to dumping.

The second important difference between the national and the international banking system is that reserves in the former are created by the national central bank and managed with the purpose—however poorly executed in practice—of providing monetary stability and facilitating economic growth; whereas the ultimate reserve in the international system is gold, the supply of which for monetary reserve purposes is dependent on the profitability of new production and the demand for hoarding. The inadequacy of new supplies of monetary gold to meet the reserve needs of a growing world economy, together with the sterility of gold as a reserve asset by contrast with convertible currency securities, has fostered the use of major national currencies as international reserves. The resulting system is very like the American correspondent banking system before the creation of the Federal Reserve, except that reserve currency countries are far less willing than correspondent banking centers to meet runs by suspending cash payments. The defects of the system have been ably analyzed by Robert Triffin [5] and need little elaboration here. Its major faults are, on the one hand, its short-run dependence on confidence in the capacity of the reserve currency country to defend its exchange rate, which renders the system peculiarly vulnerable to liquidity crises;

and on the other hand, the fact that adequate long-run growth of international reserves necessarily weakens the liquidity position of the reserve currency country unless that position is shored up by increasing commitments of the other reserve-holding nations to support the reserve currency. In short, the system lacks a central bank capable of providing liquidity without restriction in a liquidity crisis and able to expand international reserves in conformity with the needs of expanding world trade. Instead, both short-run and long-run liquidity needs are entrusted to the co-operativeness, generosity, and foresight of central banks. Owing to the well-known asymmetry of the impact of reserve changes on deficit and surplus countries, this inevitably means that the operation of the system will be unduly influenced by the countries with the most conservative monetary and financial policies and outlook. That implication used to be expressed in the contention that the gold standard had an inherent deflationary bias; one can add that fixed exchange rates also have an inherent antigrowth bias. Moreover, both because central banks are typically slow to recognize a change in the economic environment that alters the policy problem and because agreement among a group of independent agencies on the need for a change in policy must inevitably lag on the evidence of the need for change, central bank operation of the international monetary system will probably have significant procyclical effects on the world economy.

In analyzing fixed exchanges as a system for achieving the advantages of a unified currency on an international scale, I have after all come close to playing the devil's advocate for floating exchange rates. That was not my assignment. Instead, I conclude by offering three observations suggested by my analysis, on the assumption that a unified international currency or its equivalent is desirable. First, the advantages of a unified currency are primarily derived from freedom of competition in goods and factor markets; enjoyment of them on an international scale requires not merely that the fixed rate system be operated in ways that do not employ barriers to international trade and payments to achieve equilibrium, but that it be accompanied by determined efforts to reduce barriers to international movements of goods and factors. Second, from the point of view of a unified international currency, the trouble with the present fixed rate system is not that the rates are fixed but that they are not fixed firmly enough. The allowable deviation of rates from par should be narrowed, not widened as is frequently suggested. More important, the possibility of changing rates allowed under the IMF charter should be renounced, at least by the major nations; among other things the reserve currency countries should be willing to demonstrate their determination not to devalue by giving whatever explicit guarantees are necessary to carry conviction, instead of trying to have it both ways by expecting other nations to trust their promises that they will not devalue, while reserving full freedom to do so. Third, the short-run and long-run liquidity problems need to be solved by more effective means than the present one of reliance on *ad hoc* arrangements among central bankers, coupled with ritual public pronouncements that there is no liquidity problem. The obvious solution indicated by national monetary experience is the institution of a genuine world central bank. Experience of the shortcomings of national central bank policies in recent years suggests, however, that the first responsibility imposed on the world central bank should be to increase international reserves at a rate adequate to support the growth of world trade at a high level of world economic activity.

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FLEXIBLE EXCHANGE RATES

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Economists seem to be moving toward agreement on two propositions: that the balance of payments of the United States is in disequilibrium; and that the international monetary arrangements of the free world show inadequate structural soundness. These twin problems have reopened for discussion the whole gamut of possible arrangements for international payments and liquidity. Economists concerned with these subjects find themselves once more at an intellectual Bretton Woods.

In such a setting, it is appropriate to consider the case for flexible exchange rates as a method of dealing with problems of international liquidity and payments equilibrium. Flexible rates are widely felt to offer, in ideal circumstances, a solution to problems of both international liquidity and external equilibrium. This paper considers the present standing of the theoretical case for flexible rates and the circumstances which make that case relatively stronger or weaker from the standpoint of a single country. I shall take a flexible exchange rate to be one which either fluctuates freely in response to market forces or is subject only to clandestine, short-run stabilization through government purchases or sales of foreign exchange. And I shall take as the alternative to a flexible rate not the adjustable peg but permanently fixed rates. This comparison seems to be the appropriate one, in view of the reluctance of countries maintaining an adjustable peg to adjust it until all else has failed. Indeed, many of the arguments for flexible exchange rates pertain equally well to an adjustable peg which actually gets adjusted.

The Case for Flexible Rates

The case for flexible exchange rates, however put, rests upon a second-best situation. If all domestic prices and wages were perfectly flexible and adjusted to maintain a fully employed economy, then domestic economies could adapt to any arbitrary pattern of fixed exchange rates. The exchange market could still be equilibrated, along with those for domestic goods, services, and factors of production. With broad inflexibilities in domestic prices, however, this indifference disappears. The absence of direct adjustment of the exchange market by a flexible rate increases the chance of some domestic market being thrown into disequilibrium in order to adjust the foreign exchange market (see Friedman [2]). To put the argument another way, given whatever constraints or rigidities exist in the domestic economy, a flexible exchange rate would be a matter of indifference if domestic policy instruments were ample in quantity and quality to keep the foreign exchange market in balance and also achieve all other publicly defined economic goals. But when the available policy tools are too few, too slow, too blunt, or too weak to gain all desired objectives, the argument grows strong for employing a flexible exchange rate to free these instruments from direct concern with the balance of payments (see Meade [5]).

The literature opposing flexible exchange rates has put forth only two arguments of comparable generality, neither of which has fared well under close theoretical scrutiny. One has turned on the conditions for stability in the foreign exchange market. The familiar "elasticities" approach to the stability of the balance of trade seemed to raise a serious possibility of instability, implying in turn that a flexible exchange rate might run-off to zero or infinity in response to small disturbances in international transactions. Two major points have been made against this possible instability. First, as Morgan [6, pages 282-86] has shown, instability in the foreign exchange market implies an unstable equilibrium in at least one underlying commodity market, a condition which draws a negative visceral reaction from most economists. Second, there are some grounds for thinking that a foreign exchange market which appears in unstable equilibrium at one exchange rate will find itself in stable equilibrium at other rates, so that the instability is only a local one [9, pages 3-11]. Thus the argument from instability seems to have been reduced to a shadow of its former self, though hardly an insubstantial shade. For the elasticity pessimist can still argue that the exchange market may be in near-neutral equilibrium over a significant range of exchange rates, so that it takes a lot of rate variation to do a little bit of equilibrating. Or he may turn to a dynamic version of the stability problem, arguing that equilibration by a flexible exchange rate may follow a tortuous and costly path of adjustment before reaching a new stable equilibrium.

The other general argument against flexible exchange rates has concerned the effect on international transactions of increased uncertainty in comparison to fixed rates. The cost of hedging, where it is possible at all, or the disutility of uncertain future proceeds from international transactions may reduce their volume and impose welfare costs exceeding any allocative gains won through flexible exchange rates. The theoretical answer to this argument, as Professor Friedman [2, pages 173-74] and others have pointed out, is that variations in a flexible rate simply respond to disturbances in the underlying national economies. Adjustment to these disturbances must come somehow, if not through exchange rate movements, then through changes in domestic prices or incomes or in the incidence of government controls used to suppress disequilibria. Assuming an equal freedom from mistakes of public policy in each case, a fluctuating exchange rate simply relieves some domestic policy or market variables of part of the task of adjusting to disturbances. This interpretation can be applied even to long-term international capital movements, which are often singled out as the main potential victim of flexible exchange rates. Flexible rates impose a conversion risk of unanticipated gains or losses in repatriating the interest or principal of a long-term loan. But the alternative to a fluctuating rate is domestic adjustments in response to disturbances which increase what we might call the solvency risk of long-term investments—the chance of unexpected changes in quasi-rents which threaten not just the profitability of the long-term investment actually made but, more broadly, its status as the most profitable use of funds known to the lender at the time of his original commitment. Lenders cannot be denied the right to suffer greater disutility from conversion risks than from solvency risks, and thus be discouraged by flexible exchange rates. However, there is no a priori reason to think that they would be subject to this illusion.

Thus the objection to flexible rates on the basis of uncertainty lacks generality in the same sense as that based upon instability. One can imagine conditions in which either problem would amount to a critical objection to permitting the exchange rate to fluctuate freely. But neither refutes the general case for flexible rates. The same could be said of many other objections which have been raised. For instance, the discussion of the power of speculation in the foreign exchange market to destabilize the exchange rate seems to have reached agnostic conclusions. Competitive speculation has been shown to stabilize the foreign exchange market when the speculators themselves have certain knowledge about future conditions, but this latter assumption tends to avoid the real issue of the effect of speculation superimposed upon periodic, unexpected shifts of the variables affecting the balance of trade. Furthermore, there is no denying Viner's argument [11, pages 290-92] that foreign exchange is an article relatively more susceptible to speculative transactions than commodities having high storage costs, heterogeneity of units, and other such physical properties.

The complexity of the problem of predicting the effect of speculation makes necessary an appeal to the factual record. There we find, for recent evidence, Paul Wonnacott's study of Canada's experience with a flexible exchange rate [12, pages 124-28], which seems to show that short-term capital movements tended to stabilize movements of the Canadian exchange rate definitely more often than not. But a recent reappraisal of the interwar period [1] seems to show that under seriously disturbed economic conditions and considerable uncertainty or misapprehension about equilibrium currency values, speculation works much less well. These cases and the Canadian experience all suggest that beliefs about the permanence of traditional foreign exchange values have a powerful influence on the effect of speculation, for good or ill.

The appraisal of flexible exchange rates, then, seems destined to run in terms of particular cases. The general case for their use stands intact, but many arguments against them could prevail for particular countries. Since each country normally decides unilaterally whether or not to adopt a flexible rate, the balance of this paper will turn to the pros and cons of flexibility from the view point of a single country. I shall only note that treating the problem in this fashion leaves certain vital factors out of account. Flexible exchange rates among many countries may raise menacing possibilities of instability and shifting expectations which cannot be foreseen when examining the prospects of a flexible rate for a single country in a world of pegged rates.

The relative strength of the case for flexible exchange rates may differ between countries, I suggest, for two basic types of reasons. First, the typical form and source of disturbances to the balance of payments differs significantly from country to country, and a flexible rate does not help equally to restore internal and external equilibrium in all cases. Second, the adequacy and flexibility of internal policy instruments and the restrictions on their use imposed by market forces may vary among countries and the patterns may not mesh with equal smoothness with flexible rates.

Sources of Disturbance

A country has the strongest case for flexible exchange rates when disturbances to its balance of payments typically come from outside its borders. A shift in demand toward a country's exports under fixed rates initially falls entirely on the export industries. The internal adjustment of the national income and the general price level necessary to restore equilibrium comes only after and on account of the export expansion. There is a good chance that the ultimate change in the equilibrium output of the export industries will be less than the initial change before adjustment forces take hold throughout the domestic economy. A fluctuating exchange rate, by contrast, tends to adjust without an expenditure lag, cushioning the impact on the output of exports and furnishing an immediate incentive for output adjustments in the proper direction elsewhere in the economy. If resource reallocation has its costs, if large changes for a particular industry are much more costly than small ones, and if transitional changes for a particular industry which overshoot the mark cause long-lasting misallocations of fixed capital, there is a strong case for adjustment to external demand shifts through flexible rates.

Likewise, a country can generally insulate itself from income fluctuations abroad much better through flexible than through fixed exchange rates. Ruling out induced changes in capital movements, a fluctuating exchange rate averts changes in the trade balance and blocks transmission of the business cycle by that route. Various economists have pointed out, however, that income fluctuations can be transmitted in other ways as well, some of which would be unique to fluctuating exchange rates. The best known and most important of these, to make a casual empirical judgment, is the Laursen-Metzler effect of the terms of trade on saving [4]. A business cycle upswing abroad produces not a favorable movement of the balance of trade but instead an appreciation of the domestic currency, leaving aggregate demand constant in money terms. But the improvement of the terms of trade which is likely to accompany the appreciation (if supply elasticities are high) raises domestic real income. Given the frequent assumption that the marginal propensity to consume is less than the average, money savings out of a given money income increase, with a depressing effect on domestic aggregate demand. Booms abroad allegedly cause depressions at home under flexible rates. It is hard to take the Laursen-Metzler effect very seriously, at least in comparison to the forces transmitting income fluctuations under fixed exchange rates. The effect depends on the marginal propensity to consume being less than the average. Furthermore, it predicts that falling national income would go with a rising volume of exports and rising domestic prices of internationally traded goods. Countries which engage extensively in international trade often show a strong accelerator effect of export on investment and thus national income, which would countervail the Laursen-Metzler effect. In short, the original conclusion from Keynesian analysis that flexible exchange rates protect the domestic economy more effectively than do fixed rates against foreign income fluctuations still seems largely correct.

When disturbances to the balance of payments originate within the domestic economy, however, the case for a flexible exchange rate may become less strong. If the country suffers from endogenous business fluctuations which it cannot for political reasons fully control by internal policy, adopting a flexible exchange rate denies it the assistance of an income leakage through the balance of trade at the top of the cycle and an income injection at the bottom, which fixed rates would tend to promote. A country under fixed rates automatically beggars, or inflates, its neighbor, as the case may be; the unneighborliness of the act does not change the fact that internal stability is assisted thereby. The same sort of conclusion might follow for some other sorts of domestic disturbance to the balance of payments. Consider a country exporting primary products, which faces an elastic export demand but undergoes large random variations in export supply, such as through crop failures. Under fluctuating rates, the home currency would tend to appreciate in good crop years, lowering the price of imports and imparting fluctuations to the output of the import-competing industries inverse to those in the export sector if imports and import-competing goods are close substitutes. This would not be desirable, especially since by maintaining fixed exchange rates under such conditions and taking fiscal action to offset the effects of income variation a country might insulate itself somewhat from such fluctuations in the export earnings and the income and output redistributions which they imply. A fixed exchange rate could have somewhat the same function as an export stabilization scheme.

Constraints on Domestic Policy

Turning from the sources of disturbance to the foreign exchange market, the constraints upon domestic economic policy likewise influence the strength of the case for the adoption of flexible exchange rates by a particular country. Issues of this type have pervaded much of the discussion of flexible rates since World War II, especially in a setting of cost-push inflationary pressures against which internal policy measures are having questionable effectiveness. On the one hand, some economists have argued that governments must nail their flag to the mast of a fixed exchange rate, implying that the defense of a fixed rate would dissuade interest groups from seeking price increases or, conversely, that renewed wage demands would be unleashed by the cost-of-living changes following upon a currency devaluation. Viner [11, page 294] has declared that for many countries avoidance of exchange depreciation "seems to be the only factor of any strength which puts a brake on inflation." Depreciation under a flexible rate might create inflationary pressure without any symmetrical tendency for appreciation to bring about deflation. In this case, exchange rate fluctuations keyed to the ordinary business cycle could build a ratchet effect into the domestic price level where one would not otherwise exist (see Triffin [10, pages 82-84]).

On the other hand, other economists have held that conditions of wage pressure and wages rigid in the downward direction make the strongest case for flexible rates. Disturbances to the balance of payments which cannot be eliminated by internal deflation may be subject to cure by the depreciation of a flexible rate. The critical assumption of fact in this case is that internal cost pressures are not sensitive to the prices of imports, so that real income can be reduced through exchange depreciation where it would be impossible through domestic deflationary pressure without unemployment resulting [8, page 663]. Obviously these opposed arguments differ squarely on an issue of fact: the sensitivity of the domestic money cost level to the price of imports. The answer may not be the same for every country.

Indeed, all features of the flexibility and adaptability of domestic policy toward employment and the price level bear upon the case for flexible exchange rates. Economists who feel that the present regime of fixed exchange rates among the major industrial countries is workable have held that, with the aid of some international co-operation, external disequilibria can be handled through small changes in countries' relative rates of inflation. The nation in deficit need only restrain increases in its price level, and the nation in surplus may simply pursue its full employment and growth policies with slightly increased vigor (see Haberler [3, pages 180-81]). The suggestion implies that autonomous disturbances within the usual range of severity can be offset and balance-of-payments equilibrium maintained by these slight policy shifts—all without forcing any country into choosing between a change of its exchange rate and domestic policies which promote either open inflation or serious unemployment. Once again, the merit of this pragmatic case for putting up with fixed exchange rates turns upon factual questions. How much effective freedom exists for industrial countries to affect the balance of international payments by adjusting rates of change in their price levels, even through international cooperation? How large are the autonomous swings in the balance of payments which would typically have to be absorbed if this system is to work over a sustained period of time? Can such policy co-ordination secure international equilibrium in the first place and, if it be achieved, do the productivity growth rates of the industrial countries lie close enough together to let this "moving equilibrium" persist? This is no place to attempt an answer to questions of this complexity. The very complexity, however, may furnish a strong case for turning toward flexible rates.

Another consideration affecting a country's choice of a fixed or flexible exchange rate is whether such a policy can let it make use of international disturbances to help achieve internal balance. A nation's policy-makers may foresee a prolonged structural change in its foreign trade, such as declining demand for a major export product. This portends deteriorating terms of trade and tendencies toward either depreciation or balance-of-payments deficits. They may also foresee a chronic problem of internal balance, such as inflationary pressure. The best policy in such a situation might be the "adjustable peg"—periodic devaluation when the current account has become significantly unfavorable, with the recrudescent deficit on current account serving as a brake on inflationary pressures. Conversely, a country facing this same deterioration of its terms of trade and a prospect of difficulties in maintaining full employment at home would have an unusually strong case for adopting a fluctuating exchange rate. The flexible rate would not make a positive contribution to using the external disturbance to offset the internal one, except possibly by speeding the process of drawing new industries into the export markets and thus shifting upward the aggregate rate of investment through their expansion. In any case, though, it would avert the leakage of an unfavorable balance of trade as an additional burden on policies seeking to secure domestic full employment.

So far, the discussion has focused upon the trade balance and assumed that the level of private international capital movements is independent of the changes occurring in the current account. But the experience of the last five years in a number of countries has shown that induced capital movements can have critical results for the effectiveness of domestic policy. In the extreme case, large amounts of perfectly mobile capital would forge a rigid link between the interest rates of different countries. A nation raising its interest rate to restrain aggregate demand would witness only a great increase in its capital inflow or decrease in its outflow. Under fixed exchange rates its exchange stabilization authority would accumulate large holdings of foreign currency, or its exchange rate would appreciate under a regime of flexible rates, but domestic investment would not be directly checked. This is again a problem of inadequate policy instruments, in which case the choice of a flexible exchange rate may have some effect of either relieving or augmenting the inadequacy. A nation with an adequately flexible fiscal policy or selective controls on the availability of credit might be indifferent to the constraints of international capital movements on policies operating through the domestic interest rate.

To take an extreme case, suppose that monetary policy operating through the interest rate is the only substantial corrective for inflation or unemployment at home and that no domestic market forces act independently to curb these conditions. With a fixed exchange rate, attempts at changing the interest rate simply swamp the exchange stabilization fund which maintains the fixed rate, either exhausting foreign exchange supplies if the interest rate has been lowered or converting the stabilization reserve to foreign currency if it has been raised. No effective means exists, then, for achieving internal balance. If the exchange rate fluctuates freely, raising the interest rate to combat inflation causes a substantial capital inflow and appreciation of the domestic currency. This change causes the interest rate policy to have some effect, though only indirect, on internal equilibrium, for the appreciation now creates an unfavorable trade balance and affects the real capital transfer. In short, when the interest rate becomes a key domestic policy variable, international capital movements become troublesome under any system of managing international payments. But they cripple the pursuit of domestic equilibrium in the short run more under fixed than under flexible exchange rates.[1] The superiority of flexible rates in this case is further strengthened when we note the probable influence of interest rate changes on forward rates and thus on the profit margin on covered international short-term capital movements [9, page 82].

In the light of this conclusion, one grows puzzled about Canada's troubles with its flexible exchange rate, a disenchantment commonly associated with the domestic consequences of international capital movements. A view often heard is that the Canadian exchange rate failed because it did not decline sharply from 1958 on, a time when Canada's unemployment rate was disturbingly higher than that of the United States. Assuming that Canada faces interest-elastic international capital markets, Canada should have been able to stem its large inflow of foreign capital by lowering its whole structure of interest rates. That policy would have reduced the premium on the Canadian dollar and improved the trade balance and, through it, the level of employment. Casual inspection of Canadian policy suggests, however, that the whole trouble lay with policies aimed not at lowering but at raising the interest rate in the face of unemployment. The differentials of Canadian interest rates over their United States counterparts increased in the late fifties. As one would expect, the capital inflow increased, the Canadian dollar tended to appreciate, and the unemployment problem grew all the worse. The Canadian flexible exchange rate is gone now, victim not of its own malfunctioning but of squarely inappropriate monetary policies.

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[1] This conclusion differs from that of R. A. Mundell [7, pp. 237-42], who assumes a domestic corrective mechanism to inflation or deflation, e.g. the Pigou effect, such that when monetary policy (the interest rate) is imprisoned by international capital movements, the economy will still approach internal equilibrium. Furthermore, the approach will be "direct" under fixed exchange rates, since the balance of payments can easily be kept in equilibrium by slight changes in interest rates while the price level is adjusting. His analysis of the flexible-rate case, similar to that presented above, reveals to him a danger of dynamic complications in that internal disequilibrium can be eliminated only by using the interest rate to disturb the foreign exchange market.

In summary, this paper has avoided any categorical judgment for or against flexible exchange rates. While acknowledging the strong general theoretical case for them, it has attempted rather to outline circumstances relatively more or less favorable to their adoption by a single country. But I cannot suppress a personal judgment that the device has much to commend it for the major trading nations at this time. Yet persons responsible for national monetary policies usually see the flexible rate as a path leading into unknown darkness. Perhaps the next research on the subject should turn to the transitional problems of moving to a general regime of flexible rates and the merit of fixed versus flexible rates for a customs union—the so-called "optimum currency areas" problem [8]. With these practical aspects of flexible rates laid open, familiarity might help to kill contempt.

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INTERNATIONAL LIQUIDITY: THE NEXT STEPS

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The international monetary system is in better health today than for many years. The stock of reserve assets is adequate for now and for the next decade. The central banks have shown great skill and vigor in their efforts to defend the monetary system. The recent growth in European reserves has been achieved at the expense of the United States, but this country can still purchase all the time it needs to solve its payments problem if only it is willing to pay out reserves and to use the time it buys for a good adjustment. The less developed countries have inadequate reserves, but their payments problems cannot be resolved by a simple increase in liquidity, for they will spend any cash they can earn or borrow.

Yet there is a lingering concern about the future. Few critics still take aim at the margin, arguing the need for more reserves. But many still complain that the structure of reserves is unsatisfactory, arguing the need for consolidation to protect the quality of the reserve media. And too few people have examined the implications of the new arrangements connecting central banks.

The problems of the margin and of asset structure cannot, of course, be segregated. Concern about prospective growth first drew our attention to the structure of reserves. Critics argued that such growth could impair the quality of the dollar as a reserve currency.[1] Then the critics' own proposals met with similar objections. Triffin's plan for reform of the IMF, for example, could expose the Fund to a dangerous gold drain as it went about incurring liabilities.[2]

But recent history calls direct attention to the structure of reserves. There has been a quiet drift toward gold, with several central banks increasing their gold holdings or running down their dollar assets as they lost reserves. And though gold production has grown steadily since 1957, the annual accretion to official holdings has been falling off. Recent changes in the monetary system likewise stress consolidation rather than expansion. They seek to discourage or offset massive flows of private money that could drain reserves from New York and London, impairing the confidence of other central banks. They also seek to supplement national reserves and the lending powers of the IMF. These changes are described with great candor in two recent articles,[3] but a brief summary may be helpful here.

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[1] Robert Triffin, *Gold and the Dollar Crisis* (New Haven, 1960), pp. 64–69, and my own "International Liquidity and the Balance of Payments of a Reserve-currency Country," *Q.J.E.*, Aug., 1960, pp. 572–86.

[2] U.S. Congress, Joint Economic Committee, *Hearings: International Payments Imbalances and Need for Strengthening International Financial Arrangements* (Washington, 1960), pp. 193–96.

To forestall speculation and related cash flows, the U.S. Treasury and Federal Reserve System have begun to intervene in the foreign exchange markets. In adroit maneuvers against speculation, they have forced the forward mark and Swiss franc toward their interest parities. The United States, United Kingdom, and European governments have also forged a common policy toward the London gold market, which apparently includes a set of ground rules governing central bank transactions and provision for joint intervention to contain the price of gold.

This new activity has been financed by intricate credit arrangements. In some cases, the United States has made outright purchases of foreign currency, or borrowed by issuing special obligations denominated in a foreign currency. More often, it has drawn on new stand-by credits set up in its favor by foreign central banks. Under these arrangements, each central bank agrees to swap its currency for dollars. The two countries then agree to liquidate their balances at a fixed exchange rate and a fixed date three to six months later. Some of the foreign currencies drawn under these arrangements have apparently been used in spot operations; some were set aside to meet the deferred obligations incurred by the United States in its intervention on the forward markets.

The details of these "swap" transactions vary from instance to instance. So do the operations in each foreign currency. The American authorities have tried to mesh their interventions with the money-market tactics of the other central banks. Because there is a shortage of money-market assets in most European countries, commercial banks hold foreign currencies, especially dollars, as secondary cash reserves, and central banks must operate in the foreign exchange markets to carry out domestic monetary policies.[4] They cannot control bank reserves using the familiar tools; they have instead to influence Eurodollar flows. Paradoxically, the development of the Eurodollar market may delay the formal monetary integration of Western Europe—the creation of a common currency for the European Economic Community. The many kinds of Eurodollar credit substitute for more familiar money-market instruments, and the Eurodollar market gives the EEC and adjacent countries a unified money market. We may have to wait some years for a European reserve currency to join the dollar and the pound.

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[3] Charles A. Coombs, "Treasury and Federal Reserve Foreign Exchange Operations," *Fed. Res. Bul.*, Sept., 1962; Robert V. Roosa, "Assuring the Free World's Liquidity," Federal Reserve Bank of Philadelphia, *Bus. Rev.*, Sept., 1962. See, also, Bank for International Settlements, *Thirty-Second Annual Report* (Basle, 1962), pp. 125-27.

[4] Oscar L. Altman, "Foreign Markets for Dollars, Sterling, and Other Currencies," IMF *Staff Papers* (Dec., 1961), pp. 339-42.

The United Kingdom has also used central bank credits to strengthen its reserve position. After the revaluation of the deutsche mark and guilder in the spring of 1961, there was a massive run on sterling. But other central banks supplied the equivalent of $900 million in short-term credit to supplement Britain's own reserves. A few months later, the United Kingdom turned to the IMF and purchased $1.5 billion worth of foreign currencies. It used part of this record drawing to repay its debts under the so-called "Basle arrangements." In effect, Britain used the IMF to consolidate its short-term obligations and supply an indirect but uniform exchange guarantee.

In order that the IMF may continue serving this important function, ten major countries have agreed to place $6 billion worth of convertible currencies at the disposal of the Fund should it need this money to finance a major drawing. (These commitments are listed in the second column of Table 1.) This agreement is embodied in a protocol, the General Arrangements to Borrow, which provides that the Managing Director of the IMF shall propose a call on these currencies if a participating country applies for a drawing on the Fund to "forestall or cope with an impairment of the international monetary system" and if IMF resources are inadequate to finance the drawing. A call must then be approved by a complex qualified majority of the ten participants, and any member may opt out of its obligations by giving notice that its "present and prospective balance of payments and reserve position" cannot honor further calls.[5] Money borrowed by the Fund under the General Arrangements will be repaid within three to five years, when the drawing on the Fund is itself repaid, but may be paid back earlier if any of the lending countries encounters a payments problem of its own.

These credit arrangements are an intriguing mixture of reserve transfer and reserve creation. All of them involve the creation of new reserve media. A swap transaction between the United States and Germany adds dollars to German reserves and deutsche mark to U.S. reserves. It is not a transfer of outstanding reserve assets from one country to another. Nor is it reserve growth in the ordinary sense. The reserves created by the swaps will be extinguished as soon as they are used. When the United States uses its deutsche mark in the foreign ex-

[5] International Monetary Fund, *International Financial News Survey* (Jan. 12, 1962). The Baumgartner letter attached to the protocol provides that the prospective drawer shall not vote and that a call shall not be issued without (1) a two-thirds majority of the number of participants voting and (2) a three-fifths majority of weighted votes, with weights based on the size of the participants' commitments. These provisions mean that any four countries listed in Table 1 could block a U.S. drawing, but that the United Kingdom and Germany could also do so, likewise the United Kingdom or Germany, plus France or Italy, plus any third country.

change market, its balance at the Bundesbank is eradicated rather than transferred to a third country.[6] Furthermore, the cash assets created by the swaps and by special loans to the IMF are matched by liabilities and slated for repayment within a brief time; they do not bring a lasting increase in owned reserves.

But the architects of these new arrangements envisage further innovations that could enlarge owned reserves. When the United States moves over into payments surplus, it can accumulate European currencies instead of forcing other countries to run off dollar assets or to sell us gold. New reserves may also be created by more swap transactions, but larger and without express provision for repayment. Presumably, these balances could be used in any way—transferred to another country or converted into a third currency.

Underlying all these innovations is the fact and expectation of close co-operation among the key central banks. Such co-operation is essential to orderly official intervention in the foreign exchange markets and to an alignment of fundamental policies affecting trade and payments within the industrial community. But it may not be the proper basis for an international monetary standard. Reserve-asset structure can only pose a problem if some central bank decides to hold less of one reserve asset and more of another, or because a central bank does not want to hold its new reserves in the form they take when they are created. A monetary standard based on "close co-operation" rather than binding commitments and arrangements is built on a bad premise: It relies on acquiescence to cope with a crisis that can only be brought on if one central bank has declined to co-operate. Despite the talk and evidence of close co-operation, moreover, the reservoir of good will could easily run dry. During the last year or so, the London gold price has been kept a shade below $35.20 an ounce—a limit that may not allow sufficient variation to penalize potential speculators. Why this very narrow spread? Is it because central bank gold arbitrage becomes profitable when the gold price rises above $35.20 and some central banks might engage in arbitrage despite the new ground rules? Why, too, the current obsession with fixed exchange rates? Is it because some central banks would be compelled to sell off certain reserve assets if they foresaw changes in the key exchange rates?

Finally, *ad hoc* acquiescence in collective measures may have a high price. The Under Secretary of the Treasury, Robert Roosa, has opposed exchange rate guarantees because, he says, "one country after another will interpose conditions on its readiness to accept a guarantee—conditions that will at the least interpose their judgments more specifically into the determination of our military, aid, or investment activities abroad." "And where would we find ourselves," he goes on to ask, "when the demands of one of our guaranteed creditors conflicted with those of another?"[7] If Mr. Roosa is convinced that we would have to pay for the privilege of giving a guarantee, how much more costly might it be to purchase close co-operation without giving any guarantee? And what would be our price for co-operation once the United States had begun to hold large amounts of foreign currency? One might reply that every country would then have a club with which to beat the other into close co-operation. But this apparent parity would soon vanish if one country ran a payments deficit and lost reserves—and that would surely be the time when other countries would be nervous about holding the first country's currency.

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[6] The same thing will happen to dollar balances held by a foreign government, but in only one of several likely cases—if the balances are used to buy up foreign currency from Americans.

All of this is not to say that the central banks are fair weather friends. I merely argue that we should obtain binding assurances and more automaticity when the weather is its fairest so that every country can count on help when the sky begins to cloud. I shall not succumb to temptation and present another "plan" for reform of the monetary system. It would be quite wrong to backtrack now, searching for perfection, when the recent innovations offer a foundation on which to build a very strong monetary system.

In my view, these recent innovations start a major change in financial practice. They begin to substitute governmental credits for a further growth in national reserves. Carried further, these arrangements can provide liquidity without doing damage to the reserve position of any center country of the IMF. They point to a system of overdraft financing rather than the more familiar methods of deposit growth. I shall therefore urge some minor changes that will make the credit network a more perfect substitute for growth in owned reserves—changes aimed at automatic access to existing credits and at steady growth in overdraft facilities.[8]

As a first step toward a stronger system, I would favor the rapid completion of the stand-by network. Every major central bank should undertake to lend $150 million or $200 million to the United States and a like amount to the United Kingdom, obtaining a reciprocal commitment from each of those two countries. One might even want asymmetry in each overdraft agreement, with the United States committed to lend more than it is allowed to borrow. Many countries use the dollar when they intervene in the foreign exchange markets. Granted larger dollar credits, they would not need to draw on one another. In brief, bilateral asymmetry would give rise to over-all symmetry; each other country could draw dollars to defend its currency, and the United States could draw every other money.

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[7] Roosa, *op. cit.*, p. 6.

[8] I do not claim any originality for these proposals or for the way in which I have combined them. Readers who have followed the burgeoning literature will recognize bits of Triffin, Angell, Bernstein, Zolotas, and others. All remaining faults are, of course, those of the gold exchange standard.

These stand-by arrangements should be permanent, with provision for immediate drawings and for repayment in six months at the exchange rate that prevailed when the drawing first took place. A country should be able to use its stand-by credits whenever its currency falls beneath its parity with that of its partner. It should not be allowed to use borrowed funds for purchasing a third currency. With these restrictions and, perhaps, an end to cross-crediting, no country could do damage to its partners, nor influence its partners' economic policies.

There is, of course, one danger in a credit network connecting the United States with every other country. The United States might find itself financing a French deficit with some other country, say Germany or Britain. At present, by contrast, such a deficit is financed by a transfer of existing dollar balances, not with newly drawn dollars. But if American financing went on very long, the dollar would tend to a discount on the foreign exchange market and the United States could draw marks or pounds to correct the situation.

Next, I would favor two important changes in the General Arrangements to Borrow, making those arrangements fully automatic and allowing them to grow. Under the existing agreement, two or three potential lenders can prevent Fund borrowing merely by casting negative votes. Furthermore, each country is the final judge of its own ability to lend. I would consequently urge that the IMF be empowered to invoke the General Arrangements at its own discretion—power that Per Jacobssen tried hard to obtain—and that every country be compelled to participate unless the Fund's holdings of that country's currency are above the usual three-fourths of quota.

The General Arrangements also fix each participant's commitment. As time goes on and trade and payments grow, these commitments may prove far too small. They may even be too small to cope with the next crisis. The IMF cannot draw the whole $6 billion because that sum includes the currency of the deficit country. Each country's obligation should therefore be tied to its own reserves. Its commitment should be equal to, say, half its gross reserves less an exemption fixed in advance. Table 1 shows how this arrangement would work out. I have inverted my formula and calculated the fixed exemptions implied by each country's commitment under the existing General Arrangements and by its gross reserves at June, 1962. The exemptions total $29.1 billion; they are listed in the fourth column of the table. I have also listed alternative exemptions for the ten participants, cutting those of Germany and

TABLE 1

Reserves and Contingent Commitments to the IMF, Actual and Proposed
(Billions of Dollars)

Country	Gross Reserves, 6/30/62	Contingent IMF Commitment		Proposed Exemption	
		Actual	Calculated*	Implied†	Adjusted
Belgium	1.60	.15	.15	1.30	1.30
Canada	1.81	.20	.16	1.40	1.50
France	3.48	.55	.44	2.40	2.50
Germany	6.23	1.00	1.36	4.25	3.50
Italy	3.24	.55	.62	2.15	2.00
Japan	1.62	.25	.06	1.10	1.50
Netherlands	1.79	.20	.25	1.40	1.30
Sweden	.73	.10	.11	.55	.50
United Kingdom	3.44	1.00	.22	1.45	3.00
United States	17.08	2.00	2.54	13.10	12.00
Total‡	41.02	6.00	6.00	29.10	29.10

* Equal to 0.5(gross reserves less adjusted exemptions).

† Equal to gross reserves less 2×actual commitments, so that 0.5(gross reserves less implied commitments) = actual commitments.

‡ Detail may not add to total because of rounding.

Sources: Basic data from International Monetary Fund, *Annual Report* (Washington, 1962), and *International Financial Statistics* (October, 1962).

the United States and raising those for Britain and Japan. These revised exemptions make allowance for Britain's large monetary liabilities and do not reward Germany for its reserve appetite of 1956-61. But the structure of exemptions is of secondary interest. The more important feature of this formula is its flexibility. A 1 per cent annual increase in gross reserves would give the IMF power to borrow an extra $200 million. And if this were insufficient, a $200 million cut in the exemptions, spread across all countries, would give the Fund another $100 million. The IMF might even be allowed to cut exemptions by $200 million in every year that saw gross reserves rise by less than 1 per cent. Otherwise, there might be insufficient credit to replace renewed growth in national reserves. Exemptions might someday turn negative, but this would simply say that each country has a fixed commitment in addition to the one depending on reserves.

These amendments to the General Arrangements would have one other virtue. They would help to curb the reserve appetite of a surplus country. An increase in one country's gross reserves would enlarge its contingent obligation to the IMF and permit the Fund to supply its currency to deficit countries. They could then slow down their own reserve losses, though not offset more than half the surplus country's gains.

I come now to the vexed question of exchange rate guarantees.

Many people look upon a guarantee as the painless way to run a payments deficit—to lodge extra dollars with nervous foreigners. Viewed this way, guarantees hold a dangerous contradiction. "In giving a guarantee," Roosa writes, "the United States would expect to release its domestic economic performance . . . from the constraints imposed by the need for balance of payments equilibrium; in accepting a guarantee, other countries would expect the United States to maintain their confidence in its internal and external economic performance. . . ."[9] Put differently, the very growth of guaranteed claims might sap this country's will to honor its commitment (or, at least, cause other countries to distrust this nation's pledge). But this is to misconstrue the function of a guarantee. It should help the foreigner retain the dollars he already holds, not seek to seduce him into holding more. Furthermore, a guarantee would free the United States from the chief constraint imposed by its role as a reserve center. It could change its exchange rate without wrecking the international monetary system. Guarantees are often urged to forestall devaluation as they might stave off runs on major currencies. They should actually be urged because they would allow a devaluation when it is required. Without guarantees, moreover, we may confront a general exchange rate paralysis; when the United States has come to hold European currencies, the European countries may also feel a compulsion to keep their exchange rates still.

There are grave problems of practice and principle lurking in this simple case for a guarantee. But I must not spike the discussants' guns by a pre-emptive strike. Let me merely raise two common questions and sketch out the briefest answers:

1. Could the United States afford to honor a firm guarantee on some $13 billion of official dollar claims? Here, I think, the critics confuse the burden of a guarantee with the burden of devaluation. If foreign governments were told that the United States planned to devalue the dollar, they would doubtless take our gold before the devaluation. In that case, the dollar value of their gross reserves would rise by just as much as with a guarantee—and they could exercise the same extra claims on American resources. A guarantee would impose a net resource-cost only if the foreigner had imperfect foresight, or the United States gave no notice of its plans. The United States, moreover, could control the resource-cost by attaching its exchange rate guarantee to a special instrument rather than covering all dollar assets held by foreigners. The Treasury could issue nonmarketable bills carrying a guarantee (and a lower rate of interest than its other obligations). It

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[9] Roosa, *op. cit.*, p. 6. His objections would apply with special force to the recent suggestions of the United Kingdom for an indirect IMF guarantee on new foreign exchange holdings. He has, in fact, anticipated this proposal in a trenchant passage (*ibid.*, pp. 7-8).

might then decline to issue more such bills, save at stated intervals and in such amounts as it thought appropriate to secure desired growth in total dollar claims.[10] It might even limit the amount it would sell to a single country. But this would create two kinds of dollar balances and might give rise to new gold losses; the giving of a guarantee on some dollar assets could itself stir rumors of devaluation and cause foreigners to convert their other dollar assets into gold.

2. Could the United States give a firm guarantee when, in fact, it broke its word back in the thirties? Here, I think, there is misunderstanding of the old gold clause case. In my inexpert reading of the jurists' jargon, the Supreme Court did not sanction abrogation of the gold clause. On the contrary, it held that the government could not break a contract. But it then declined to grant redress because U.S. citizens were not injured by the abrogation; their other obligations were denominated in paper dollars, not in gold. This argument on damages could not be applied to foreign central banks that have obligations outside the United States. In any case, we could assuage foreign doubts about the sanctity of a U.S. guarantee by accepting the compulsory jurisdiction of the World Court in all matters that pertain to the guarantee.

Some critics of the present monetary system would like to give the IMF a much larger role. They would dispense with stand-by credits and rely directly on drawings from the Fund. They would give guarantees through the IMF by having it take dollars in exchange for new deposits. But bilateral arrangements have a place in any system; they can be invoked quickly and quietly, giving faster aid than a drawing on the Fund. They can then be paid off by a drawing on the Fund when calm is again restored. As I see it, moreover, expansion of the IMF comes at a later stage. When it comes time to enlarge national reserves, the necessary growth should be secured by a further change in the General Arrangements to Borrow. The Fund's obligations should be made transferrable (or exchanged for deposits at the IMF). The IFM could then create reserves when countries came to it for aid, and the attendant growth in total reserve assets would be a response to manifest needs. At that point, too, the IMF would become quite Triffinesque. But most of the sensible proposals lead in this direction. They differ more in rates of change than in their objectives. And while all roads may lead through Yale, after some meandering, we do not have to travel them at a breakneck speed.

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[10] One might link this arrangement to the Posthuma proposals under which, it is reported, central banks would keep the gold content of their gross reserves below a specified percentage.

DISCUSSION

J. Marcus Fleming: To facilitate a division of labor among discussants which I hope will increase our productivity, I propose to confine my comments exclusively to the subject of international liquidity, and hence to Mr. Kenen's paper.

Let me begin by confessing that I was surprised to hear Mr. Kenen say that the stock of reserve assets is adequate, for now and for the foreseeable future. Of course, a lot depends on how far you attempt to look ahead. Thanks largely to the rapid increase in wages in Continental Europe, there is now probably a better equilibrium in the basic balance of payments between industrial countries than in recent years. Partly for this reason, but mainly, perhaps, because of the strengthening of the emergency defenses of the international monetary system of which Mr. Kenen has spoken, the speculators and hot money men have gone into retirement, for the time. A relative calm has descended on the foreign exchanges, and the short-term need for reserves has in a sense been reduced. Nevertheless, there are a number of considerations that argue against a too complacent view. If, as I believe, the primary indicator of the desirability of larger or smaller international reserves is the deflation/inflation situation of the world, and particularly of those countries whose financial policies are likely to be influenced by the state of their reserves, and if, as many suspect, we are entering on a period when the pressure of aggregate demand is likely to be less marked than in the fifties, that consideration alone would tend to justify an increase in the level of reserves. So much for the static aspect. From the dynamic point of view, perhaps Mr. Kenen would agree, the actual and prospective rate of growth of reserves is much below any reasonable estimate of the long-run rate of growth in requirements. Over the last two years, the amount of gold in country reserves has increased by less than 1 per cent per annum and the ratio of foreign exchange to total reserves has increased only slightly from 35.9 per cent to 36.1 per cent. Indeed, after allowance is made for the change in the distribution of reserves between countries with different ratios, there has been a very slight decline in the average ratio. No doubt, the low level of monetary gold increase has been partly attributable to an abnormal amount of private gold hoarding which is unlikely to persist. But the tendency to a decline in the propensity of countries to hold their reserves in the form of foreign exchange has occurred despite (1) the possibly abnormally high level of the Canadian proportion of foreign exchange, (2) the new willingness of the United States to hold foreign exchange reserves, (3) the rather artificial increase in exchange reserves associated with bilateral swap transactions, and (4) the general restoration of confidence in the international exchanges, of which I spoke earlier.

There are two approaches to the problem of providing the world with the right amount and the right kind of external liquidity: a more or less bilateral intercentral bank approach and a multilateral more or less institutional approach. Up to a point these approaches are complementary; beyond that point they are probably competitive.

The intercentral bank approach has been prominent in recent years as a result of the short-term credits provided by various central banks to the United Kingdom in 1961 and as a result of the network of swap transactions and stand-by swap arrangements established over the last two years for the tactical defense of the dollar. In order that such credits and stand-bys should be genuinely helpful, they must be accompanied by understandings, however informal, regarding the willingness of the participating countries to go on holding such of their balances in each other's currencies as are not covered by the exchange guarantee which the swaps provide. Given these understandings, the arrangements can provide a useful first line of defense against speculative attacks.

However, I agree with Mr. Kenen—and Mr. Johnson—that loose co-operation between central banks does not provide a firm foundation for an international monetary standard. I even doubt whether the various amendments which he proposes to intensify and systematize that co-operation would suffice to make of it an instrument capable of providing for the requisite long-run growth in world liquidity. Both the swap stand-bys and the actual swap transactions are short term; they are bilateral; and they lend themselves to an undesirable interference of one national government or central bank in the internal policies of another. Their short-term character detracts from the quality of the liquidity which they confer.

Mr. Kenen's suggestion of making the mutual stand-bys permanent certainly improves matters, but sums borrowed under his stand-bys are to be repaid in six months, and there is a limit to the amount of international liquidity that can usefully be provided in the form of short-term borrowing facilities. Moreover, the long-term character of the stand-bys would intensify certain difficulties arising from their bilateral nature. A country's need to draw on its credit or its right to be repaid for a previous loan might coincide with a time when its partner was in difficulties. Mr. Kenen's other suggestions do not seem to me to meet this problem. To say that the proceeds of borrowing should not be converted into third currencies is meaningless in a world of free exchange markets when any currency can be used to support the domestic currency. To set up the bilateral exchange rate as an automatic criterion of which country in each pair has the right to borrow from the other is to overlook the fact that exchange rates are managed and can be manipulated, and so need not reflect the relative strengths of the balance-of-payments situations of the two countries. And even if exchange rates were reliable indicators of relative currency strengths, the criterion would not prevent weak currencies from being drawn upon to support even weaker ones. My conclusion is that automatic borrowing rights cannot be established on a bilateral basis. And this lack of automatism, together with the short-term nature of the accommodation provided, means that such bilateral borrowing facilities cannot provide an adequate substitute for the growth of reserves in asset form.

The only type of intercentral bank action that might really make a dent on the problem of liquidity growth would be some arrangement whereby monetary authorities could be relied upon to hold a steady and increasing proportion of their national reserves in the form of foreign exchange, and perhaps even in the form of particular currencies. Mr. Kenen does not discuss this question but deals with the related question of foreign exchange guarantees. I say "related" because it seems unlikely that countries would agree to cooperate in holding reserve currencies, to a greater extent, and more steadily, than comes naturally to them individually without some sort of exchange guarantee of official balances in terms of gold or creditor's currency. Perhaps these need not be 100 per cent guarantees. The idea of partial guarantees deserves further exploration.

I agree with Mr. Kenen—and by implication disagree with one of Mr. Johnson's conclusions—that the point of exchange guarantees is to give countries of reserve currency the same moral freedom to alter their exchange rates, subject to the approval of the international community as expressed through the IMF, as is possessed by other countries, while at the same time making it probable that any capital flight that may occur will be offset rather than reinforced by the movement of official balances. I agree, also, that the objection to this on grounds of cost is a poor one. Over any reasonable period of years, the guaranteeing country would be likely to make a substantial profit owing to the lower rate of interest that need be paid on guaranteed balances. However, it is very difficult for a country to take the initiative in offering exchange guarantees without giving an impression of its intentions that may be entirely erroneous.

Moreover, even with guarantees, a managed exchange standard, especially one with a multiplicity of reserve currencies, would be open to grave weaknesses and abuses, unless it were subjected to so many rules and regulations as to become, in effect, institutionalized.

I turn, therefore, to the institutional, or IMF, solution. This Mr. Kenen seems to write off, for the time being at least, though not in the long run, apparently because he feels that any IMF contribution to world liquidity is dependent on its ability to accept deposits or to make its obligations transferable. There seems to be a deep confusion here. Quite apart from the question of whether the Fund could accept deposits, it should be realized that there are potential and actual claims on the IMF that are just about as liquid and reserve-worthy as the deposits of a Triffinian bank. I am thinking not only of the claims that would accrue to countries lending money to the Fund under the General Arrangement to Borrow, but also of the existing net asset positions of countries in the Fund—known in the trade as Gold Tranche Positions—which confer on the possessor the right to receive the overwhelming benefit of the doubt on any drawings it may request. In retrospect, it is clear that the founding fathers of the IMF, in deciding that the Fund should have the form of a "mixed bag" of currencies rather than that of an orthodox deposit bank did it a great disservice, not so much because they prevented it from doing anything substantive that it has wanted to do, but rather because they have made its operations so mysterious to the outside observer.

The constitution of the Fund could no doubt be improved, but even as it stands it has the power to increase the amount of unconditional liquidity that it provides, in a very simple way, by extending gold tranche treatment, as it is called, to one or more of the credit tranches, and thus increasing the magnitude of the drawings on which members receive the overwhelming benefit of the doubt. And insofar as it can induce its members to increase the general level of quotas, or feels that its own liquidity is sufficiently secure to enable it to permit larger drawings without increasing quotas, it can expand that very valuable type of international liquidity about which, perhaps, too little is heard; viz., liquidity available on condition that appropriate policies are adopted by the drawing countries.

In order to ensure the Fund's own liquidity, Mr. Kenen makes two interesting suggestions: first, that the IMF be allowed to invoke the General Arrangement to Borrow at its own discretion and, second, that the lending commitment of each participant, instead of being of fixed amount, should vary with its gross reserves. I would personally agree that the first part of this proposal is desirable. As regards the second, I think that the general principle is right, but that it should not be forgotten that the Fund can replenish its resources, not only by increasing its borrowing potential, but also by increasing member quotas and hence the amounts subscribed in gold and domestic currency. These quotas determine not only a member's right to draw but also its obligation to provide resources to the Fund. Perhaps, therefore, it is not the credit arrangements alone but the sum of quotas and these arrangements which ought to be brought into some sort of relationship with national reserves. At this point, however, we have to realize that we are speeding rather fast along the road—or is it through the clouds—to Yale.

HARRY C. EASTMAN: What small fault can be found in Professor Caves's wise paper lies in the fact that his guides for the most appropriate foreign exchange rate policy are perhaps too subtle. He shows that, in a particular case, the decision should be affected by what is the "typical" cause of disturbances in the balance of payments and also by a comparison of the policies required to achieve external and internal balance. No doubt, fixed rates are less inappropriate in some cases than in others, because the surpluses or deficits automatically brought about may mitigate undesirable domestic developments. But the advantages of such aid in maintaining domestic stability over the cycle can easily be overemphasized in relation to the obstacles to long-run domestic stability created by policies required to maintain the fixed rate itself. A divergence typically arises between the fixed rate and the rate that would maintain equilibrium under reasonable price and employment conditions, because the latter has a nonzero trend in the long term. Even a country most likely to benefit from a fixed rate and best able to maintain it, namely, one with internally generated changes in the balance of payments that are a small proportion of national income, is placed in difficulties by a fixed rate as is shown by the present plight of that cradle of world-wide income fluctuations that is the United States. It was also this problem of changing trend in the equilibrium rate with which Canada was quite incapable of coping when on a pegged rate from 1945 to 1950.

Turning to the closely analogous problem of harmonizing policies affecting the balance of payments and those necessary for stabilizing domestic conditions, we are given the illustration of a country suffering from declining foreign demand. The tentative prescription is step-by-step devaluation with a pegged rate if the country is suffering from inflation (so that the deficit may moderate price increases) but flexible rates to maintain a zero foreign balance if the country suffers from deflation. This dichotomy between fixed and flexible rates is surely unreal. The contrast is between countries that can borrow for balance-of-payments purposes and those that cannot, and this is not systematically related to the type of exchange system in use. If a country cannot borrow on acceptable terms, a pegged rate cannot be used to maintain a deficit in the long run. If the country can borrow, no distinction can be made between the results of the two systems on internal balance, for a deficit can be achieved with a flexible rate as with a pegged one.

Canadian experience with a flexible rate provides illustrations of the working of the exchange mechanism and the flow of capital. Capital has not been so responsive that Canadian interest rates were rigidly linked to foreign rates; indeed the differential between Canadian and United States' long-term rates have, in recent years, at times exceeded the usual differential by 1 per cent. Nevertheless, differentials in interest rates induced very large inflows of capital, chiefly because Canadian borrowers turned to the United States market. (Parenthetically, Canadian experience with flows of long-term foreign capital not only reveals that such capital is interest elastic, but also that its reputed skittishness is illusory. The oft expressed fear of bankers and their friends about the prospective withdrawal of foreign capital before the threat of any departure from domestic policies of the most rigidly orthodox benevolence toward capitalists was ill-founded. Canada has very large budgetary deficits that are badly regarded by international financiers. It is not free of hostility to foreign investment and even of timid expression of this hostility in discriminatory policy, nor is expropriation of private firms unknown. Yet vast amounts of capital continue to enter in response to differentials in rates of interest and of prospective earnings.) Be that as it may, borrowing abroad reduced the ability of the monetary authority to curtail aggregate demand in Canada. Professor Caves apparently shares the view expressed at times by the Bank of Canada that its inability to control demand because of the interest-elastic capital flows was regrettable and indicated that policy instruments were inadequate. It is unfortunate that time did not permit him to deal more fully with this proposition which is far from self-evident. The inflow of capital provided resources for investment and other wants and, by increasing supply, complemented the purpose of monetary policy in restricting demand. With a flexible rate, international capital movements induced by monetary policy furthered the achievement of the central bank's goals with respect to the price level or employment.

However, it might be that a decision to restrict the money supply is made, not to protect the price level, but perhaps to restrict the volume of investment which the government might believe at a moment of time to be excessive in terms of its future profit possibilities. Public opinion in Canada, unlike that in many other places, has not yet concluded that the analytical ability of government is sufficiently superior to that of everybody else that we ought to opt for one big mistake rather than many small ones, a skepticism well founded in the light of the policies which we have actually experienced in the past few years, some of which Professor Caves has explained. However, now that we are back on a pegged rate like every other country, the mutual congratulation such harmony brings and the self-reinforcing evidence of wisdom it provides will give us greater faith in the judgment of our leaders and courage to face the hardships that that policy ineluctably brings, as everyone in the United States presumably realizes by now.

J. HERBERT FURTH: Ever since I was first asked to participate in this meeting, I have been looking forward to discussing conditions of international monetary equilibrium under fixed exchanges. How can such equilibrium be preserved under domestic policies for maximum employment and maximum economic growth, under an international payments system based on reserve currencies rather than on gold, and with huge transfers of government funds impervious to market forces?

Unfortunately, Professor Johnson's paper does not deal with any of these subjects, but merely compares the existing system of fixed exchanges with an ideal system of a uniform world currency.

Confronted with a choice between discussing my assigned subject and discussing Professor Johnson's presentation, I prefer to attack the live target. In order to keep within my allotted time, I shall deal only with five subjects: Professor Johnson's three concluding observations; his initial statement about the international monetary system; and his views of the central banking fraternity.

1. I agree with Professor Johnson's first concluding observation that the advantage of a system of fixed exchanges lies mainly in its unifying effect, and that this effect presupposes freedom of international competition. But I disagree with his contention that the existing payments system has not been conducive to such freedom. In recent years restrictions on the international movement of goods and capital have been eliminated at a pace and to an extent that all of us would have considered impossible a short time ago. And most recently, the Trade Expansion Act has taken an initial step toward solving the problem of relief for U.S. enterprises and individuals damaged by international competition—a relief Professor Johnson rightly considers an essential supplement to free competition. Incidentally, this step was suggested ten years ago by one of those wicked central bankers—Al Neal, then Vice-President of the Federal Reserve Bank of Boston.

2. I also agree with Professor Johnson's second observation that a single international currency would be even more unifying than a system of national currencies with fixed exchanges. But such a currency will obviously

remain out of the question for the foreseeable future—and I have an uneasy feeling that this is the reason why Professor Johnson is so enthusiastic about it. More importantly, the difference in unifying effects between the present system and Professor Johnson's ideal is smaller than Professor Johnson believes. International commercial and financial transactions are not seriously hampered by intramarginal exchange rate movements. And only on rare occasions are they seriously disturbed by fears of devaluation or revaluation of a major currency; and central banking co-operation can go a long way to curb disruptive effects of such fears. Moreover, such fears usually occur because a major country is in bad political trouble or because its policies are incompatible with its underlying economic situation; such trouble and such policies would have adverse effects under any system of international payments.

3. I disagree with Professor Johnson's third observation that the world needs a genuine international central bank, whose first responsibility would be to provide for an increase in international reserves.

It seems inconsistent for a paper that expresses such a poor opinion of central bankers to plead for a central bank that would be bigger and more powerful than any of the existing national institutions. And while it would be bigger, it would certainly not be better: at best, it would be controlled by the same forces that today control the central banks of the major countries.

More important, an increase in world liquidity is the last thing the present international payments system needs. This statement is not "ritualistic," as Professor Johnson believes, but follows from the fact that none of the urgent problems of our international financial situation can be attributed to a shortage of international liquidity. The first of these problems is the inability of many less developed nations to pay for all the imports they would need for maximum economic development. But the plight of these nations could no more be lightened by a general increase in international liquidity than the plight of a southern sharecropper could be lightened by a general increase in bank liquidity in the United States.

The second major problem is the continuing deficit in the U.S. balance of payments. If the surplus countries were prepared to finance our deficit forever and ever, the difficulties caused by the deficit might indeed disappear. But their unwillingness to do so would hardly be eliminated by a further rise in their international reserves, which are already excessive rather than deficient.

4. One root of Professor Johnson's errors is found in his opening statement in which he deplores forty-five years of chronic international monetary disorder. The present international payments system, with its mixture of national and international institutions, of gold and reserve currencies, of market forces and government intervention, is ideed about as disorderly as, say, the constitutional system of the United States. But it seems to work about as well. Under this system, the world has experienced its most rapid economic growth, in the domestic economies of its major members as well as in international trade and finance, together with an unprecedented absence of cyclical dis-

turbances and a remarkable improvement in income distribution. The burden of proof seems clearly to fall on those who claim that the world would have done as well or better under another system.

5. Another root of Professor Johnson's errors is his poor opinion of central bankers. The present system can indeed function only if central bankers show a modicum of competence. But is Professor Johnson right when he believes that their performance has been poor and that they are inherently conservative and orthodox, lacking awareness of economic change?

I cannot go into the details of these indictments. For instance, I cannot defend myself against the accusation of having recently produced "the most solemn exercises in 'primrose pathos' ": I have not the faintest idea what this means, and even whether it is a compliment or an insult. Also, I am not sure what economic attitudes are or are not conservative and orthodox. Does Professor Johnson mean the orthodoxy of Cambridge, England, or of Cambridge, Massachusetts; of Chicago, Illinois, or of my native Vienna, Austria? But whatever he means, it is difficult for an economist who was introduced to central banking under the regime of Marriner Eccles to view central bankers as either incompetent or inherently conservative and orthodox.

Central bankers, like academic economists, have indeed sometimes failed to spot a cyclical turning point. But more often, they have recognized changes and shifted their policies accordingly long before our academic colleagues became aware of them. If central bankers had waited for academic economists, for instance, to tell them of the change in the international position of the dollar, they would have continued to pursue policies appropriate for a period of dollar shortage as late as in 1958 and 1959.

Although my comments may sound as critical of our academic colleagues as Professor Johnson's paper is of central bankers, I do not underrate the potential contribution of academic economics to central banking. Central bank economists have little time for fundamental research. But our academic colleagues can help us only if they are not content to deal with economic models of their own making, but, like us, face the world as it is.

PROBLEMS OF REGIONAL INTEGRATION

EUROPEAN ECONOMIC INTEGRATION AND THE PATTERN OF WORLD TRADE

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The purpose of this paper is to examine the impact of European economic integration on the pattern of world trade, specifically with respect to third (nonmember) countries.

The first section is devoted to a theoretical presentation of the major economic effects of integration on trade and on welfare both from a short-run and long-run standpoint. The theory underlying the tools used to measure quantitatively the effects of integration on third countries is also discussed.

Section II applies the above tools to measure quantitatively the initial impact of European economic integration within the EEC on the imports from third countries. It will be seen that the initial impact is essentially the short-run consequence of tariff discrimination consequent to economic integration assuming constant capacity, efficiency, and tastes in the integrating countries.

Section III, finally, relaxes the *ceteris paribus* assumption and attempts to estimate the effects of integration on growth (and capacity) and the resulting demand for imports. Very rough orders of magnitude of the demand for imports into the EEC from abroad are presented for 1975 on the basis of different assumptions concerning the rate of growth of GNP.

It should be noted at the outset that the applied parts of this study examine the effects of integration within the Six mainly on their demand for imports from lesser developed countries.

No attempt is made at estimating the over-all welfare implications of regional economic integration, in general, or even European integration. Likewise, the short-run and long-run changes in the pattern of trade which might result within the EEC and as between the Community and the industrialized nations are not explicitly discussed.[1] At the same time the methodology underlying Sections II and III would appear to be applicable with some modification to other regional groups such as the Latin-American Free Trade Association (LAFTA). A less presumptuous and more accurate title for this paper would be: "European economic integration and the underdeveloped world."

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[1] This task is left to the other two papers to be presented at this session, with the qualification that Dr. Krause will examine the effects of European integration on the United States specifically.

I

The treatment of the welfare implications of economic integration has over the last decade evolved from the path-breaking but rather restrictive contribution of Viner [1] to the generalized and systematic formulations of recent authors such as Lipsey and Lancaster (see particularly [2], [3], [4], and [5]).

Very briefly, it is possible to distinguish between the typically static and the typically dynamic effects of integration. The static effects consist of the production, consumption, and terms-of-trade effects, whereas the dynamic effects result from the interrelationship between the size of the market and economic growth, the possible existence of scale and external economies in a fusion of national markets as well as the impact of integration on market structures (and competition), investment, and the introduction and application of technological change (see [4] and [6]).

From the standpoint of nonparticipating countries, the static effects usually will be negative and the dynamic effects usually will be positive. The former results from the discriminatory consequences of the elimination (or reduction) of intraunion tariffs while external tariffs are maintained. The negative production effects take the form of Vinerian trade diversion; i.e., the substitution of a higher cost source of supply of a given commodity within the union for a lower cost source of imports outside the union (intercountry substitution of a given commodity). The negative consumption effects increase the possibility of intercommodity substitution within the union in favor of union-produced goods and as against extra-union goods. Finally, trade diversion leads to a worsening of the net terms of trade of the outsiders vis-à-vis the insiders, by reducing the import prices and increasing the export prices of the union, at least under the normal assumption of upward sloping supply curves and downward sloping demand curves.

The dynamic effects would follow from the consequences of a larger common market on productive efficiency, investment (both domestic and foreign), increased competition, and the development of new technology. By accelerating over-all economic growth within the union the demand for imports will rise via the real income effect (assuming constant domestic prices) moving the terms of trade against the union. This tendency will be further reinforced by the price reduction of rts) through the improvement in productive the case where the price reductions are confined mport-substitute sector [7]. In the long run, therefore, ticipating countries can expect to be faced with a larger for their exports compared to the demand they would face im- diately after the imposition of a discriminatory tariff. It is clear that the latter will only benefit from a process of economic integration elsewhere if the dynamic effects outweigh the static effects.

There appears to be little controversy concerning the direction and the rough magnitude of the static effects (at least the production effects) on the outsiders. With dynamic effects, on the other hand, the range of opinion as to their magnitude varies from negligible to considerable. In any case the causal relation between integration and growth for which there would appear to exist valid a priori grounds is extremely difficult to establish in practice.

Very few attempts have been made to estimate these effects empirically.[2] Specifically with respect to the impact of the EEC on third countries, Tinbergen [13] examined the initial impact of discrimination and Erdman and Rogge [14] attempted to give rough orders of magnitude of both the preference and growth effects of the EEC (which are roughly equivalent to the static and dynamic effects in the present terminology).

In order to measure the initial impact of integration on third countries it is necessary to assume constant capacity and constant productive efficiency within the union. The elimination of tariffs on intra-EEC trade and the granting of essentially similar treatment by the Community to imports from the associated overseas members (AOM), while maintaining a common external tariff, introduce an element of discrimination against third countries. The rate of discrimination can be defined (see [13]) as the difference between the import duties levied on imports from outsider countries and the duties imposed as an average on all imports. The latter consist partly of imports into the EEC from partner countries and the AOM and to that extent are exempt of tariffs. In general the average duty will vary between zero (if all imports of the given commodity originate within the EEC and AOM) and the comon external ad valorem tariff rate (if all imports of a commodity originate outside the broader community).

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[2] Verdoorn [8] and Janssen [9] estimated the static effects of a Free Trade Union and the EEC, respectively, at a very high level of aggregation. Verdoorn [10] and Kristein [11] attempted to go beyond the purely static effects and discuss some of the dynamic effects of the Benelux experience. Johnson [12] calculated the static effects which would result for the U.K. in the presence as opposed to the absence of a Free Trade Area.

For any commodity i and any third coun extent of the discrimination depends on (1) the share of j's exp Community, (2) the share of the total exports o of country j,[3] (3) the common external tariff rate o the share of imports of i into the Six coming from partn the AOM. This rate of discrimination can be calculated fo modity i exported by third country j to the EEC. It can also be pressed as a weighted average of all exports of j to the EEC or on exports from j to the world (see [13, pages 394-95] for formulæ used to compute the rate of discrimination).

The above defined rate measures the initial impact of the creation of the union on third countries. It is the amount of import duties which the latter have to pay (expressed as a percentage of their exports to the EEC or to the world as a whole) in addition to what the average importer into the EEC has to pay, based on the present or recent volume of trade. It represents the initial and immediate relative loss of export receipts after the institution of the union.

But as a partial equilibrium tool the rate of discrimination does not indicate the price and volume adjustments which will be triggered by the discrimination. These will depend on the various supply, demand, and substitution elasticities.

Other measures of the static effects of the creation of a union along Marshallian lines have been offered by Scitovsky [6][4] and H. G. Johnson [12], the latter providing an ingenious device to compute the "maximum-loss estimate" from nonparticipation as opposed to participation in a union. From the standpoint of the nonmember country the loss from trade discrimination on the export side (equal to the gains from free trade foregone because the country is not included in the union) amounts to the loss of income that would result if the productive factors employed in meeting the additional demand created by the union had to be diverted to producing for the domestic or other foreign markets.

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[3] Symbolically:

$$\frac{{}^iX^j_{EEC}}{{}^iX^j_{\text{Total}}} \cdot \frac{{}^iX^j_{\text{Total}}}{\sum {}^iX^j_{\text{Total}}} = \frac{{}^iX^j_{EEC}}{\sum {}^iX^j_{\text{Total}}}$$

where ${}^iX^j_{EEC}$ is the value of exports of commodity i from country j to the EEC ${}^iX^j_{\text{Total}}$ is the total value of exports of i from j, and $\Sigma^iX^j_{\text{Total}}$ is the total value of exports (summed over all commodity groups) of j to all destinations.

[4] The loss of welfare between union and nonunion members is, according to Scitovsky, "equal or less than $\Sigma(\Sigma t_i d_i + \Sigma t_e d_e)$ where the t_i's are duties levied by member countries on imports from nonmembers, the t_e's are duties to which member countries' exports to nonmembers are subject, the d_i's and d_e's are, respectively, the changes in member countries imports from and exports to nonmember countries, and the summation extends over all goods entering into trade between them and nonmembers" [6, pp. 58–59].

The maximum loss which third countries would have to incur would be if all prices of export goods to the union had to be reduced so as to exactly offset the external tariff. This maximum-loss estimate would be the value of exports of a country to the union, multiplied by the proportion in the prevailing price of the union of the external tariff rate which has to be offset. It can readily be shown that this loss amounts to $V\left(\frac{t}{1+t}\right)$ where V stands for the value of exports to the union and t for the ad valorem tariff rate.[5]

It should be noted that the above formulation applies, strictly speaking, to the case of a country which has the opportunity to join a union and which is interested in the maximum loss which would result from nonparticipation. (It was applied to evaluate the gains from the U.K. from a Free Trade Association covering the OEEC countries as opposed to a Common Market, exclusive of the U.K., on the basis of forecasts of U.K. exports in 1970 under a Free Trade set up.) At the same time, if certain assumptions are made, this approach appears to be also applicable to third countries which have no possibility of being inside the union. The above tool could, then, be computed on the basis of the present trade values and it would indicate for outsiders the loss in terms of export receipts (resources) necessary to compensate for the discriminatory tariff and still sell exports competitively at the outset of integration. The maximum-loss concept should rigorously be calculated only on the basis of the export value of a given country (assumed within the union) after at least the production effects of integration have materialized.

Neither the consumption effects of integration (intercommodity adjustments) nor the adaptive terms-of-trade adjustments are measured by the "rate of discrimination" or included in the above discussed welfare loss for exports of third countries. They are, however, implicit in the "maximum-loss estimate" when the latter is computed on the future value of exports of a country assumed to be inside the union after all primary static integration effects have taken place.

It is interesting to note that adaptive price changes to restore equilibrium between the insiders and the outsiders would appear to be relatively small.[6] Verdoorn [8] calculated the appreciation of European member countries' currencies necessary for the maintenance of balance-of-payments equilibrium with the outside world and found it to be of the order of 2 to 10 per cent. In a more recent study Janssen [9] showed the change in the terms of trade of union members vis-à-vis outsiders as a function of the common external tariff. It is obvious that the extent to which outsiders will gain or lose by the customs union depends on the level of the common external tariff and on the initial level of duties of member countries before integration. A relatively low common external tariff could move the terms of trade in favor of outsiders and, even, a common tariff equal to the average incidence of the national tariffs prior to the creation of the union could move the terms of trade against the initially more protectionist members countries.

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[5] Johnson [12] computes correctly the maximum-loss estimate for the U.K. on the basis of the above formula, but on p. 249, fn. 1 it is erroneously stated that the proportion by which V has to be multiplied is $\left(\frac{t}{1-t}\right)$.

[6] If t is the specific tariff, p the reduction in domestic price when the tariff on partner imports is removed, M_f the initial volume of imports from foreign countries, M_s the volume of trade

It seems relevant before concluding this discussion of the static effects of integration on nonparticipating countries to remember that the few highly aggregative attempts made at measuring empirically the welfare losses from trade diversion place these losses at a relatively modest level. On the basis of 1952 figures and on the assumption of a union comprising the U.K. and the Scandinavian countries in addition to the Six, the loss to the rest of the world through trade diversion and a worsening in the terms-of-trade position is placed at $540 million out of total exports of the rest of the world to the union of $10 billion [6, page 66]. This represents an order of magnitude of perhaps 3 per cent on all exports from outsiders and, as such, compares with some of the annual changes which have taken place in recent years in the terms of trade between primary products and manufactures. Furthermore, the static effects of integration have a once and for all repercussion on the outside world.

Attempts to quantify the dynamic effects of integration are essentially taxomonic in nature and take the form of general "navigation tables"; i.e., postulating higher rates of growth for an integrating area then would obtain for the same group in the absence of unification. A clear-cut distinction between the static and the dynamic effects is not made (with one possible exception [14] discussed below), probably because of the methodological difficulties inherent in, first, trying to distinguish between them conceptually, second, devise ways to quantify them on a comparable basis, and, third, the implicit inclusion of static

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diverted by the change, then the above losses would be equal respectively to: $p(M_f - M_d)$ and $\frac{1}{2}pM_d$. It can also be shown that $p = \dfrac{s_p t}{d + s_h + s_f + s_p}$ where d is the slope of home demand curve (defined to have positive sign), s_h, s_f, s_p are the slopes of the domestic, foreign and partner supply curves respectively—all slopes referring to the price axis [15, p. 45].

production effects in long-run forecasts of union members and foreign production and consumption data.[7]

A modest attempt is made in the following sections to compute roughly the static and dynamic effects of integration within the EEC on imports from third countries and more specifically the nonindustrialized countries. The importance of this question derives from a number of considerations. First, export receipts are a major determinant of the possible growth rate of the underdeveloped countries. Second, the process of integration within the EEC will affect underdeveloped regions (and export commodities) unevenly and thereby influence the development prospects of these regions, selectively. Last, since some flexibility exists within the Treaty of Rome for tariff and other forms of adjustment, such as duty-free quotas (Article 25) to correct serious damage occasioned to third countries through trade displacement, an analysis of the future changes in the export pattern of the nonindustrialized world may help generate corrective measures.

II

In the preceding part a number of concepts have been discussed which can be used to assess the short-run (static) effects of integration. Two of these tools—first the rate of discrimination, followed by the maximum-loss estimate—are used here to measure the initial impact of the Community on outsiders. Table 1 shows the rate of discrimination computed in detail for a few specific countries and Table 2 shows this rate on total exports for a number of countries.[8]

A few observations can be drawn from these figures. As was pointed out previously, the countries which will be most discriminated against are those who (1) face a high common tariff, (2) export goods to the Community, a high proportion of which originates within the EEC or the AOM, and (3) direct a large share of their exports to Europe. Taking coffee as an example, about one-fourth of total imports into the Six originates in the AOM. In some of the member countries, however, this proportion is much larger (roughly two-thirds in both France and Italy). The relative discrimination, being a function of the share of imports satisfied from within the union and the tariff (16 per cent ad valorem), is therefore quite high, amounting to about 11 per cent

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[7] In this connection it is interesting to note that H. G. Johnson [15] uses estimates of British exports to a (hypothetical) Free Trade Association for 1970 which assume a greater increase in GNP in the presence of a FTA than without it. The static gains from freer trade are thus—as he points out—overestimated, since at least some of the dynamic effects are implicit in the higher growth rate.

[8] It should be noted that the rates in Table 2 are based on 1955 export figures. The order of magnitude, however, would remain essentially the same for more recent years.

TABLE 1

RATES OF DISCRIMINATION* FROM EEC

Commodity	European Tariff Per cent	Rate of Discrimination
India (1)		
Coffee	16	3.1
Tea	26	.16
Wool (rough)	3	1.7
Wool	3	.5
Crude minerals	4	2.1
Crude vegetable matter	3	1.4
Textile yarns, thread	12	7.8
Textile articles	15	1.7
Fodder, oil seeds	0	0
Iron and steel scraps	0	0
Weighted average		1.7
Rate of discrimination on total exports		.1
Argentina (2)		
Extract of beef, canned meat	3	0.5
Eggs	13	6.2
Wheat	15	1.4
Paddy	12	1.7
Barley	12	1.1
Maize	10	0.2
Rye	10	0.6
Hides, skins, wool	0	0
Linseed oil	12	3.4
Extract of quebracho	10	1.8
Weighted average		1.0
Rate of discrimination on total exports		0.28
Japan (2)		
Fish and fish products	21	3.2
Timber	6.5	0.5
Silk	5	0.9
Animal oils	9.8	0.6
Organic chemicals	11.4	4.2
Cotton manufacture	15.9	7.9
Other textile manufactures	17.1	11.0
Pearls, precious stones	0	0
Iron, steel, and alloys	9.5	7.1
Copper	2.9	0.6
Nickel	2.9	1.3
Weighted average		4.5
Rate of discrimination on all exports		0.15
Brazil (1)		
Coffee	16	5.6
Cocoa	9	2.5
Sugar and honey	80	42.4
Tobacco	30	4.8
Cotton	0	0
Iron ore	0	0
Weighted average		4.5
Rate of discrimination on total exports		1.0
Greece (2) (3)		
Dried fruits	15	0.9
Alcoholic beverages	108	76
Raw tobacco	30	2.1
Hides and skins, untanned	2.5	0.5
Cotton	0	0
Raw minerals	0.2	0.02
Nonferrous ores	0.3	0.07
Vegetable oils	6.6	1.2
Manufactured oils	7	0.4
Weighted average		3.5
Rate of discrimination on total exports		1.63

* Calculated as a percentage of value, on exports of main commodities to EEC, or total exports to EEC and on total exports. For definition see text and [13].

SOURCE: [13] and

1. Based on 1960 data.
2. Based on 1955 data.
3. Greece has become an Associate member of the Community. The reason for including Greece is to indicate the degree of discrimination which it would have been subjected to in the absence of integration.

TABLE 2

RATE OF DISCRIMINATION* ON TOTAL EXPORTS

Australia	0.03
Austria	1.52
Brazil	0.73
Burma	0.07
Ceylon	0.21
Colombia	0.59
Costa Rica	1.38
Cuba	5.00
Dominican Republic	3.52
Ecuador	1.47
Greece	1.63
Guatemala	0.50
Honduras	0.60
Iceland	0.80
Iran	1.12
Iraq	0.26
Liberia	0.00
New Zealand	0.13
Nicaragua	0.39
Pakistan	0.00
Portugal	0.50
Salvador	0.88
Saudi Arabia	0.00
Spain	1.49
Thailand	0.08
Turkey	1.19
United Arabic Republic	0.11
Uruguay	0.08
Yugoslavia	0.88

*Based on 1955 figures.
SOURCE: [13].

(= ⅔ × 16 per cent) in the latter two countries. The rate of discrimination for Brazilian coffee exports to the Community as a whole comes out at 5.6 per cent. The third countries which will be subjected to the largest relative discrimination are the small suppliers of heavily protected goods, a high proportion of which originates within the union. At the limit, the marginal suppliers of a commodity originating almost exclusively within the union are faced with a rate of discrimination equal to the common external tariff, since the average import duty in this case approximates zero (intratrade).

In general, the initial impact of the EEC on third countries appears to be relatively small relative to the total exports of these countries. At the same time this impact can be relatively large for certain commodities and, even, relative to the exports of a third country to the Six. It seems fortunate that, on the whole, very few outside underdeveloped countries experience both a high degree of geographical concentration of exports to the EEC and a commodity concentration on heavily protected goods.

Since the export dependence of most nonindustrialized areas (excluding the AOM) on the Community is lesser than and since their commodity dependence on low tariff commodities (raw materials) is greater than most of the European countries outside the Six, it is to be expected that the latter would encounter a higher initial negative impact. Table 2 confirms this argument (at least for those countries shown).[9]

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[9] Greece is included in the computations, even though Greece has become an associate member of the Community and thereby free from any discrimination, to indicate the cost of nonparticipation.

In order to try to estimate the static and the dynamic effects of integration it is necessary to formulate a method whereby these effects can be distinguished on a comparable basis. Essentially the question can be raised in terms of the impact of the Six on their imports requirements from third countries at some future date both in the presence and in the absence of integration. A future date has to be selected in order to allow the dynamic effects (whatever their magnitude) to influence the relevant economic variables. It is assumed that real GNP reflects the impact of integration on growth and is a good indicator thereof.[10]

From the standpoint of the EEC it is likely that the process of economic integration will result in a higher level of real GNP achieved in the future. No attempt is made, here, to predict the growth rate which would obtain with or without integration.[11] Instead a range is selected with the assumption that by some future date (1975) the level of real GNP will tend towards the upper limit under the forces of integration and towards the lower limit without integration. The selection of 1975 as a future bench mark is mainly dictated by the availability of production and consumption projections for Western Europe which used that year as their target [18, 19, 20, 21, 22].

Briefly, integration within the Six by its effect on factors such as competition, technology, efficiency, and investment may lead to a higher level of GNP than would otherwise prevail. The total requirements of the Community corresponding to the assumed level of GNP in 1975 can be estimated on the basis of empirically derived income elasticities of demand. Accelerated growth will encourage an increase in intra-EEC and AOM production and intratrade. Estimates of supply response and output changes corresponding to changes in national income can be used to derive the above data. Access to a larger protected market and the possibility of a larger flow of public and private investment from the Six to the AOM under integration conditions should be reflected in a rise in AOM output and exports to the Continent. Net imports from third countries could then be estimated as the residual to fill the gap between total EEC consumption requirements and EEC output in addition to imports from the AOM in 1975.

It appears likely, on the other hand, that without integration a lower level of GNP will be attained in the Six. One major change which this would entail—besides reduced over-all consumption requirements, domestic production and intratrade in the Six—is a lower rate of output in the AOM since the latter would not enjoy preferential (tariff free) treatment in the whole Community but only in their own metropolis. The lack of a guaranteed preferential market for AOM exports would inhibit investment into new capacity from the Continent to the periphery and no displacement of third countries' exports by AOM exports should be expected under the circumstances.

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[10] It is, of course, true that the choice of any unidimensional concept as an indicator of growth (i.e., real GNP) is somewhat arbitrary and reduces a multidimensional process to a unidimensional one.

[11] For a discussion of the qualitative factors which are expected to lead to higher growth under integration see [4, Part II], and [6].

The major methodological difficulty in long-range projections consists of converting volume estimates into value figures. Specifically from the standpoint of exploring the impact of integration on third countries' exports it would be desirable to trace through, first, the price changes in the Community resulting from the elimination of tariffs on intratrade and AOM exports through the production effects. This can be done along partial equilibrium lines given a knowledge of the various demand and supply elasticities (and slopes) as was indicated in the previous section. The next step would be to work out the intercommodity adjustments (consumption effects) and the terms-of-trade changes necessary to restore balance-of-payments equilibrium between insiders and outsiders. (For a good conceptual analysis of these steps see [9].) After having allowed for the static repercussions on prices and volumes, the dynamic effects on efficiency (cost of production), capacity, and growth should be taken into account in order to determine the ultimate changes in prices and volumes.

III

In the absence of a combination growth and general equilibrium model in which these various relations would appear explicitly (and quantitatively) and where the system could be solved for the equilibrium values of the relevant variables in 1975 for both the case of union and nonunion, a number of heroic assumptions will be made in estimating net imports from third countries and the static and dynamic effects of integration.

In general no changes in relative prices are assumed and future value figures are given in current purchasing power. Estimates of net imports of raw materials and foodstuffs into the EEC are made for 1975 corresponding to two real GNP levels. For any commodity, two levels of demand would exist. In order to estimate the static (production) effects of integration the "maximum-loss estimate" is applied to the value of net imports corresponding to the lower GNP level. (These

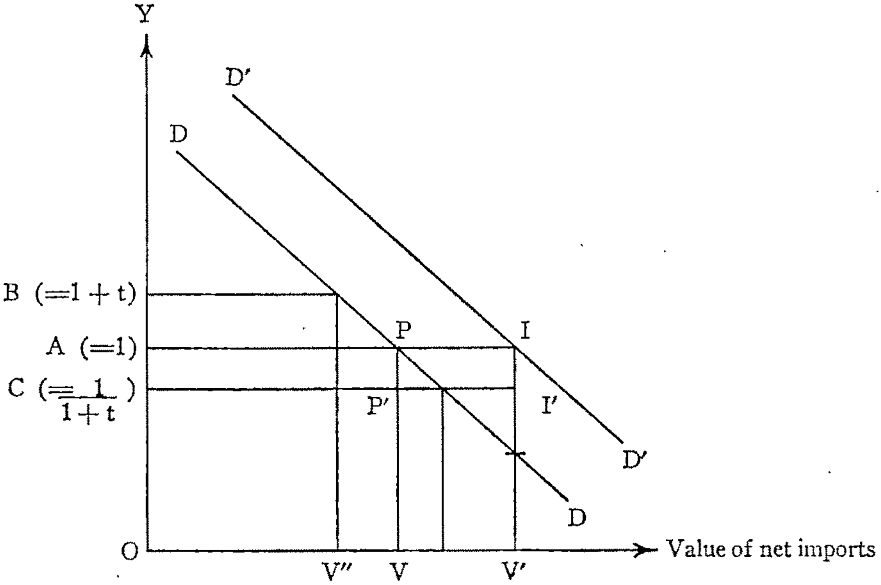

FIGURE 1

effects are really comparative static effects.) Estimates of the dynamic effect of integration are derived from the difference between the levels of net imports corresponding to the two GNP rates. Since the demand for net imports of the Six is dependent on changes in domestic and AOM output, it is, of course, possible to obtain negative dynamic effects, i.e., for a number of temperate zone agricultural commodities. Conceptually, whether the future Community net import demand for a given commodity after union is to the right or the left of the demand, assuming no integration, depends on the European and AOM supply response and the income elasticity of demand. The lower the income elasticity of demand in the EEC and the higher the production elasticity in the EEC and AOM (possibly because of a high level of protection) for any given good, the greater will be the leftward shift in the net imports demand curve and the negative dynamic effects of integration.

Figure 1 illustrates these effects, *DD* and *D'D'* are the demand curves of the Six for net imports in 1975, respectively, with and without integration. *OV* is the value of net imports without and *OV'* the value with integration. The European price is assumed to be the same in both cases; namely, *OA* (= 1). This assumption is not as unrealistic as may appear at first. It has been seen that the static and the dynamic effects tend each in turn to set up adaptive price changes in the direction of maintaining price stability.[13] If an ad valorem tariff rate t is applied on imports from third countries (assuming AI to be the export supply curve of outsiders to the union) then, in order to offset exactly the effect of the tariff, outsiders will have to reduce their price by AC $\left(= \frac{t}{1+t}\right)$. Johnson's "maximum-loss estimate" as a measure of the static (production) effects would be $ACP'P$ or the value of imports (OV) multiplied by $\left(\frac{t}{1+t}\right)$. The dynamic effects would be approximated by $VV'I'P'$ $(=VV'PI - PP'I'I)$ or the positive (or negative) difference between the value of net imports in the presence and absence of integration (VV'), multiplied by $\left(\frac{1}{1+t}\right)$ (this last term being equal to $OA - AC$).

The above definition of the dynamic effects is strictly valid only if either one of the following conditions is met: (1) The additional resources necessary to produce the increased volume of exports (in the case of positive dynamic effects) were previously idle or disguised unemployed, and the resources released (in the case of negative dynamic effects) have no alternative use. Thus, the quantitative estimates of these effects are maxima, overstating these effects (both positively and negatively to the extent that the above conditions are not met). (2) The demand curve DD shows unitary elasticity over the relevant range and resources are fully employed and specialized (VP and $V'I$ being the relevant supply curves). The dynamic effects would be larger than indicated here if DD was inelastic and vice versa.

Before giving empirical estimates of the impact of the EEC on third countries for 1975 the present pattern of imports into the Community

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[13] The static production effects will tend to reduce prices within the union as a result of the internal tariff elimination (or reduction) which will lead to trade diversion with outsiders. The balance of payments moves in favor of the union which necessitates an adaptive price rise (or currency appreciation) domestically to restore static equilibrium. The dynamic integration effects will tend to increase real income and reduce the union price level, through improvements in efficiency. Both factors will tend to move the balance of payments against the union and in favor of third countries. Again adaptive price changes will be triggered moving the terms of trade against the union. This is equivalent to a currency devaluation which leads to an increase in the price of imports (in domestic currency).

TABLE 3

IMPORTS, EXPORTS, AND NET IMPORTS OF PRIMARY PRODUCTS INTO THE EEC, ANNUAL AVERAGE 1953–55 (Million Dollars)

COMMODITY CLASSES AND DESCRIPTIONS IN TERMS OF THE SITC	TRADE AMONG THE SIX COUNTRIES (F.O.B.)	IMPORTS (C.I.F.) FROM							
		Other West Europe	Overseas Territories	North America	Latin America	Overseas Sterling Area	U.S.S.R. and Eastern Europe	Rest	Total* Third Countries
Foodstuffs (0, 1, 22, 4)	*722*	*494*	*1,022*	*631*	*683*	*342*	*86*	*539*	*3,797*
Livestock and products (00, 01, 02)	242	188	3	23	13	19	23	48	317
Cereals (04)	70	58	103	327	155	63	41	45	792
Fruits and vegetables (05)	221	47	180	37	42	20	8	149	483
Sugar (06)	13	6	46	1	43	—	2	7	105
Coffee, tea, cocoa (07)	20	7	226	4	348	117	—	64	766
Oilseeds, fats and oils (22, 4)	41	57	202	139	33	110	5	162	700
Tobacco (12)	9	47	18	71	17	5	3	27	188
Other food and beverages	106	84	244	29	32	8	4	37	430
Raw Materials (2 excl. 22 and 266, 682–9)	*690*	*847*	*503*	*633*	*408*	*1,124*	*92*	*645*	*4,252*
Rubber (23)	1	1	11	32	—	144	—	58	246
Wood (24)	38	205	49	20	7	14	47	113	460
Woodpulps (25)	15	161	—	27	—	—	2	37	227
Natural textile fibers (26 excl. 266)	179	118	66	269	246	691	14	209	1,613
Iron ore (281)	50	127	25	9	13	8	—	33	215
Nonferrous ores (283)	7	22	74	14	14	55	5	72	256
Nonferrous metals (682–9)	198	111	181	100	69	67	2	10	540
Ferrous and nonferrous scrap (282, 284)	67	17	9	93	2	12	1	3	137
Other raw materials	135	85	88	69	57	133	21	105	558
Fuels (3)	*616*	*99*	*60*	*210*	*92*	*798*	*65*	*344*	*1,663*
Coal and coke (311)	499	74	3	150	—	2	44	3	276
Crude petroleum (312)	—	—	17	4	75	789	5	331	1,221
Petroleum products (313)	103	25	40	56	17	7	16	10	171

COMMODITY CLASSES AND DESCRIPTIONS IN TERMS OF THE SITC	EXPORTS (F.O.B.) TO							
	Other West Europe	Overseas Territories	North America	Latin America	Overseas Sterling Area	U.S.S.R. and Eastern Europe	Rest	Total* Third Countries
Foodstuffs (0, 1, 22, 4)	*535*	*274*	*157*	*53*	*114*	*86*	*156*	*1,375*
Livestock and products (00, 01, 02)	109	60	57	18	34	23	37	338
Cereals (04)	66	37	3	2	11	20	32	171
Fruits and vegetables (05)	181	26	28	7	12	18	16	288
Sugar (06)	16	77	5	1	16	3	16	134
Coffee, tea, cocoa (07)	33	6	14	1	5	3	5	67
Oilseeds, fats and oils (22, 4)	19	13	11	10	5	5	10	73
Tobacco (12)	11	4	3	—	2	2	1	23
Other food and beverages	100	51	36	14	29	12	39	281
Raw Materials (2 excl. 22 and 266, 682–9)	*364*	*32*	*157*	*37*	*15*	*48*	*87*	*740*
Rubber (23)	1	—	—	—	—	—	2	3
Wood (24)	29	12	—	2	—	1	3	47
Woodpulps (25)	5	—	—	—	—	—	4	9
Natural textile fibers (26 excl. 266)	104	4	16	3	1	27	26	186
Iron ore (281)	9	—	—	—	—	—	—	9
Nonferrous ores (283)	6	—	4	—	—	1	—	11
Nonferrous metals (682–9)	117	10	94	20	11	14	40	306
Ferrous and nonferrous scrap (282, 284)	9	—	—	—	1	—	1	11
Other raw materials	84	6	43	7	2	5	11	158
Fuels (3)	*457*	*110*	*6*	*4*	*75*	*6*	*153*	*811*
Coal and coke (311)	202	6	4	2	—	5	17	236
Crude petroleum (312)	—	—	—	—	—	—	—	—
Petroleum products (313)	255	104	2	2	75	1	136	575

TABLE 3—(*Continued*)

Commodity Classes and Descriptions in Terms of the SITC	Net Imports (f.o.b.) in Trade With							
	Other West Europe	Overseas Territories	North America	Latin America	Overseas Sterling Area	U.S.S.R. and Eastern Europe	Rest	Total* Third Countries
Foodstuffs (0, 1, 22, 4)	*− 60*	*630*	*390*	*560*	*170*	—	*320*	*2,010*
Livestock and products (00, 01, 02)	70	−60	−40	−10	−20	—	—	−60
Cereals (04)	−10	50	270	120	40	20	—	490
Fruits and vegetables (05)	−140	130	—	30	10	−10	120	140
Sugar (06)	−10	−40	—	40	−20	—	−10	−40
Coffee, tea, cocoa (07)	−30	210	−10	330	100	—	60	660
Oilseeds, fats and oils (22, 4)	40	170	110	20	80	—	130	550
Tobacco (12)	40	10	70	20	—	—	20	160
Other food and beverages	−20	160	10	10	−20	−10	—	110
Raw Materials (2 excl. 22 and 266, 682–9)	*410*	*400*	*400*	*330*	*970*	*20*	*470*	*3,000*
Rubber (23)	—	10	30	—	130	—	50	220
Wood (24)	160	30	20	—	10	40	100	360
Woodpulps (25)	150	—	20	—	—	—	30	200
Natural textile fibers (26 excl. 266)	10	50	220	210	610	−20	160	1,240
Iron ore (281)	90	20	10	10	—	—	20	150
Nonferrous ores (283)	10	50	—	10	40	—	60	170
Nonferrous metals (682–9)	−10	160	—	50	50	−10	−30	210
Ferrous and nonferrous scrap (282, 284)	10	10	80	—	10	—	—	110
Other raw materials	−10	70	20	50	120	10	80	340
Fuels (3)	*−370*	*−60*	*140*	*70*	*530*	*40*	*100*	*450*
Coal and coke (311)	−140	—	100	—	—	30	−20	−30
Crude petroleum (312)	—	10	—	60	600	—	250	920
Petroleum products (313)	−230	−70	40	10	−70	10	−130	−440

* Includes AOM (Overseas Territories).
Source: GATT [20].

is examined. Table 3 presents imports (c.i.f.), exports (f.o.b.) and net imports (f.o.b.) of foodstuffs, raw materials and fuels into the EEC for 1953-55 (annual average). It is taken from a GATT study paper [20] which attempted to measure the impact of the EEC on imports from third countries in 1973-75 on the basis of different rates of growth of GNP. Table 4 was computed for 1960-61 (annual average) using essentially the same breakdown and following the same conversion method as the GATT Secretariat to derive net imports on an f.o.b. basis.[13]

The above study attempted to measure the impact of the EEC by, first, forecasting the net imports into the Six, assuming no economic integration and a 90 per cent increase in the real GNP of the Community over a twenty-year period (1953-55–1973-75). Second, a set of

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[13] It should be noted that import figures from the AOM refer only to 1960, since the 1961 data were not available at the time Table 4 was prepared. Imports from the AOM are thus shown separately only for 1960, but are included in the "rest of the world" column for 1960–61. Also, third countries in both Tables 3 and 4 include the AOM.

TABLE 4

IMPORTS, EXPORTS AND NET IMPORTS OF PRIMARY PRODUCTS INTO THE EEC, ANNUAL AVERAGE 1960–61
(Million Dollars)

COMMODITY	IMPORTS (C.I.F.) FROM									
	Intra-trade	Other Western Europe	N.A.	L.A.	Over-seas Sterling	Eastern Europe	(AOM)*	Rest†	Total‡ Third Countries	World
Foodstuffs (0, 1, 22, 4)	*1429.7*	*982.4*	*1012.1*	*1007.6*	*466.0*	*280.5*	*(919.9)*	*1448.6*	*5197.2*	*6626.9*
Livestock and products (00, 01, 02)	445.6	361.2	59.0	70.5	20.5	120.7	(6.1)	29.8	661.7	1107.3
Cereals (04)	152.2	60.8	483.2	173.4	72.0	69.8	(17.5)	52.5	911.7	1063.9
Fruits and vegetables (05)	449.5	242.6	67.8	147.0	45.6	33.0	(174.2)	314.2	850.2	1299.7
Sugar (06)	29.8	6.3	2.7	68.6	1.2	4.0	(66.3)	38.8	121.6	151.4
Coffee, tea, cocoa (07)	39.2	12.3	2.9	337.5	159.2	1.4	(189.9)	237.4	750.7	789.9
Oilseeds, fats, and oils (22, 4)	53.4	86.9	259.2	75.6	126.5	24.1	(185.8)	372.1	944.4	997.8
Tobacco (12)	45.1	46.7	93.1	19.7	17.0	8.9	(11.5)	36.2	221.6	266.7
Other foods and beverages	214.9	165.6	43.7	120.3	24.0	18.4	(268.6)	363.3	735.3	950.2
Raw Materials (2 excl. 22 and 266, 682–9)	*1297.7*	*1520.1*	*1151.3*	*646.7*	*1281.5*	*283.4*	*(535.4)*	*1110.8*	*5993.8*	*7291.5*
Rubber (23)	21.0	7.2	92.3	1.3	185.3	0.8	(17.0)	95.0	381.9	402.9
Wood (24)	74.4	391.6	43.7	13.0	68.4	125.5	(96.9)	123.2	765.4	839.8
Wood pulp (25)	27.4	281.3	70.4	—	0.4	10.8	(—)	3.3	366.2	393.6
Natural textile fibers (26 ex. 266)	215.3	125.4	340.9	241.2	596.7	40.1	(48.0)	224.7	1569.0	1784.3
Iron ore (281)	106.7	219.7	25.1	108.4	14.3	0.2	(14.4)	93.1	460.8	567.5
Nonferrous ores (283)	15.4	76.0	36.8	34.9	61.4	22.0	(56.9)	94.9	326.0	341.4
Nonferrous metals (682–9)	361.4	195.2	297.5	179.6	160.3	23.4	(230.0)	244.6	1100.6	1462.0
Ferrous and nonferrous scrap (282, 284)	185.9	28.0	128.9	3.7	9.8	1.4	(4.5)	10.0	181.8	367.7
Other raw materials	290.2	195.7	126.9	64.6	184.9	59.2	(67.7)	222.0	1033.1	1132.3
Fuels (3)	*827.9*	*87.1*	*249.8*	*225.1*	*568.4*	*222.9*	*(165.7)*	*1456.0*	*2809.3*	*3637.2*
Coal and coke (311)	585.0	32.9	182.3	0.3	2.5	69.5	(0.2)	3.0	290.5	875.5
Crude petroleum (312)	9.6	0.3	2.3	167.5	525.3	65.8	(164.9)	1382.9	2144.1	2153.7
Petroleum products (313)	211.8	45.4	65.0	82.4	40.5	87.6	(0.7)	45.9	366.8	578.6

TABLE 4—(*Continued*)

COMMODITY	EXPORTS (F.O.B.) TO									
	Intra-trade	Other Western Europe	N.A.	L.A.	Over-seas Sterling	Eastern Europe	(AOM)*	Rest†	Total‡ Third Countries	World
Foodstuffs (0, 1, 22, 4)	*1421.4*	*741.9*	*205.8*	*101.9*	*154.5*	*50.4*	(*364.7*)	*539.0*	*1793.5*	*3214.9*
Livestock and products (00, 01, 02)	472.0	187.9	54.5	39.1	56.0	11.3	(103.4)	114.1	462.9	934.9
Cereals (04)	146.5	78.3	6.2	13.1	3.6	7.3	(68.8)	131.6	240.1	386.6
Fruits and vegetables (05)	436.7	228.2	34.6	9.4	17.8	16.2	(38.8)	51.2	357.4	794.1
Sugar (06)	29.9	21.2	7.9	1.3	8.3	0.1	(62.8)	90.9	129.7	159.6
Coffee, tea, cocoa (07)	35.4	48.0	19.3	1.4	6.2	3.0	(7.0)	64.6	142.5	177.9
Oilseeds, fats, and oils (22, 4)	48.2	33.9	13.6	6.8	6.0	6.0	(16.2)	29.1	95.4	143.6
Tobacco (12)	36.6	8.5	6.8	0.8	2.9	0.9	(4.3)	6.0	25.9	62.5
Other foods and beverages	216.1	135.9	62.9	30.0	53.7	5.6	(63.4)	51.5	339.6	555.7
Raw Materials (2 excl. 22 and 266, 682–9)	*1285.3*	*467.2*	*192.6*	*40.7*	*31.7*	*85.9*	(*27.2*)	*157.1*	*975.2*	*2260.5*
Rubber (23)	19.5	9.9	0.4	1.9	1.3	8.8	(0.1)	9.7	32.0	51.5
Wood (24)	75.1	32.7	0.1	0.8	0.2	0.7	(8.1)	10.2	44.7	119.8
Wood pulp (25)	25.6	13.3	0.3	0.2	0.6	0.1	(0.5)	1.1	15.6	41.2
Natural textile fibers (26 ex. 266)	222.0	87.9	23.4	7.8	3.5	37.3	(3.7)	5.0	164.9	386.9
Iron ore (281)	107.4	11.5	—	—	—	—	(—)	—	11.5	118.9
Nonferrous ores (283)	12.5	16.5	0.3	—	0.1	0.1	(—)	0.4	7.4	19.9
Nonferrous metals (682–9)	362.1	169.4	93.8	22.6	15.9	35.1	(7.5)	59.7	396.5	758.6
Ferrous and nonferrous scrap (282, 284)	173.2	10.5	0.7	0.1	0.1	0.6	(—)	5.0	17.0	190.2
Other raw materials	287.9	125.5	73.6	7.3	10.0	3.2	(7.3)	66.0	285.6	573.5
Fuels (3)	*891.1*	*604.6*	*8.1*	*12.1*	*49.1*	*2.3*	(*97.3*)	*298.1*	*974.3*	*1865.4*
Coal and coke (311)	589.8	124.6	2.1	2.5	0.7	1.3	(4.5)	9.0	140.2	730.0
Crude petroleum (312)	6.2	1.0	—	—	—	—	(—)	0.1	1.1	7.3
Petroleum products (313)	276.8	463.1	5.9	9.5	48.2	0.9	(89.8)	284.8	812.4	1089.2

TABLE 4—(*Continued*)

COMMODITY	NET IMPORTS (F.O.B.) IN TRADE WITH							
	Other Western Europe	N.A.	L.A.	Over-seas Sterling	Eastern Europe	(AOM)*	Rest†	Total‡ Third Countries
Foodstuffs (0, 1, 22, 4)	*195*	*670*	*765*	*255*	*195*	*(450)*	*750*	*2830*
Livestock and products (00, 01, 02)	155	—	−25	−30	100	(−100)	−80	120
Cereals (04)	−20	400	125	50	55	(−45)	−90	520
Fruits and vegetables (05)	—	25	120	25	10	(110)	230	410
Sugar (06)	−15	−5	65	−5	—	(−5)	−60	−20
Coffee, tea, cocoa (07)	−40	−15	320	135	—	(170)	155	555
Oilseeds, fats, and oils (22, 4)	50	210	60	95	15	(150)	310	740
Tobacco (12)	40	80	20	10	5	(5)	25	180
Other foods and beverages	25	−25	80	−25	10	(165)	260	325
Raw Materials (2 excl. 22 and 266, 682–9)	*895*	*845*	*525*	*1105*	*160*	*(445)*	*835*	*4365*
Rubber (23)	−5	85	—	165	−10	(15)	80	315
Wood (24)	330	40	10	60	110	(80)	100	650
Wood pulp (25)	255	50	—	—	10	(—)	—	315
Natural textile fibers (26 ex. 266)	30	275	205	525	—	(35)	195	1230
Iron ore (281)	150	25	80	5	—	(10)	75	375
Nonferrous ores (283)	40	25	25	45	15	(40)	80	230
Nonferrous metals (682–9)	20	195	150	135	−15	(210)	170	655
Ferrous and nonferrous scrap (282, 284)	15	110	—	10	—	(—)	—	135
Other raw materials	60	40	55	160	50	(55)	135	500
Fuels (3)	*−515*	*160*	*195*	*400*	*180*	*(25)*	*870*	*1290*
Coal and coke (311)	−100	115	130	—	55	(−5)	−5	195
Crude petroleum (312)	—	—	10	400	50	(120)	1020	1480
Petroleum products (313)	−415	45	55	—	75	(−90)	−145	−385

* EEC, ANALYTICAL TABLES; Import, Export—1960. Used for AOM data—1960 figures instead of Annual Average 60–61.

† "Rest of the World" includes AOM.

‡ Includes AOM.

SOURCES: Derived from data in OECD, ANALYTICAL ABSTRACTS (Series B; [6] Jan.–Dec. 1961).

TABLE 5

NET IMPORTS OF PRIMARY PRODUCTS IN EEC AND REST OF WESTERN EUROPE,* 1953–55, 1960–61 AND 1973–75, DISTINGUISHING THREE DIFFERENT LEVELS OF ECONOMIC ACTIVITY (Billion Dollars, f.o.b.)

Commodity	1953-55		EEC 1960–61		1973-75 Western Europe Outside the EEC GNP=160†	EEC 1973–75 Assuming GNP of the actual level in 1953–55†		
	Western Europe Outside EEC†	EEC†	Current Values‡	In 1953–55 Prices§		at 190%	at 220%	at 250%
Fuels	*1.0*	*0.5*	*1.3*	*1.35*	*2.2*	*0.5*(1.2)**	*1.15*	*1.85*
Ores and metals	0.5	0.5	13.5		0.9	1.25	1.5	1.8
Natural textile fibers	0.8	1.3	1.2	1.5	0.5	1.25	1.45	1.65
Natural rubber	0.15	.2	0.3		(0.2)	(0.3)	(0.4)	(0.5)
Wood and pulp	0.1	.6	.95		−0.1	1.0	1.2	1.45
Other materials	0.25	.3	0.5		(0.45)	(0.55)	(0.65)	(0.8)
Raw Materials	*1.8*	*2.9*	*4.35*	*4.7*	*1.95*	*4.35*	*5.2*	*6.2*
Coffee, tea, cocoa	0.75	0.65	0.55	0.9	1.0	1.0	1.1	1.2
Fruits and vegetables	0.55	0.15	2.3		3.4	A 0.0 B 1.4 (0.0)** C 2.8	0.0 1.4 2.8	0.0 1.4 2.8
Other foodstuffs	1.7	1.2						
Foodstuffs	*3.0*	*2.0*	*2.85*	3.35	*4.4*	*1.0–3.8*(1.0)**	*1.1–3.9*	*1.2–4.0*
Total	5.8	5.4	8.5	9.4	8.55	5.35–8.65	7.45–10.25	9.25–12.05

* Rest of W. Europe consists of non-EEC OEEC countries.

† Source: [20].

‡ Derived from Table 4. It should be noted that the 1960–61 real GNP was 37 per cent higher than the 1953–55 GNP.

§ Computed by deflating 1960–61 current value figures by commodity price indices (U. S., *Monthly Bulletin of Statistics*, March, 1961, and September, 1962).

** Value of net imports corresponding to 1973–75 GNP=190 per cent of 1953–55 GNP, but assuming no economic integration and specifically no Euratom.

Note: For the significance of the three GNP levels distinguished in respect of the Common Market, see text. The three sets of net import figures shown for foodstuffs other than coffee, tea, cocoa, and the resulting ranges in the total foodstuffs and total primary products figures are purely notional. They indicate three different net import volumes—the same for each of the three GNP levels shown—as follows: A—net imports *nil*; B—net imports of the same volume as in 1953–55; C—net imports of twice the volume of 1953–55. Such import volumes may be expected to obtain with the following percentage declines in the agricultural labor force:

	GNP (in % of the 1953–55 level)		
	at 190	at 220	at 250
A	10	18	23
B	15	22	26½
C	19	25½	29½

projections of net imports was computed corresponding to GNP increases of, respectively, 90, 120, and 150 per cent over the same period, assuming the materialization of integration within the EEC. (These levels of GNP imply annual growth rates of, respectively, 3, 4, and 4.7 per cent.) The main impact of integration was taken to be on the rate of growth, agricultural output (within the realm of the common agricultural policy), and the production of domestic energy under the impetus of Euratom. A comparison of the two sets of projections would show the possible effects of the EEC on net imports, for any growth rate falling between the above limits. Table 5 shows net imports into the Six (and Western Europe outside of the Six), by broad commodity groups for 1953-55 (and 1960-61), as well as the projected imports in 1973-75 on the basis of the three growth rates mentioned above.

It was thought interesting to compare the 1953-55 figures to the pattern in existence in 1960-61 (annual average) in order to evaluate the changes which have already taken place and as a possible test of the quality of the forecasts after one-third of the original twenty years time horizon had elapsed. The third and fourth columns of Table 5 contain the data on net imports for 1960-61 in current values (derived from Table 4) and at 1953-55 prices, by deflating the current values by the relevant U.N. commodity price indices. Real GNP in the EEC increased by 37 per cent between 1953-55 and 1960-61 as Table 6 indicates.

This is equivalent to an annual growth rate of about 5 per cent over the above period. If the GATT GNP targets for 1973-75 are expressed in terms of the level of GNP already attained in 1960-61 (setting 1960-61 as a base period = 100) the three levels for 1973-75 become 139 $\left(\text{i.e., } \frac{190}{137} \times 100\right)$, 161 and 182, which are equivalent to annual

TABLE 6

EEC REAL GNP AND INDUSTRIAL PRODUCTION INDICES

Year	Real GNP		Industrial Production	
1953	100	ave. 106.4	100	
1954	105		110	
1955	114		122	
1956	120		132	$\frac{\text{Real GNP 1960–61}}{\text{Real GNP 1953–55}} = 137.2$
1957	126		140	
1958	129		144	
1959	135		153	
1960	143	ave. 146	170	
1961	149		180	

SOURCE: U.N., *National Income Yearbooks*; GATT, *International Trade*, 1959.

growth rates of respectively 2.5, 3.6, and 4.6 per cent between 1960-61 and 1973-75.

It is quite revealing that between 1953 and 1961 the actual rate of growth of GNP was slightly above the annual rate necessary to attain the upper limit postulated by GATT (an increase in GNP of 150 per cent over the twenty-year period). A continuation of the above rate would lead to the attainment of the upper range specified by GATT. It would appear reasonably likely that the annual growth rate of the EEC in the next thirteen years will be of the order of at least 4.5 per cent and thus sufficient to attain the upper bound.[14]

A comparison of net imports in 1960-61 with 1953-55 and with the projected pattern for 1973-75 shows that the total net imports of primary products into the Six had risen by slightly over one-half between 1953-55 and 1960-61 (8.5 as apposed to 5.4 billion dollars) in current values and by roughly three-fourths in terms of constant 1953-55 prices (from $5.4 to $9.4 billion). The rate of increase of net imports over the above period seems to be at least in line with the upper GATT estimates. It should, however, be remembered that an important effect of integration will be to generate a higher rate of agricultural output, in line with the common agricultural policy and of energy production, under the auspices of Euratom. It appears likely in view of recent projections of production and consumption requirements in EEC countries to 1965 (see particularly [23]) that the present level of foodstuffs imports of temperate zone products will not change much over the next decade, whereas imports of tropical foodstuffs can be expected to grow in volume terms and possibly in value terms. The import gap for temperate zone commodities which did and would have continued to narrow independently of the EEC (see [24]) is likely to be entirely eliminated (or replaced by an export surplus) for a number of commodities over the next decade. It is very difficult to separate the direct or indirect impact of integration per se from other forces such as the rate of migration out of agriculture on agricultural output and demand in the Community. Essentially similar considerations apply with respect to the influence which Euratom might have on accelerating the production of atomic energy in the Six. In any case the growth prospects for net imports of primary goods appear fairly limited even on the assumption that the recent rate of economic growth of the

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[14] In a very recent study GATT [22, pp. 129–30] assumes a rate of growth of 4.5 per cent per year for Western Europe, as a whole, in making long-term (1975) projections. It might be argued that this implies an even higher growth rate from the Six, since Western Europe outside of the Six has lagged behind the Six throughout the postwar period and is not likely without the impetus of integration (at least at present) to change this pattern in the future.

TABLE 7

TOTAL IMPORTS OF PRIMARY PRODUCTS FROM NONINDUSTRIALIZED AREAS INTO EEC, 1960; PROJECTED IMPORTS 1975 AND STATIC AND DYNAMIC EFFECTS OF INTEGRATION ON IMPORTS FROM NONINDUSTRIALIZED COUNTRIES*

		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Commodity	SITC	Index I	Index II	AOM	Non-industrialized Outside AOM	Total Non-industrialized (3+4)	Common External Tariff (*t*)	$\frac{t}{1+t}$	Non-industrialized Outside AOM 1975 Index I (1×4)	Static Effects (8×7)	Non-industrialized Outside AOM 1975 Index II (2×4)	Non-industrialized Outside AOM 1975 Index 2 12-13	Total Non-industrialized 1975 (2×5) (11+13)	AOM 1975 Corrected	Dynamic Effects 10−8 or 11−8	Dynamic Effects $14\times\frac{1}{1+t}$
Foodstuffs:																
Meat and livestock	00,01	90	130	6.0	69.1	75.1	18	.153	62.2	9.5	89.8	—	—	—	27.6	23.4
Dairy products	02	90	100	—	26.8	26.8	15	.130	24.1	3.1	26.8	—	—	—	2.7	2.3
Fish	03	100	100	7.0	27.0	34.0	23	.187	27.0	5.0	27.0	—	—	—	0	0
Bread grain	04	90	80	17.5	267.6	285.1	16	.138	240.8	33.2	214.1	193.1	228.1	35.0	−26.7	−23.0
Fruits and vegetables	05	100	145	174.2	309.3	483.5	15	.130	309.3	40.2	—	347.8	696.2	384.4	39.5	33.5
Sugar	06	110	130	66.3	66.9	133.2	80	.444	73.6	32.7	—	57.2	173.2	116.0	−16.4	−9.1
Coffee	071	130	160	120.0	372.6	492.6	16	.138	484.4	66.8	—	348.2	788.2	240.0	−136.2	−117.4
Cocoa	072	145	165	65.4	132.7	198.1	9	.083	179.1	14.9	—	238.6	326.9	88.3	59.5	54.6
Tea	074	120	130	0.1	13.7	13.8	18	.153	16.4	2.5	17.8	—	—	—	1.4	1.2
Other food	09	130	170	—	—	—	—	—	—	—	—	—	—	—	—	
Beverages	11	100	100	246.3	33.7	279.8	n.c.	n.c.	33.7	n.c.	33.7	—	—	—	−0	
Tobacco	12	125	150	11.5	55.0	66.5	30	.231	82.5	19.1	—	7.8	99.8	92.0	−74.7	−57.4
Feedstuffs	08	100	120	15.2	139.1	154.3	n.c.	n.c.	139.1	n.c.	166.9	—	—	—	27.8	n.c.
Oil seeds	22	100	100	101.7	209.5	311.2	0	0	209.5	0	209.5	—	—	—	—	0
Vegetable fats, oils	42	145	155	83.3	92.2	175.5	15	.130	133.7	17.4	142.9	—	—	—	9.2	8.0
Animal fats, oils	41	80	80	—	7.7	7.7	6	.057	6.2	.4	6.2	—	—	—	−0	0
Totals				914.5	1,822.9	2,737.4			2,021.6	244.8					−86.3	−83.9
Fuels:																
Coal	32	170	170	0.2	4.4	4.6	n.c.	n.c.	7.5	—	—	—	—	—	—	
Crude petroleum	331	140	250	164.9	1,776.0	1,940.9	0	0 }	2,680.4	0 }	4,978.0	—	—	— }	2,297.6	
Petroleum products	332			0.7	138.6	139.3	0	0 }		0 }		—	—	— }		
Total fuels				165.8	1,919.0	2,084.8			2,687.9							
Minerals:																
Crude minerals (including fertilizer)	27	145	165	10.3	128.5	138.8	0	0	186.3	0	212.0				25.7	
Iron ore	281	550	900	14.4	198.7	213.1	0	0	1,092.8	0	1,788.3				695.5	
Nonferrous ores and scrap	283,284	120	125	60.9	169.6	230.5	0	0	203.5	0	212.0				8.5	

Commodity	SITC	1 Index I	2 Index II	3 AOM	4 Non-industrialized Outside AOM	5 Total Non-industrialized (3+4)	6 Common External Tariff (t)	7 $\frac{t}{1+t}$	8 Non-industrialized Outside AOM 1975 Index I (1×4)	9 Static Effects (8×7)	10 Non-industrialized Outside AOM 1975 Index II (2×4)	11 Non-industrialized Outside AOM 1975 Index 2 12–13	12 Total Non-industrialized 1975 (2×5) (11+13)	13 AOM 1975 Corrected	14 Dynamic Effects 10−8 or 11−8	
Base Metals:																
Copper	682	165	210	188.5	287.1	475.6	0	0	473.7	0	602.9				129.2	
Lead	685	170	210	0.4	22.7	23.1	10*	.091	38.6	3.5	47.7				9.1	
Zinc	686	250	300	0.1	4.5	4.6	10*	.091	11.3	1.0	15.5				2.2	
Tin	687	120	125	7.8	33.4	41.2	0	0	40.1	0	41.8				1.6	
Aluminum	684	400	500	17.3	0.1	17.4	10	.091	—	—	—					
Nickel	683	160	200	—		—	—		—	—	—					
Other metals	689	320	400	15.8	0.6	16.4	10	.091	1.9	.2	2.4				.5	
Total minerals and metals				315.5	845.2	1,160.7			2,048.2							
Agricultural Raw Materials:																
Hides and skins	21	100	120	14.6	118.2	132.8	0	0	118.2	0	141.8	258.7	345.5	86.8	23.6	
Natural rubber	231	140	180	17.0	305.2	332.2	0	0	427.8	0	549.4				121.6	
Wool	262	135	150	0.7	166.5	167.2	0	0	224.8	0	249.8				25.0	
Cotton	263	105	110	43.4	270.7	314.1	0	0	284.2	0					−25.5	
Jute	264	120	130	—	66.9	66.9	0	0	80.3	0	87.0				6.7	
Hard fibers	265	125	130	3.9	57.0	60.9	0	0	71.3	0	74.1				2.8	
Wood and cork	24	—	—	96.9	96.9	193.8	5	.048	n.c.	n.c.	n.c.				n.c.	
Other raw materials	25	135	150	10.4	60.6	71.0	0	0	81.8	0	90.9				9.1	
Total				186.9	1,142.0	1,328.9	Grand total		1,288.4 8,046.1	4.7		Total for all primary goods			3,333.2	

* *Explanations and Sources:* Columns 1 and 2 indicate the projected increase in imports into EEC by 1975 and are based mainly on the projections made by the ECE [18 Chap. 5] for Western Europe as a whole with some adjustments for foodstuffs derived from [23]. Columns 3, 4 and 5 show the total value of imports (c.i.f.) into the EEC in 1960 for respectively the AOM, the nonindustrialized countries outside the AOM and total nonindustrialized countries.

The nonindustrialized countries are defined as Africa, Latin America, and Asia (excluding China Mainland and Japan). These data were derived from [26]. Column 6 gives the estimated common external tariff applying to the commodity group considered. In some cases it was necessary for composite groups to take an estimated average. Column 7 shows the ratio of $t/1+t$ (see text). Column 8(=Column 1×Column 4) indicates total imports from nonindustrialized areas (outside the AOM) assuming the lower growth index. Column 9 provides an estimate of the "maximum-loss estimate" (see text) and is the product of columns 8 and 7. Column 10 shows projected total imports from nonindustrialized areas (outside the AOM) on the basis of the higher index and assuming no greater than proportional increase in AOM output and exports to EEC by 1975 than for other nonindustrialized countries between 1960 and 1975.

Column 11 includes the corrections which have been made for a more than proportional share of the AOM in EEC imports of specific goods in 1975. Column 12 shows the total imports from all nonindustrialized sources assuming Index 2 (Column 2×Column 5). Column 11 is obtained by subtracting the corrected AOM share of EEC imports (column 13) from column 12 to get estimates of total imports into EEC in 1975 from nonindustrialized areas outside the AOM. Column 15 is the product of column 14 and $\left(\frac{1}{1+t}\right)$ (see text).

Six will be maintained in the future, which, as has been seen above, would lead to the realization of the most optimistic of the GATT alternatives.[15]

In the nature of an exercise, an attempt was made to give some quantitative evaluation of the static and dynamic effects of integration on imports into the EEC from third countries. The methodology described previously was used for this purpose. The nonindustrialized world (defined as comprising Africa, Latin America, and Asia with the exception of Japan and China Mainland) was divided into the AOM and the nonindustrialized areas outside the AOM. The value of imports of primary products into the EEC by commodity groups was given for 1960 for total groups (see Table 7). The ad valorem common external tariff of the Community was estimated for each corresponding SITC group.[16]

Two hypotheses were selected with respect to the growth of GNP. The first hypothesis assumes that the level of GNP in 1975 will be 60 per cent higher than in 1960 and the second that GNP will double over the same period implying annual growth rates of 3.7 and 4.75 per cent, respectively.[17] The total demand for imports into the EEC was projected by commodity groups to 1975, on the basis of the two growth hypotheses. With minor allowances, the projections made by the ECE were taken over. (By shifting the base period from 1955 to 1960 higher growth rates are postulated than the ones used by the ECE study.) It should be noted that these projections referred to Western Europe, as a whole, and not the EEC specifically. It is unlikely, however, that projections made specifically for the EEC would be far off projections constructed for the whole of Western Europe (at least for most primary products). In any case a more applicable set of projections can always be substituted for the one adopted here.

The two sets of projections are included in Table 7 under Index I (annual rate of growth of GNP: 3.7 per cent) and Index II (4.75 per cent). It is, furthermore, assumed that: (1) the lower growth rate above would obtain in the absence of and the higher in the presence of integration; (2) the share of the AOM in EEC imports would remain the same (by commodity group) in the former case but not in the latter.

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[15] The GATT forecasts of net imports of fuels into the Community by 1975 appear to be underestimates, particularly for the high growth alternative.

[16] A certain element of arbitrariness was introduced in arriving at the tariff rate for a composite commodity class, i.e., a SITC (first two digits) group, for which a number of tariff entries applied. In some instances the conversion of the SITC to the Brussels tariff nomenclature used by the Community results in a number of tariff rates for even a three digit SITC commodity group. Also some tariffs are specific tariff expressed in gold unit equivalent.

[17] These two hypotheses were made initially by the ECE [18, Chap. V] about the growth of Western Europe, as a whole, but covering the period 1955–75. The corresponding annual growth rates amounting to only 2.4 and 3.5 per cent over the twenty-year period.

Very rough corrections were made for anticipated output increases (supply response) in the AOM under the impetus of integration and the consequent rise in the share of the AOM in the Community imports. (These corrections were mainly based on [14] and [23].)

The static and dynamic integration effects were computed applying the tools discussed earlier. (See the explanations at the end of Table 7 for the procedure followed in calculating these effects.) The static effects were significant only with respect to foodstuffs, since with few exceptions fuels, minerals, metals, and agricultural raw materials are exempt from duties. The total static effects amounted to $245 million for foodstuffs and were high for coffee and sugar (together about $100 million), bread grain and fruits and vegetables. In relative terms, the static effects would amount to about 12 per cent of the projected total value of imports of foodstuffs into the EEC on the basis of Index I and to about 3 per cent of projected total primary imports (sum of column 8, Table 7).

The dynamic effects appear to be relatively very large for fuels ($2.3 billion), iron ore (almost $700 million), copper and natural rubber.[18] Some caution should be used in interpreting these figures. The important qualifications that should be kept in mind are, first, that the import data for 1960 are on a c.i.f. basis and no attempt was made to adjust them to f.o.b. values. This tends, of course, to lead to substantial overestimates of the absolute (but not relative) impact of the dynamic effects. This is particularly true for fuels for which the c.i.f. to f.o.b. ratio is relatively high. The second qualification is that projected import requirements of some of the commodities specified above are subject to a large range of error. For instance, accelerated technological developments under the impetus of Euratom could lead to a substantially greater substitution of internally produced atomic energy for imported fuels than is assumed here. Likewise imports of natural rubber depend on the technology of synthetic rubber production. A third qualification is that for a few primary products the export capacity of the nonindustrialized areas (excluding the AOM) might be limited so that the shift in demand could not be satisfied from these sources.

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[18] No adjustment was made for the impact of tariff discrimination in computing the dynamic effect for fuels, minerals, metals, and agricultural raw materials since with very few exceptions these commodities are duty free. This explains the absence of column 15 (Table 7) for these products.

A final qualification is the implicit assumption that the full brunt of the EEC, in terms of the dynamic effects—positive as well as negative—falls on the non-AOM nonindustrialized countries. This assumption tends to overstate the magnitude of these effects for these countries to the extent that a part of the dynamic effects is likely to be shared by the industrialized primary exporters.

The dynamic effects of integration on foodstuffs came out to be negative. The largest negative impact was on coffee (—$117 million), tobacco (—$57 million), bread grain and fruits and vegetables. The meaning of negative dynamic effects in the present context is that the nonindustrialized countries outside the AOM suffered a reduction of their exports to the Community under the impact of integration (hypothesis II), because of the increase in output and AOM exports, as compared to what their exports would have been without European integration (hypothesis I). At the same time, most export groups from third lesser developed countries to the Six indicate absolute increases (in some cases even substantial increases) in 1975 under hypothesis II, compared to 1960. (It is obvious on the basis of the definition of the concept that negative dynamic effects are perfectly compatible with increases in the absolute value of third countries' exports to the EEC.)

In view of the fact that the negative dynamic effects fall mainly on tropical commodities, it should be recalled that these same products are also subject to relatively high direct and indirect fiscal charges in most countries of the Community, which further increases the level of discrimination against them. In a recent study by Tinbergen [25, pages 300-04] a quantitative attempt was made to estimate the rise in imports which would result from a complete elimination of tariffs and fiscal charges for a number of these products in the Six. On the basis of 1959 figures, this increase would amount to $42 million for coffee and to at least $250 million for sugar. (Losses of revenue would follow which for some countries, i.e., Germany, would be relatively large.) This same study recommends substantial reductions—varying between 50 and 80 per cent—in the common external tariff on tropical beverages, sugar, dried fruits, and cotton fabrics.

These tariff reductions would go far towards alleviating the negative dynamic effects of integration as the quantitative estimates suggest.

Important regional implications follow from the estimates of the impact of integration. The Middle East is likely to benefit greatly from accelerated growth and larger oil requirements in Europe. On the other hand, the tropical producing countries of Latin America would appear to stand to lose most. A detailed geographical analysis of the impact of the EEC would be instructive but cannot be attempted here.

IV. *Conclusions*

The conclusions which emerge from the paper are that the initial impact of integration within the Six on third countries is likely to be relatively moderate. Quantitative estimates of the static effects of integration based on the "rate of discrimination" and a variant of the "maximum loss" concept indicated that these effects would appear to be small compared to the total value of exports of nonindustrialized countries (both individually and combined). For some commodities, however, the degree of discrimination may prove to be substantial. Those products which are likely to be most affected by the static effects are the tropical beverages (particularly coffee), sugar, tobacco, and a number of temperate-zone foodstuffs.

The dynamic effects of integration appear to favor fuels, minerals (particularly iron ore), and base metals. In contrast, net negative dynamic effects appear possible for foodstuffs as a whole. Here, again, tropical beverages would appear to be most affected because of the substitution of AOM sources under the impetus of integration.

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EUROPEAN INTEGRATION: PROBLEMS AND ISSUES

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In a discussion of problems of economic integration in Europe one could conceivably contrast the experiences of the two trade blocs: the European Economic Community and the Free Trade Area of the "Outer Seven." Instead, I have chosen to focus on the Common Market since this has clearly emerged as the nucleus of integration efforts in Europe.

One recalls that the establishment of the Community in 1958 was greeted with expressions of sanguine hope by some observers—and participants—while pessimistic views were expressed by others. The disagreements centered especially on the impact of the Common Market on trade patterns and economic growth. Although the experience of a few years that have elapsed since the Treaty came into effect does not provide sufficient evidence for judging any of these issues, some tentative remarks can still be useful.

I

Should we want to compare the achievements of the European Economic Community with the performance of the American and British economies, the choice of the relevant time period presents some problems. Considering that the first internal tariff reductions took place on January 1, 1959, one might wish to compare developments since 1958. But 1958 was a recession year in the United States and the United Kingdom, and this raises some awkward questions regarding comparability. At the same time, the reaction of businessmen to the promise of larger markets was already noticeable at the time the treaty came into effect (January 1, 1958). It appears appropriate, therefore, to make comparisons over the 1957-61 period.

The gross national product of the Common Market countries increased by 20.9 per cent between 1957 and 1961, whereas the rise in the United States' and the United Kingdom's GNP was 9.8 per cent and 11.3 per cent, respectively. As a result, in 1961, the gross national product of the Community reached about one-half of United States GNP as compared to 45 per cent in 1957.[1] But this discrepancy in the growth rates is not a new phenomenon, since the increase in the gross national product of the Common Market countries was 26.8 per cent between 1953 and 1957, while the performance of both the United States and the United Kingdom was roughly the same in the two periods.

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[1] For the purposes of international comparisons, gross national product data expressed in national currencies have been converted into dollars by the use of purchasing power parity ratios (cf. M. Gilbert and Associates, *Comparative Products and Price Levels*, Paris, Organisation for European Economic Cooperation, 1958).

Similar results are obtained if we consider manufacturing only. With the exception of Italy, manufacturing production in the EEC countries rose at a slower rate between 1957 and 1961 than in the previous four years. A comparison of rates of increase of manufacturing and industrial production has led some observers to conclude that apparently the Common Market's establishment did not contribute to economic growth in the participating countries.[3]

But all these comparisons are based, implicitly or explicitly, on *ceteris paribus* assumptions. Statistical evidence on the maintenance of past growth rates or the continuing gap between the performances of the EEC countries, the United States, and the United Kingdom enables us to arrive at meaningful conclusions concerning the effects of the Common Market on economic growth only if we can assume that the autonomous factors contributing to growth behaved in the same way during the two periods. Failing this, the relevant comparison is between the actual performance of the Common Market economies and their potential performance in the absence of the EEC's establishment.

Although there is no way of estimating the hypothetical growth rate that would have obtained without the creation of the Common Market, there are indications that a slackening of economic expansion was in store for several of these countries. To begin with, the excess labor which had been frequently mentioned as a factor contributing to the rapid rate of expansion in several of the EEC countries virtually disappeared in the late fifties. The ensuing fall in unemployment was accompanied by increases in vacancies and shortages developed in skilled labor. With the tightening of the labor market, the rate of growth of employment declined by about one-half as compared to the yearly 2 per cent rise experienced between 1953 and 1957; thus, the rate of increase of GNP per man stayed approximately unchanged.

Some further factors that had contributed to a high rate of growth prior to the Common Market's establishment had little effect thereafter. Much of the aftereffects of the war—lags in the application of technological innovations, building boom, etc.—had worn off by 1957 and, accordingly, most observers expected a slowing down in the economic expansion of the Common Market countries. In this connection,

[3] Cf. Alexander Lamfalussy, "European Progress: Due to Common Market?" *Lloyds Bank Rev.*, Oct., 1961, pp. 1-16, and R. L. Major, "The Common Market: Production and Trade," *Nat. Inst. Econ. Rev.*, Aug., 1962, pp. 24-36.

it should be recalled that forecasts prepared several years ago projected the gross national product of the present EEC countries to rise at rates of 2.6 to 3.3 per cent in the period after 1955, as compared to the actual rate of increase of 4.9 per cent in the four years of the Common Market's existence.[3]

Approaching the problem in a different way, we note that although the impact of the Community on business decisions cannot be gauged by the all-too-frequent references to the Common Market in company reports, there is evidence that the EEC's establishment influenced the business climate in the participating countries. The introduction of large-scale production methods and the increased degree of specialization observable in several industries indicate that businessmen have often acted in anticipation of an enlargement of the market. Optimistic expectations as regards future expansion in the framework of the Common Market have also contributed to an expansion of investment activity: between 1957 and 1961, the volume of gross fixed investment rose by 31 per cent in the EEC countries as compared to the 21 per cent increase in their gross national product.

These considerations point to the possible beneficial effects of the Common Market on economic activity in the participating countries. This does not mean, however, that economic expansion in the Community would necessarily continue at the pace observed in 1957-61. To begin with, the increasing tightness of the labor market has contributed to wage claims exceeding the rate of growth of productivity, and the ensuing rise in labor costs has led to price rises and/or shrinking profit margins in various industries. A study prepared by the EEC Commission notes that the shortage of labor and the imbalance between skills is expected to continue in all participating countries and may create bottlenecks to future expansion.[4] At the same time, higher prices would limit the growth of exports while a lowering of profit margins is likely to be associated with a reduction in the share of investment in GNP.

II

A further question relates to trade creation and trade diversion resulting from the Common Market's establishment. Up to the end of 1961, internal tariffs in the EEC were reduced by 30 per cent and some alignment of tariffs on imports from third countries was also effected. But the first 10 per cent tariff cut which took place on January 1, 1959, was generally applied to nonmember countries and, except in cases where the tariff in question was already below the originally envisaged common external tariff, the realignment of national tariffs was based on a 20 per cent cut in the common tariff. Accordingly, there was virtually no discrimination against outsiders until July 1, 1960, when the second internal tariff reduction took place, and the degree of tariff discrimination was generally between 10 and 20 per cent in 1961.

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[3] An annual growth rate of 2.6 per cent is the medium estimate of Ingvar Svennilson (*Prospects of Development in Western Europe,* Stockholm, 1959), while the GATT projected the gross national product of the present EEC countries to grow at a rate of 3.3 per cent in the absence of the Common Market's establishment. The GATT also assumed that the creation of the Common Market could raise this growth rate above 4.0 per cent (*Trade Intelligence Paper No. 6,* Geneva, 1957).

[4] *Rapport sur les problèmes conjoncturels de main-d'oeuvre dans la Communaute en 1962* (Bruxelles, 1962).

As regards changes in the pattern of trade, disparate developments are observed in intra-area and extra-area trade. While the volume of total imports of the Common Market countries grew at an annual rate of 10.7 per cent between 1957 and 1961, in the same period imports from within the area rose by 14.8 per cent a year and imports from third countries by 8.0 per cent. As a result of changes in trade volumes and prices, the share of intra-area trade in the value of total imports increased from 28.3 per cent to 38.3 per cent.

These developments cannot be explained by reference to past trends alone since in the 1953-57 period the discrepancy between the growth of intra-area and extra-area trade was considerably smaller: a yearly 14.0 per cent increase in imports from EEC countries was accompanied by a 11.6 per cent rise in imports from third countries. This conclusion is strengthened if we consider that between 1960 and 1961, i.e., after the degree of discrimination increased, Common Market imports from member countries rose by 15 per cent and from nonmember countries by 6 per cent. The question arises as to what extent trade creation and trade diversion were responsible for these developments.

To measure trade creation and trade diversion, it would be necessary to ascertain how much trade would have taken place in the absence of integration. Failing this, we may attempt to compare the income elasticities of demand for imports before and after integration.[5] It appears that, whereas between 1953 and 1957 a 1 per cent rise in the gross national product of the Common Market countries was accompanied by a 1.9 per cent increase in imports, the corresponding ratio was 2.2 per cent in the years 1957-61, and the income elasticity of imports from member countries was 2.3 per cent and 3.0 per cent and from nonmember countries 1.8 per cent and 1.6 per cent in the two periods respectively.

Under *ceteris paribus* assumptions, trade creation would be accompanied by a rise in the income elasticity of (total) imports while a fall in the income elasticity of imports from nonmember countries would indicate trade diversion. However, there is no reason to assume that the income elasticity of imports would have remained unchanged in the absence of integration and it should not be forgotten that changes in relative prices other than the discriminatory effects of internal tariff reductions also contribute to substitution between domestic consumption and imports on the one hand and among the various sources of imports on the other. Further, a comparison of the income elasticities of demand for imports before and after integration will understate trade creation and overstate trade diversion since this measure does not allow for the impact of the union's establishment on growth.

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[5] The term "income elasticity of demand for imports" is used here to indicate the relationship between imports and GNP. As an *ex post* concept, it reflects both income and substitution effects.

Various influences affected the income elasticity of (total) imports during the period under consideration. At least one factor would have warranted a slackening in the imports of manufactures: the removal of quantitative restrictions that contributed to the expansion of trade in manufactures between 1953 and 1957 could have had little further influence after 1957. But actually the rise of imports of manufactured goods accelerated while there has been a slowing down in the increase of primary imports.

An investigation of eighty-five commodity groups (three-digit SITC categories in Sections 5-8 excluding nonferrous metals and commodity groups with a value of intra-area trade $5 million or less) indicates that much of the increased trade in manufactures occurred within rather than between commodity groups. Between 1958 and 1961, in only four of the commodity categories did the two largest suppliers of that commodity category account for the entire increase in intra-area trade; in all other cases across-the-board increases have been experienced. Thus, while the two largest suppliers in the individual commodity groups, taken together, had a share of 64 per cent in intra-area trade in 1958, they accounted for only 50 per cent of the increase in trade. These results point to improvements in productive efficiency through specialization within commodity categories and to improvements in the efficiency of exchange through intensified exchange of consumer goods. The possible restraining effect of the EEC's establishment on the slowing down of economic growth in the member countries further contributed to trade creation.

As regards trade diversion, the decline in the income elasticity of demand for imports originating in nonmember countries between the periods 1953-57 and 1957-61 could be taken as an indication of the trade-diverting effects of the EEC. But should we assume that the Community's establishment raised the growth rate by slightly over one-tenth of one per cent, the growth effect would have counterbalanced a preference effect of this magnitude. Thus, although a detailed analysis taking into account changes in relative prices could conceivably show trade diversion in some commodity categories, there is no evidence for a substantial degree of trade diversion up to 1961.

This conclusion should not mean that the growth effect would necessarily counterbalance the preference effect in years to come. While the 10 to 20 per cent tariff discrimination existing in the second half of 1961 per se represents an approximately 1 to 3 per cent price differential for most products, the reduction of internal tariffs to one-half of their initial level in the middle of 1962 more than doubled the extent of discrimination, and further increases will be forthcoming in future years. Moreover, the application of the new agricultural policy is expected to lead to a reduction of Common Market food imports.

III

The trade-diverting effects of Common Market agricultural policy are of a different character than trade diversion in manufactures. While the establishment of a common tariff on manufactured goods leads to discrimination against exports of third country producers in favor of member country suppliers equivalent to the rate of the common duty applied, the variable levy system of EEC in its effects amounts to a variable quota.

Whereas, prior to the establishment of the Community, the domestic market of each country was open to competition by all foreign suppliers after the domestic producers had sold their crop, under the new agricultural policy third country producers will not be able to enter the Common Market unless all member countries have disposed of their excess supplies. This arrangement undoubtedly provides an incentive for expansion in member countries with potential surpluses. In fact, French agricultural policy aims at a continuing expansion of production which, in the absence of a proportionate rise in consumption, necessitates a rise in the share of exports. The Fourth Plan foresees a 30 per cent increase in agricultural output accompanied by an approximate doubling of exports.

The Common Market agricultural policy is expected to lead to a substantial reduction in EEC imports of temperate-zone foods which amounted to $2.7 billion in 1961. But the reduction of imports is only part of the problem. As projections prepared by the Common Market and FAO indicate, the Community would show a surplus in wheat, eggs, poultry, dairy products, pig meat, and sugar, even if present national price levels were maintained.[6] Although the common price level has not yet been decided upon, available information leads one to believe that the prospective arrangements would increase rather than reduce the surplus.

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[6] Communauté Economique Européenne, *Tendances de la production et de la consommation en denrées alimentaires dans la C.E.E.* (1956 à 1965) (Bruxelles, 1960) and FAO, *Agricultural Commodities-Projections for 1970* (Rome, 1962).

To dispose of the eventual surpluses, the new agricultural policy provides for export subsidies, an increasing part of which would be financed from the proceeds of the variable levies. Should Britain enter the Community, much of the surplus would find outlet in the U.K. market and the expansion of agricultural production in the Continental member countries would presumably further be stimulated with a corresponding reduction of British temperate-zone food imports that amounted to $3.0 billion in 1961.

The ensuing substitution of high-cost Continental sources of supply for low-cost non-European sources would lead to a decrease of productive efficiency and could be expected to interfere with economic growth in the Common Market countries inasmuch as the transfer of the labor force from agricultural to nonagricultural occupations would be slowed down. Import quotas accorded to the Commonwealth countries would reduce trade diversion, but discrimination against Latin-American and U.S. producers would persist.

To ensure "orderly" markets in the face of world surpluses, the so-called "Baumgartner plan" presented by the French government to GATT proposed fixing a world standard price for bulk commodities such as grain, butter, and sugar—at levels presumably equal to or higher than present French prices—and to donate quantities not disposed of at this price to the developing countries. But such a plan may not appeal to the less developed countries since it would make foreign aid subservient to the purely domestic needs of the developed economies. It can also be objected that the burden of financing of this scheme would be borne chiefly by the food importing countries. Last, but not least, the proposal would take care of the marketing problem only, and not that of overproduction. Rather, at higher prices, increasing surpluses would be generated.

It appears, therefore, that a policy of high prices could not be followed without introducing quantitative restrictions of some form. Restrictions on acreage have long been applied in the United States, and the introduction of acreage restrictions or production quotas for several products has been proposed in some countries of the Common Market, too. The Commission has considered the possible need for limiting the production of dairy products and—if a substantial fall in the prices of pig meat, poultry, and eggs is to be avoided—the use of production quotas may have to be further extended.

These considerations indicate the shortcomings of agricultural policies pursued in many developed countries. Efforts have been made in these countries to raise the incomes of the farming population through maintaining high prices at the time when technological progress in agriculture coupled with a slowing down in the expansion of the home consumption of foodstuffs would have warranted a lowering of prices. It follows that any solution designed to combat farm surpluses and trade diversion in an enlarged Common Market will have to take account of the problem of farm incomes.

Possible alternatives are high prices coupled with production and marketing quotas or greater reliance on the free market with a gradual reduction of protective measures. It appears likely that the second alternative would also require a great measure of planning for a transitional period in order to smooth the transfer of the working force from agriculture to other sectors. But this alternative would not only benefit primary producing countries by reducing discrimination against their products but would also provide long-term benefits for the Common Market countries themselves.

Given the increasing labor shortage in industry, the attainment of a high growth rate would necessitate the continuing transfer of labor from agricultural to nonagricultural occupations. A reduction of the agricultural labor force in the EEC by one-half—reportedly advocated by Mr. Sicco Mansholt, Vice-President of the Commission—would provide a perspective for future development; yet this could not take place in the framework of a protectionist agricultural policy. At the same time, a liberalization of agricultural policies would reduce the cost of protection which can be approximated by the amount of subsidy that would be necessary to compensate EEC farmers for the difference between domestic and world-market prices. If the domestic price is calculated as the arithmetical average of prices paid to French and German producers, this subsidy would amount to 2.1 per cent of Common Market GNP under present production patterns and would increase with a rise of agricultural output.[7]

IV

As regards manufacturing goods, the elimination of internal tariffs will give rise to discrimination against third country producers. It can be argued, however, that while the New York producer is discriminated against in favor of German producers in the French market, he enjoys similar discriminatory advantages over German producers in the California market. But irrespective of how we want to define discrimination, the question arises whether the advantages of tariff reduction could not be had over an area larger than the European Common Market or the United States.

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[7] Communauté Economique Européenne, *Quelques considérations au subjet du système 'deficiency payments' en agricolture et la coût de son application dans la C.E.E.* (Bruxelles, 1960).

Under the Trade Expansion Act, duties on manufactures of which the United States and the Common Market together supply more than 80 per cent on the world market could be eliminated, while on all other manufactures tariffs could be cut by one-half. This proposal has elicited varying responses from Europe—from highly favorable to downright hostile. The objections centered around three problems: the relative gains from tariff reductions, the rules governing competition, and the co-ordination of economic policies.

The difficulties involved in measuring the gains accruing to individual countries from a removal or reduction of tariffs are well known, and I do not propose to make a guess here on the relative magnitudes of prospective gains to be made by the two parties. But one can indicate the futility of much of the discussion that has taken place on the two sides of the Atlantic by contrasting two statements frequently made in public discussions. In Europe, a proposition has gained currency in French official circles according to which the superior technology of American firms enables them to outcompete their European counterparts lest the latter be protected by tariffs. On the other hand, a minority statement to the 1961 report of the Joint Economic Committee asserted that, in the absence of tariff protection, lower wages in Europe would give a competitive advantage to European producers.

As regards the rules governing competition, fears have been expressed that protectionist pressures in the United States could lead to a unilateral reinstatement of tariffs or other restrictive measures in selected industries. More generally, even a partial elimination of tariffs is said to presuppose the establishment of common rules concerning measures of safeguard, subsidies, dumping, etc., and to provide for the means of enforcing them. Otherwise, protective measures could be taken unilaterally and the continuity of trade might be broken through successive retaliatory actions. Further, it has been argued that in the absence of the co-ordination of economic policies, an increased degree of interdependence of the national economies could frustrate economic policies followed in individual countries through the transmission of cyclical fluctuations and the impact of inflationary and deflationary policies.

These considerations should not be dismissed out of hand. The example of the textile industry indicates that political pressures can give rise to protectionist measures in the United States. Also, the danger of deflation recently emphasized by Per Jacobsson can become real if some countries pursue deflationary policies since, while in an open economy the balance-of-payments repercussions of inflationary actions undertaken by any one country necessitate the application of corrective measures by that country, no such constraint operates in the case of deflation. But an adequate solution of these problems is by no means impossible.

Although one should not underestimate the protectionist sentiment in the U.S. Congress, the wide powers given to the President in the Trade Expansion Act can provide a basis for negotiations on a "code of good conduct" which would cover not only the rules governing competition but also penalties applied against infringements upon these rules. It should not be forgotten that for the first time the President has the power to use measures designed to ease adjustments, in the form of financial assistance to relocating workers and reconverting firms, instead of restricting his arsenal of weapons to the blunt instruments of tariff protection.

It is suggested here that the possible repercussions of an increased degree of interdependence between national economies can also be adequately dealt with without having recourse to a supranational approach which would not appear feasible under present conditions. Alternative solutions include some degree of co-ordination of interest rate policies coupled with the establishment of a common reserve fund à la Triffin and the introduction of a system of flexible exchange rates. A discussion of the merits and demerits of these proposals and the institutional changes necessary for their application cannot be attempted in the confines of the present paper, however.

EUROPEAN ECONOMIC INTEGRATION AND THE UNITED STATES*

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Introduction

For the second time in the twentieth century, a new area is dominating international trade. Just as the United States eclipsed Great Britain as the world's largest trading nation in the early part of the century, so is the European Economic Community outpacing the United States today. The exports and imports of the Common Market represent 24 per cent of world trade (including intra-Community trade) while the U.S. percentage is only 16 per cent. The members of the European Economic Community maintain their separate political identities, but for questions of international commerce they must be treated as a single unit, since they make unified decisions with respect to commercial policy. If all the countries currently applying for membership in the EEC were to be accepted, then the United States would be relatively small by comparison.

The formation of the European Economic Community has not only redefined trade statistics, it has and is changing the patterns of world trade. In 1958, the last year before tariff preferences were begun, less than 30 per cent of the imports of the member countries originated in other member countries. In 1961, just three years later, 36 per cent of the imports of the member countries came from other members. Looked at from the other side, exports of the EEC countries to nonmembers increased by only 29 per cent between 1958 and 1961 while their exports to each other increased by 73 per cent.

The economic success of the Common Market has been demonstrated, not only by the remarkable increases in trade, but also by the fact that the dismantling of internal restrictions to trade has been appreciably accelerated. With the reaching of an agreement on the beginning of a common agricultural policy, the EEC has moved into the second transitional stage of the Rome Treaty. Because of the very success of the Common Market in its economic objectives, it has become even more necessary to evaluate the EEC as to its effects on nonmember countries.

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The EEC as a Political Mechanism

It has been a unanimous observation of all analysts of the EEC that the effects of European integration for nonmembers cannot be determined by studying the Rome Treaty alone. As much or more will depend on the administration of the provisions and subsequent policy decisions that put its ideas into practice. Since these subsequent policy decisions while economic in character are determined within the particular political institutions of the EEC, some evaluation of the political mechanism is required to make judgments as to eventual outcomes.

It must be recalled that the determining motivations for the formation of a Common Market among the original six countries were primarily political in character. The desire to institutionalize the Franco-German *rapprochement,* the need to give an outlet to West German nationalism through the goal of wider European unity, and the attraction of combining the individual national powers into a large "third force" that could stand on the same level and, independent of the United States and the Soviet Union, were probably much more important in bringing about the Rome Treaty than the expected economic gains of membership in an exclusive trading group. The vehicle chosen for furthering these political objectives was an economic union because of the willingness of the prospective member countries to accept this type of divestiture of national sovereignty as evidenced by the European Coal and Steel Community and corresponding reluctance to join a political union as indicated by the defeat of the European Defense Community.

Certain consequences follow directly from using economic means for political ends. When questions arise as to the treatment of nonmember countries, there is always a political motive for discriminating against nonmembers even if no great economic interest is at stake, because the economic benefits of membership cannot be weakened without undermining the political unity that it is intended to cement. The common external tariff, for instance, is more than a means of protecting industry of the member countries from outside competition; it is the tie that binds the countries together. This suggests that the EEC may be unwilling to reduce their external tariffs to zero unless progress is made toward political unity through other means.

In sharp contrast to the substantial progress that the EEC has made on the economic front, the development of political unity has been minimal. The institutional framework provided for in the Rome Treaty is just sufficient to enable the customs union to operate, but nothing more. The center of power rests in the Council of Ministers and the ministers are representatives of the member countries. Unanimous consent is required for practically all isues.[1] With such a system, implying as it does a veto power by all members, decision making is slow, very cumbersome, and extremely painful. An exact balance of national interests must be obtained before agreement can be reached on any issue. In the decision process, little if any weight is given to the interests of countries not represented at the bargaining table because of these difficulties. Problems of nonmember countries that resulted from the creation of the EEC, therefore, are unlikely to receive attention by the Council of Ministers because nothing gets decided unless it poses a crisis for the Community itself or for one of the member countries. Furthermore, once agreement on an issue has been reached, it is next to impossible to get the Council of Ministers to reconsider their decision because they are unwilling to open old wounds. This is true despite some adverse consequences of the policy for nonmember countries.

Economic Implications of the EEC for the United States

The importance of the political bias against the economic interest of nonmember countries depends in part on what effects the Community is likely to have on excluded countries. For the United States, this question can be analyzed by looking at the consequences of the EEC on the bilateral trade between the U.S. and the EEC and also at its consequences on our trade with third countries. For this purpose, United States exports to the Common Market can be divided into three groups: industrial products, agricultural products, and nonagricultural raw materials.

U.S. Exports of Industrial Products

Unlike trade in agricultural products, the Common Market has made relatively few changes in the institutional system whereby industrial goods are imported into member countries. A tariff is levied on industrial products when imported from nonmembers, but this has always been the case. The new element arises from the fact that goods coming from other member countries will not have to pay a tariff or be restricted in any way, and the tariff barrier to the outside world will be uniform regardless of which member country the import is sent.[2] The consequences of this system for nonmembers depend on how much protection the external tariff wall provides as compared to the previously existing national tariffs. The common external tariff was calculated by taking an unweighted average of the French, German, Italian, and Benelux tariffs (with some exceptions).[3] The protectiveness of the tariff cannot be determined by merely comparing the resulting increases and decreases in tariffs required to reach the calculated level. For a producer within the Community that was previously protected by a high tariff, the most serious challenge to its competitive position will come from low-cost producers within the Community against which it will have no tariff protection. Unless the high-cost producers can bring their prices (and costs) down to the level of the low-cost producers within the EEC, they cannot stay in business. The essence of economic integration depends on this type of competition taking place. The prices of the large, low-cost producers within the Community will set the competitive level for the entire market. The common external tariff will be protective only to the extent that it protects the firms that can survive the internal competitive struggle.

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[1] The Rome Treaty provides that some issues are to be settled through qualified majority voting in the Council of Ministers and the number of issues so determined are to increase with successive stages within the transitional period. So far, no issue of any consequence has been determined by majority voting.

[2] During the transitional period, existing barriers on intra-Community trade will be gradually reduced and finally eliminated.

An analysis of the protectiveness of the external tariff was made by estimating the amount of protection it affords the dominant low-cost suppliers within the EEC. The dominant suppliers were identified by looking at the trade flows among the member countries before the establishment of the Community. It was assumed that the dominant suppliers of a particular product class are to be found in the country which had the largest share of intra-Community trade in that product class. We can compare the level of the external tariff for each commodity class with the former national tariff of the country with the largest share of intra-Community trade. If the new tariff rate is higher than the old national rate, then the amount of protection it affords is greater than the rate needed to protect the industry previously. If the rate is the same as previously or lower, then we can say that the amount of protection has not been increased. We cannot definitely state that a reduced tariff will undermine a previously existing level of protection without knowing whether there was excess protection under the old national tariff rate.

Two further assumptions are required to make the comparison meaningful. It must be assumed that if the high-cost firms cannot meet the competition within the Community, then the low-cost producers can expand output without significantly changing their average costs. Further, it must be assumed that changes in the tariffs on imported raw materials will not greatly affect the competitive position of the dominant supplier. As to the first assumption, much of the literature of economic integration suggests that with the enlargement of market size, substantial economies of scale will be captured and thereby lower average costs. However, further economies of scale may be limited for producers which are already quite large and increases in cost with expansion may well offset them. With respect to the second assumption, the common external tariffs on industrial raw materials were purposely set quite low so as not to weaken the competitive position of processing industries and therefore this assumption seems quite reasonable.

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[3] This average led to tariffs somewhat on the high side because of the unweighted feature and also because the national tariffs used in the calculation were above those actually in force on January 1, 1958. The German tariff which was averaged excluded the 25 per cent tariff reduction of August, 1957. The rate chosen for Italy excluded the tariff reductions of 1951. Also a Benelux tariff on most chemicals of 12 per cent was averaged in, although no such tariff was in force. Rome Treaty, Article 19(2), Article 19(3)(d) and List E.

TABLE 1

COMPARISON OF THE NEW EXTERNAL TARIFF RATE OF THE EEC ON SELECTED PRODUCTS WITH THE OLD NATIONAL TARIFF OF THE DOMINANT SUPPLIER OF EACH PRODUCT*

TYPES OF PRODUCTS	TOTAL	TARIFF COMPARISONS†		
		Plus	Even	Minus
Chemicals	12	10	0	2
Textile products	7	3	1	3
Manufactured products classified by material, other	19	14	3	2
Machinery and transport	11	11	0	0
Other manufactured products	12	8	1	3
Total	61	46	5	10

* Figures refer to number of products in category.

† Plus: tariff increased. Even: tariff unchanged. Minus: tariff declined.

SOURCE: "Post-Geneva Weighted Average *Ad Valorem* Equivalent of Duties," prepared by International Trade Analysis Division, U.S. Department of Commerce, March, 1962, P.E.P. *Atlantic Tariffs and Trade.*

A comparison of the new EEC tariff for each of sixty-one three-digit SITC commodity classes with the former national tariff protecting the dominant suppliers gives the vivid impression that the protectiveness of the new tariff is much greater than that previously existing (Table 1). Our analysis shows that 75 per cent of all manufactured products will have their protection raised and by large amounts. The size of the tariff reduction required to bring the new tariff down to the level of the old national tariff of the dominant supplier is over 50 per cent for twenty-one out of the sixty-one classes, 26 per cent to 50 per cent for sixteen classes and less than 25 per cent for only nine classes. Furthermore, for half of the ten classes for which tariff protection has been reduced, other methods of trade restrictions are commonly employed.

The impact of these higher EEC tariffs upon United States exports may well be substantial. Most of the industrial products which the United States exports to the EEC are concentrated in twenty-two of the sixty-one SITC classes analyzed (Table 2). Substantial tariff reductions on these product classes will be required to bring the level of protection down to that previously granted to the dominant supplier. Up to now, the United States has benefited greatly from the rapid growth of the European economies and the pressures put on the German engineering industry in particular. We have been the residual supplier making the most of excess demand. However, this may not continue.

If Great Britain should join the Common Market, further difficulties will be created for American exports. As desirable as the union may be on other grounds, it will cause a twofold increase in the troubles facing U.S. exporters. In the first place, as British exports cease to feel the discrimination of the EEC, an easy target for market dislodgment by the EEC and U.S. producers will be removed. Furthermore, the British themselves then may well become a serious competitor for existing American shares of the EEC market. In all the twenty-two product groups in which U.S. exports are concentrated, with the excep-

TABLE 2

IMPORTS OF MANUFACTURED PRODUCTS BY THE EEC FROM THE UNITED STATES, 1961*

Products	Value of Imports (Million Dollars)	U. S. Share of EEC Imports	Reduction of EEC Tariff Required to Maintain Protection
Inorganic chemicals	47	19.7%	49%
Organic chemicals	142	31.8	50
Paints	9	13.6	48
Medicinal and pharmaceutical products	37	22.3	25
Miscellaneous chemicals	130	26.0	63
Paper, paperboard	31	6.9	10
Yarn and thread	21	4.9	20
Processed gems	14	7.1	0
Iron and steel	63	3.4	36
Manufactures of metals, n.e.c.	35	8.1	42
Power machines	104	27.1	68
Agricultural machinery	23	10.6	65
Office machines	79	28.2	4
Metal working machinery	99	28.0	65
Miscellaneous machinery	301	21.8	67
Electric machinery	199	16.2	48
Road motor vehicles	61	7.1	35
Aircraft	190	62.5	100 (if applied)
Apparel (non-fur)	13	3.7	0
Instruments	54	26.2	55
Musical instruments	13	11.9	26
Manufactured goods, n.e.s.	22	9.2	40
Total	1,687		

* Values will differ from U.S. exports of these items to the EEC because of the inclusion of transport costs and differences in accounting periods.

SOURCE: U.N. *Commodity Trade Statistics* (1961).

tion of paper and paperboard, the British also have a substantial position. In seven groups, the British share of the EEC market exceeds our own, and it is well above 5 per cent for most of the others. This means that the British have a substantial base for expanding their exports if their competitive position improves. The competitive pressures arising from membership in the EEC and the tariff preferences gained thereby may be enough to transform the British into a fierce competitor. United States exports would become the prime target under such circumstances and maintaining our market position in industrial products would not be easy.

U.S. Agricultural Exports to the European Economic Community[4]

The most serious challenge to United States exports arises from the adoption this year by the EEC of a common agricultural policy. While many important aspects of the policy are yet to be determined, the major impact of the policy can be inferred from the mechanism that has been created. This development is of particular concern to the United States because Western Europe is such a large purchaser of U.S. agricultural products. During recent years the EEC countries alone absorbed over 20 per cent of our agricultural exports and close to one-third of all such exports sold for hard currencies. The addition of the U.K. to the Common Market would increase the latter percentage to one-half and amounts to $1.5 billion.

The agricultural agreement of January 14, 1962, provides the initial steps for the integration of the agricultural sector of the EEC. The regulations found in the agreement differ substantially among the various products, but there are some common features. Existing restrictions of all kinds on imports are to be replaced by variable levies designed to offset the differences in market prices (after adjusting for transportation costs) in the EEC country importing and in the country of origin. The levies on imports from other EEC countries are to be gradually reduced until 1970, when a one-price system will emerge. The levies on imports from non-EEC countries are to be sufficiently high to ensure preference for imports from other EEC countries; they will not be reduced over time and might be increased if Community and world prices diverge further.

This system of variable levies will work in such a way that a chain of preference will be set up. Demands for agricultural products in any EEC country will be met first by domestic production. Should these supplies prove insufficient at existing support prices, then products from other EEC countries will be allowed into the market. Only if aggregate production within the Community is insufficient will imports be allowed from non-EEC countries, regardless of how competitively priced these goods may be.

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[4] A more extensive treatment of this subject can be found in my paper published by the Joint Economic Committee, *Factors Affecting the United States Balance of Payments*, Part 2, The Common Market: New Challenges to U.S. Exports, 1962.

Provision is also made in the agreement for export subsidies designed to offset differences in market prices which will enable a member country with an exportable surplus to export to another member country needing imports but having a lower price level. Subsidies on intra-Community trade will disappear by 1970. However, similar provisions are made for export subsidies in the event that the Community as a whole experiences an exportable surplus to enable its products to compete in world markets and these provisions will not end with the transition period.

With imports determined solely as a residual between production and consumption of agricultural products within the Common Market, evaluation of the consequences of this policy depends on these magnitudes. Since the target level of price supports has not yet been agreed upon (nor pricing criteria), production cannot be estimated with certainty. The eventual agreement on price level will result from a compromise of the political requirements within the member countries. So far the West Germans have been particularly unwilling to consider a lowering of their price supports and German prices are the highest in the Community. In all likelihood, agreement can only be reached by substantially raising the average level of prices within the Community.

The implications for EEC imports of agricultural products from nonmember countries are quite clear. With rising average prices, increases in production will exceed increases in consumption and an accelerated movement toward self-sufficiency for a wide range of products will occur. U.S. exports of wheat, coarse grains, and meat will be endangered, and these products make up a substantial portion of our exports. Some other U.S. exports will be undermined by the granting of tariff preferences to the products of the Associated Overseas Countries; namely, fats and oils and to a lesser extent tobacco and cotton.

Even with this over-all pessimistic expectation, the United States will probably fare better than most third country exporters of agricultural products because of the structure of our comparative advantage. The U.S. exports mainly coarse grains to Europe rather than wheat, poultry rather than beef or lamb, and oil seeds, cotton, and tobacco rather than cocoa, coffee, and sugar. The prospects are that other countries whose product structure is concentrated in the latter categories will be in a much worse position than the U.S.

The aggregate of unfortunate consequences for nonmember countries of the common agricultural policy is greater than the sum of the losses in individual product markets. The agreement essentially re-moves the agricultural sector of the EEC from the resource allocating mechanism of world-market forces. If the EEC policy is pursued without moderation, bargaining for trade liberalization will be almost impossible because the mechanism ensures an absolute level of protection.

Exports of Nonagricultural Raw Materials

While the United States exports a considerable amount of nonagricultural crude materials to the Common Market ($750 million in 1960), the common external tariff is unlikely to interfere very greatly with this trade. Only for two commodity groups, petroleum products and aluminum, is the United States likely to suffer a trade loss. The loss may arise from the expansion of capacity that is induced by the tariff shelter. For most other products in this category, the external tariff is either zero or very low.

Trade Effects of the EEC via Third Countries

The commerce of the United States can be further affected by the Common Market even though American goods are not directly involved. If countries which normally spend a substantial portion of their foreign exchange earnings in the United States have their exports curtailed, then our balance of payments will subsequently suffer. This may well come about through tariff preferences granted the Associated Overseas Countries by the EEC.

The former French colonial areas of Africa are already substantial exporters of most tropical-zone agricultural products, and these compete with Latin-American exports to Europe. The common external tariff levied on Latin-American goods plus the marketing privileges offered African products gives a distinct competitive edge to the African suppliers. If there were a net shortage or balance of world supplies relative to demand at ruling prices, then the diversion of African supplies to Europe would leave other markets open to Latin America. Since, however, there are more than sufficient world supplies of coffee, cocoa, sugar, and tropical fruit (the principal products involved), the losses to Latin America of sales to Europe as a result of tariff discrimination cannot be made up elsewhere and are likely to lead to a substantial loss in foreign exchange for the Latin-Americans. Since the Latin-American countries buy from 48 to 64 per cent of their total imports from the United States while the African countries buy only 4 per cent of the total imports from us, the net loss to United States exports from the preferential position which the EEC affords African supplies could be substantial.

A further problem is created for the United States because another good customer of ours, Japan, is prevented from exporting to the EEC. All of the member countries discriminate against Japanese goods either by refusing to grant most-favored-nations treatment or by more devious methods. This affects us in two ways. The Japanese are denied foreign exchange, a major part of which would have been spent in the United States, and the Japanese are forced to send a disproportionate share of their output to the U.S. because we have the only market that is even partially open to them. The problem promises to be more serious as Japanese development continues. Even though discriminations against the Japanese predate the Rome Treaty, they are aggravated by the EEC because the freedom of transshipment of goods within the EEC will limit the liberalization of their restrictions to the pace of the least liberal country. Since the EEC countries must liberalize jointly, the speed of the liberalization measures will be much slower than the pace some countries were prepared to undertake in its absence.

Offset: Higher Rates of Growth of the EEC Countries

In discussions of the external impact of economic integration, the point is always made that the unfavorable impact of the formation of a customs union on nonmember country exports will be offset in whole or in part by the increase in member country growth rates stimulated by the establishment of internal free trade. If the EEC has had or is likely to have a growth-stimulating effect, then it must be recognized that the beneficial consequences of this growth on the trade positions on nonmember countries is not as great as one might expect at first glance. Since this growth is likely to be of an import-replacing character, the natural consequence of stimulating growth via exports induced by preferential tariffs, the quantity of additional imports demanded from nonmember countries is likely to be less per unit of growth in income than could have been expected from past behavior. It is not enough for the growth of income merely to lead to a slight increase in imports from nonmembers if the Community's extra exports to the rest of the world per unit of growth of income are not declining sufficiently fast to maintain balance.

In order to make the point somewhat differently, consider the EEC as if it were a single country. The character of the growth of this country is such that it is progressively requiring less imports per unit of output. If at the same time, the exports of the country are not also declining fast enough in relation to GNP, then other countries' balances of payments will be put under pressure. Merely to maintain the existing balance-of-payments positions of third countries, outsiders will have to improve their competitive position vis-à-vis the Common Market. Rapid growth within the EEC has not so far led to a deterioration in their competitive positions in third markets; in fact, the contrary may be true. As a result, rapid growth of the EEC has been combined with balance-of-payments surpluses.

Summary and Conclusions

As indicated previously, the consequences of the Common Market for nonmember countries will depend as much or more on subsequent policy decisions of the Community as on the provisions of the Rome Treaty. This analysis suggests that at present nonmember countries have been put in a disadvantageous position. Future developments will depend in large measure on the willingness of the EEC to recognize the interests of nonmembers in their deliberations or, to use the standard phraseology, whether the Common Market will be outward or inward looking. There is some evidence to indicate that up to now the Community has been looking a little in both directions.

One can get some comfort in the belief that the Common Market will be outward looking from the Rome Treaty itself. Article 110 indicates that the interests of the Community lie in expanding world trade. Furthermore, the influential leaders of the Commission of the EEC and Professor Hallstein in particular have continually supported this belief in public statements. Nor has the liberalism of the Common Market been confined to words alone. The first two internal tariff reductions were generalized to nonmember countries (as long as the resulting tariff did not fall below the external target rate). Also, the Common Market was willing to enter negotiations through GATT in the so-called "Dillon round" and eventually reached agreements with other countries through which their external tariff was reduced by almost 20 per cent.

There is some indication, however, of less liberal tendencies within the EEC. The tariff reductions made so far by the Common Market have been relatively painless in that they have merely removed some but not most of the excess protection created by the common external tariff. The common agricultural policy, however, has not only raised new barriers to trade, it has established a system that guarantees perfect protection. This policy can only be considered as extremely protectionist from the point of view of agricultural exporting countries.

Policy formation within the Common Market, of course, is not created in a vacuum. If the United States moves toward greater trade restrictions through actions such as raising tariffs on carpets and glass, then we strengthen the protectionist forces within the EEC and bring forth retaliatory restrictions to an excessive degree such as did occur. With the passage of the Trade Expansion Act, the United States has taken the first step toward leadership in the liberal direction. It is now up to the Common Market to indicate its willingness to proceed along this road.

Unfortunately, the immediate response in Europe to the American overture seems far from enthusiastic. While there is little question as to the willingness of the EEC to take part in another tariff negotiation, the hoped for major reduction in tariffs that is possible under the American law has little chance of acceptance according to some EEC spokesmen.[5] If a negotiated tariff reduction seems difficult, then a unilateral reduction by the EEC of their tariffs seems remote indeed. Yet according to this analysis, a unilateral reduction by the EEC is called for to satisfy the spirit, if not the letter, of the GATT requirement that protection not be increased through the formation of a customs union.

The economic size and power of the European Economic Community has cast it in a leadership role in the world. This power, unfortunately, can be used to further the narrow self-interest of the member countries as they conceive it to be. A continuance of existing political institutions almost assures that the power will be used in this way. On the other hand, responsible exercise of this power is possible with the development of greater political unity. Many difficult decisions would have to be made in exercising enlightened leadership, such as inducing major shifts of resources out of agriculture, and this is possible only with a strong sense of unity among the members.

Political unity may, but need not, mean a single federal state. Certainly many transitional stages would be gone through even if a single state was the ultimate goal. Fortunately, responsible leadership need not depend on reaching this particular ideal of unity. Progress toward majority rule and closer co-ordination of national policies may well be sufficient. From this point of view, the accession of Great Britain to the Rome Treaty would be the most important single step toward bringing about a more outward looking Community. British membership would add greater balance to the Community and would assure members and nonmembers alike as to the permanence of European unity and thus responsibility would follow.

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[5] Speech by M. Couve de Murville on February 20, 1962, before the American Club of Paris; speech by Valery Giscard d'Estaing on March 27, 1962, at the opening of the Lyon Trade Fair; and speech by Michel Maurice Bokanowski on October 25, 1962, to the American Chamber of Commerce in Paris.

DISCUSSION

Randall Hinshaw: It is a pleasure for me to make my main comments on the interesting paper of Professor Balassa, whom I knew as a graduate student of great promise when I was a visiting professor at Yale five years ago.

Balassa first points out that economic expansion in the Common Market during its infant years was at a somewhat slower pace them during the years immediately preceding, but he properly resists the temptation to jump to the conclusion that the Common Market has therefore not contributed to that expansion. The slackening of the growth rate, which is still about twice that of the United States, he attributes in part to a tightening of the labor market —a proposition which seems to me correct, and which suggests a moral at a later stage in my remarks.

Balassa devotes most of his paper to the important subject of trade diversion—the villain which Professor Viner so successfully identified and described in his book, *The Customs Union Issue,* of a dozen years ago. If I interpret him correctly, Balassa finds only slight evidence for trade diversion thus far, and points out that even this evidence is open to alternative interpretations. After all, even in the absence of emerging tariff discrimination, it was to be expected that Western European countries would obtain an increasing proportion of their imports from one another as long as Western Europe was becoming increasingly competitive in relation to the outside world.

In any case, as Balassa shows, trade diversion in manufactured goods could hardly have been important up to the end of 1961—the year his statistical analysis ends. True, customs duties within the Community had been reduced by 30 per cent, but with average Common Market tariffs on manufactures in the neighborhood of 14 per cent ad valorem, the resulting tariff preference for Common Market suppliers averaged less than 5 per cent and, at the retail level, the average price advantage was of course much less than this. Moreover, in some cases, as Balassa reminds us, the internal tariff reductions were extended to the rest of the world.

More important than this, the emergence of tariff discrimination within the Six and the Seven must be weighed against the virtual removal of Western European discrimination against the dollar area in the sphere of quantitative restrictions. We must not forget that the tariff discrimination introduced by the Common Market replaces an earlier regime of much greater discrimination implemented by import quotas. The removal of Western European quotas on imports from the dollar area began in earnest less than a decade ago and, contrary to one observation by Balassa, continued on an important scale after the formation of the Common Market. It should be remembered that the first internal tariff cuts at the beginning of 1959 coincided with the return of Western Europe to formal convertibility under the European Monetary Agreement—a development which, together with increased European awareness of American balance-of-payments difficulties, did much to encourage the dismantling of most of the remaining import quotas on American products. But at the beginning of 1959, such restrictions were still of major dimensions in such important countries as France, Italy, and the United Kingdom, which means that there was ample scope for a further reduction of discrimination in this sphere after the first appearance of Common Market tariff preferences. Such a reduction in quota discrimination did indeed take place—notably during 1959—and, on balance, probably outweighed the emerging tariff discrimination during 1959-61.

But this tells us nothing about the future. When tariffs within the Community have been entirely removed four or five years from now, the average tariff preference enjoyed by Common Market suppliers will be in the neighborhood of 12 or 13 per cent, compared with less than 5 per cent at the end of 1961. Under these conditions, it is clearly hazardous to rule out the possibility of significant trade diversion in manufactured goods.

In any case, there is one area in which all three speakers agree—and I heartily concur—that there is a major threat of trade diversion. I am referring, of course, to the sector of foodstuffs. In agriculture, the Community has agreed that it is to have a "common agricultural policy," in which regional protection will be substituted for national protection. For each of the main agricultural products, a regional support price will replace the existing national support prices, and this will be kept above the world price by a system of variable import levies. The degree of protection in each case will depend, of course, on the height of the regional support price, and this is a matter which has not yet been firmly decided.

It would be one thing if these regional support prices were to be set at the average levels, within recent years, of the respective national support prices. Unfortunately, however, the political pressures are far from symmetrical. For while the low-cost producers within the Community have little, if any, objection to an increase in support prices, the high-cost producers, as events have abundantly demonstrated, have the strongest objection to a reduction. In these circumstances, as Professor Krause has observed, the regional support price in each case is likely to be established at a level much closer to the highest, than to the lowest, of the existing national price supports. The result, in other words, is likely to be a substantial increase in agricultural protection within the Community.

This is a matter of major American concern, first, because the United States leads the world in exports of temperate-zone foodstuffs and, second, because Western Europe is the leading American customer for such products. Unfortunately, because Continental Western Europe is already so nearly self-sufficient in most food items, a modest increase in European production relative to European consumption can mean a precipitous decline in American food exports to Europe.

But, as Balassa has reminded us, the prospect of increased agricultural protection within the Community should be of serious concern, not only to the United States, but to the Community itself. For a transfer of resources from other sectors to high-cost agriculture can only impede economic growth, and conceivably can make the Common Market not an engine of economic expansion but a brake on that expansion. In this connection, I am reminded of Solomon Fabricant's conclusion that the major long-term factor accounting for the Soviet Union's rapid industrial growth was a persistent transfer of manpower, year after year, from agriculture to industry. And this brings me to my moral: Let it not be said a decade or two from now that the Common Market, on balance, had a negative effect on Western European economic expansion because it promoted a transfer of resources from industry to agriculture.

ROBERT E. BALDWIN: As requested by the Chairman, I shall direct my comments mainly towards Dr. Thorbecke's paper. Dr. Thorbecke has, I think, been most ingenious and bold in providing us with a quantitative estimate of the effect of European integration on the nonmember, underdeveloped countries. It is all too easy in thinking about the effects of an institutional change such as this to list all the factors tending to benefit the underdeveloped countries and all the factors tending to worsen their position and then unconsciously to assign a set of weights to these various factors that are based on the worst sort of casual empiricism. However, when one makes an effort to quantify the net benefits of such an arrangement he is forced to think more carefully about the issue and consequently is more likely to come up with a better evaluation of the matter.

There are, on the other hand, certain dangers associated with quantitative estimates, such as Dr. Thorbecke's, that are justified as being "in the nature of an exercise." As we all know, subsequent writers who refer to one's quantitative estimates of some variable seldom spell out the qualifications one needs to know in order to make a judgment about the accuracy of the estimate. Indeed, the more an estimate is quoted, the more it acquires an independent authority of its own. Even the author of the estimate begins to forget the wide margin of error that is likely to be involved in his quantitative estimate. Consequently, it is useful to all, I think, to scrutinize carefully a new quantitative measure, such as Dr. Thorbecke's, in so difficult a field in which to work as the gains from trade.

As Dr. Thorbecke points out, his measure of the welfare effects of the Common Market on the nonindustrialized nations is only valid under rather restrictive assumptions. In applying the measurement method used by Johnson the appropriate procedure is to compare the export receipts obtained by the underdeveloped countries with economic integration in existence and the export receipts they would receive from the same volume of exports but under nonintegration demand conditions. Unfortunately, the only demand data at Dr. Thorbecke's disposal are quantity estimates based on existing prices and different rates of growth. However, by assuming no change in prices within the Common Market area and by assuming demand curves to possess unitary elasticity in the relevant range, he is able to present a set of estimates based on this information. But they are not maximum or minimum figures, as Johnson's were. They are simply estimates based on particular assumptions concerning demand and supply elasticities. Certainly Dr. Thorbecke has provided us with an ingenious set of estimates on which future discussions will be based but, as he recognizes, we still need more information concerning price elasticities in order to improve upon the estimates.

Another area where further investigation is sorely needed concerns the growth effects of the Common Market. Indeed, it is apparent that differing appraisals of these effects are the main source of differences among specialists concerning the total economic impact of integration. Dr. Thorbecke is more optimistic than the authors of the other two papers with regard to these growth effects. In his quantitative estimating procedure he assumes a 3.7 per cent growth rate for the six European countries in the absence of integration and a 4.75 per cent growth rate with integration. His objective is, of course, mainly to illustrate a method of estimating the benefits of the Common Market to third countries rather than to construct an estimate of growth within the Common Market countries. Nevertheless, it is important to comment upon the subject of the growth effects of the Common Market.

In this regard almost all writers agree that the establishment of the Common Market is likely to produce some positive effect on the growth rates of the countries involved. But there is considerable disagreement concerning whether this effect will be negligible or substantial. In terms of economic analysis the manner by which economic integration may lead to such benefits as more rapid technological progress and a greater rate of investment has not received much detailed attention. Those of us who work in the field of economic development are often quite rightly criticized for our vague and untested growth theories. But some of the sweeping generalizations about growth benefits from the establishment of the Common Market make even the more expansive growth theorists look like conservatives.

Usually investigations into the so-called "static effects" of economic integration are based on a careful industry-by-industry study. The results of most of these studies seem to indicate that the static economic benefits from the Common Market are relatively small. But to get a complete estimate one must add in the dynamic effects. These are not computed on the basis of a careful industry-by-industry study but apparently mainly by picking a number that is positive but does not seem too large to the investigator. On this basis, figures like ½ of 1 per cent per year or 1 per cent per year seem reasonably small. However, since the once-and-for-all static effects are so small, whether the figure is 1 per cent or ½ of 1 per cent or $\frac{1}{100}$ of 1 per cent mainly determines whether the economic effects are significant. In short, the main part of the question about the economic effects of integration is tied up with the growth effects of the union and yet on this matter both our theory and estimating procedures are very inadequate. It is, consequently, an area where much more investigation is needed.

On the same casual basis that most estimates of the growth effects of the Common Market are made, I would like to emphasize a point Dr. Kravis recently made. We can, I think, expect continued reasonably high growth rates in the Common Market countries. But these high rates will be due mainly to more basic changes that have occurred in these countries than the creation of the Common Market organization. The Common Market is more a result of the factors making for faster growth than a cause of the growth. To expect, for example, that the establishment of economic unions among various underdeveloped countries will significantly accelerate growth is, I think, to seriously underestimate the difficulty of the growth problem.

RICHARD N. COOPER: The three papers read at this session present rather different views on the effect of European economic integration on the rest of the world. Of the three, Krause's is by far the most pessimistic. Krause foresees the mounting trade discrimination inherent in the formation of the European Economic Community (EEC) as increasingly detrimental to U.S. exports, both directly to Europe and to traditional customers of the United States in Latin America and elsewhere, whose foreign exchange earnings will be affected adversely by such discrimination. And he finds little comfort in the economic growth which will supposedly be generated by economic integration, for this growth, he thinks, will be centered largely in the industries which compete with imports.

Two of the economic assumptions in Krause's analysis leave me uneasy, for alternative assumptions seem to me equally, or even more, plausible; and the modified assumptions alter substantially the degree of protection afforded by the common external tariff of the EEC and the impact of economic growth on EEC imports from third countries. But I concur wholeheartedly with his emphasis on the degree to which the ultimate outcome depends on policy decisions still to be made by the EEC and on the importance of guiding these policy decisions in a more liberal direction.

Krause finds the new common external tariff of the EEC to offer considerably more protection to producers within the Common Market than did the previous national tariffs of the member countries. This result derives from the observation that the "dominant suppliers" in the EEC member countries were protected by tariffs generally lower than the new common external tariff and from the assumption that these major suppliers are the low-cost producers. If their production can be expanded substantially without a significant rise in costs, then the new external tariff will permit these producers to expand sales greatly, both at the expense of high-cost producers within the EEC and of producers in third countries. But that is a big if. Krause similarly expects British producers to become fierce competitors with American exporters if Britain enters the Common Market. The assumption of elastic factor supply to the low-cost producers is open to question, both on the Continent and in fully employed Britain.

One of the most striking differences which is expected between the past decade and the next is the change in total employment in the six members of the EEC. According to estimates by the Organization for European Economic Cooperation, the labor force in these six countries grew about 12 per cent from 1950 to 1960; employment grew even faster because of the sharp reduction of unemployment in Germany and Italy. In the decade 1961-71 the labor force is projected to grow only 4 per cent. Since new entrants to the labor force are its most mobile members, a sharp reduction in the rate of growth of the labor force will make it increasingly difficult for firms to increase their staffs substantially without adding to labor costs; even labor released from former high-cost industries can be substituted for younger, more mobile, and better trained new entrants only at substantial cost. As Krause recognizes, it is unlikely that large economies of scale are still in store for the dominant firms in present European markets; so rising hourly labor costs are likely to raise unit labor costs. The projected decline in labor force is sharpest for Germany, where unemployment is now negligible and the inflow of work-seeking refugees from East Germany has been reduced to a trickle; and Germany accounts for a substantial number of Krause's sixty-one dominant suppliers.

In addition to rapid growth in total European employment during the past decade, there has been a remarkable shift in the composition of the labor force from agricultural to nonagricultural employment. In Italy, for example, agricultural employment fell from 40 per cent of total employment in 1954 to 31 per cent in 1960. Only slightly less dramatic changes have occurred in the other countries. The Europeans thus face something of a policy dilemma. As Balassa points out, setting high agricultural target prices will tend to discourage departure from the farms, and this reduction in labor mobility in a period of labor shortage will make expansion of industrial output more difficult than would less protectionist agricultural policies.

Over time, of course, severe labor shortages generate increasing attention to laborsaving techniques of production. It may not be entirely amiss to think that since the U.S. pioneered many laborsaving production techniques, U.S. exports of capital goods will increasingly benefit from the very sharp change in relative factor costs which can be expected in Europe in coming years. The doubling of such exports to the EEC since 1958 may already reflect this change.

Krause's other assumption which made me uneasy was his suggestion that EEC growth will be of "an import-replacing character." I cannot see why this should be so.

Viewed in static terms, the removal of trade barriers between member countries will produce a more efficient allocation of resources within the EEC. This greater efficiency will raise output and incomes. Why presume that the resulting rise in productivity will be greater in import-replacing industries than elsewhere? Indeed, this rise in incomes results from a shift in resources from low- to high-productivity industries, not from an improvement in efficiency in particular industries.

Viewed in dynamic terms, the removal of internal trade barriers may raise productivity and incomes by jolting businessmen out of old habits—it may result in invigoration by the "cold shower of competition." This response can be expected to be greatest in the high-cost industries faced with new competition from suppliers within the Common Market. But again, I see no reason to presume that improvements in efficiency will be concentrated in industries competing primarily with imports from countries outside the EEC. Yet in a full employment environment a successful attempt by high-cost producers to lower costs will raise output and incomes, and this in turn will tend to raise EEC imports from third countries.

In sum, I think Krause understates the beneficial effects of European growth on the exports of nonmembers to the EEC.

These two observations leave me more sanguine than Krause seems to be about the economic benefits—even for the rest of the world—which will result from formation of the Common Market. Yet the paper performs a very valuable service in putting us on guard against some adverse economic effects of the EEC on nonparticipants which, at least in this country, have been either inadequately treated or glossed over. Like most great political and economic experiments, the formation of the EEC contains great potential both for good and for ill. The net effect, as Krause rightly emphasizes, depends on the new policies adopted in these formative years. So far not all EEC decisions have been such as to inspire unqualified confidence that the interests of third countries will always be taken adequately into account. Krause's observations underline the importance of continuing to press the EEC to adopt the liberal policies which, in the long run, will benefit both the Community itself and the rest of the world.

PROBLEMS OF METHODOLOGY

INTRODUCTORY REMARKS

By Fritz Machlup
Princeton University

In opening a session on "Problems of Methodology," I have an opportunity to make another plea for sanity in the use of language. Is it really necessary that the semiliterates get away with their misuse of words, and that through sheer repetition the misuse eventually becomes respectable and legitimate by virtue of the authority of a new edition of *Webster's?* I refer, of course, to the misuse of the word "methodology" when actually "method" or "technique" is meant.

Methodology, in the sense in which literate people use the word, is a branch of philosophy or of logic, though some logicians prefer to regard logic as a part of methodology. Semiliterates adopt the word when they are concerned neither with philosophy nor with logic, but simply with methods. Instead of "statistical techniques" they would say "statistical methodology," and instead of "research methods" they love to say "research methodology." They do not understand that the same method may be justified on very different methodological grounds, and that from the same methodological position one may defend very different methods of research.

It is, I suppose, quite unnecessary for me to make this remark before this audience. Perhaps, however, I may enlist your help in my campaign for the correct usage of words. Needless to say, the three papers presented here are all on problems of methodology in the proper sense of the word. Two of the speakers are "endogenous" methodologists—economists discussing the methodology of economics—whereas one is an "exogenous" methodologist, an eminent philosopher of science invited to referee one of our most fundamental methodological controversies.

THEORY CONSTRUCTION AND EMPIRICAL MEANING IN ECONOMICS*

By ANDREAS G. PAPANDREOU
University of California, Berkeley

For some time now, it has been the prevalent view of economists (1) that universal, nonanalytic statements can be formulated in economics which can be shown to be refutable, in principle, by reference to empirical data, and (2) that such statements typically express propositions concerning the signs of rates of change of "endogenous" variables with respect to "exogenous" variables.

In this paper I set about to question both aspects of this prevailing view. Specifically, I intend to argue (1) that universal, nonanalytic statements in economics must take on a taxonomic[1] character, lest they be refuted outright; (2) that universal, taxonomic statements, for the present, can only be confirmed (or found inapplicable) by reference to empirical data; (3) that it is possible and desirable to construct refutable descriptive statements, such statements being derivable from models; (4) that it is possible to formulate a general taxonomic approach which with the accumulation of knowledge may be shown to converge to a general non-taxonomic theory; (5) that the class of empirically meaningful statements in economics is much wider than is generally suspected.

In simple terms, the view taken in this paper may be cast as follows: The economist's practice is characterized by the construction of taxonomic models which, when anchored to some particular space-time segment and thus become descriptions of that segment, may be tested by reference to data drawn from that segment. If tested and confirmed, they constitute an explanation. If untested, they constitute a prediction. Furthermore, the proposed structure for a particular space-time segment is selected, as it were, from a card file of structures on the basis of observations on that segment.

Since the argument is rather involved, I intend to make some necessary simplifications. Thus no distinction will be made between qualitative and quantitative economics nor between static and dynamic structures or models.

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* I wish to thank Professors John M. Culbertson and G. C. Archibald for their helpful suggestions.

[1] The meaning attached to the word "taxonomic" in this paper will become clear as the argument progresses.

I. *On the Impossibility of Non-taxonomic Theory in Economics*

We begin, as is customary, by positing a set of *open sentences* (equations):

$$F_i(x) = k_i, \qquad i = 1, 2, \cdots, I \qquad (1.0)$$

where $x \in X$ and is an n-tuple of reals. These open sentences characterize a *structure*

$$\phi = \{F_1, F_2, \cdots, F_i, \cdots, F_I\} \qquad (1.1)$$

provided $\bigcap_i F_i \neq \phi$. In what follows we assume that this condition is always satisfied.[2]

One additional step is required now in order to arrive at the formulation of a theory based on ϕ. Let A be the class of observation acts and A_r the rth subset of A. We posit the mapping:

$$r: A_r \to X, \qquad x = r(a).^3 \qquad (1.2)$$

Our theory may now be cast in the following form:

$$\bigwedge_{a \in A_r} \left[r(a) \in \bigcap_i F_i \right]^4. \qquad (1.3)$$

Such a theory is capable, in principle, of being refuted by reference to empirical data.

It is well known, however, that the economist refuses to "take his chances" with such a theory—for it would be readily refuted by reference to a wide range of space-time experience. Indeed, the formulator of the theory would probably argue that he would be willing to "take his chances" if the theory were tested in some appropriate context. We may give this notion of "context" meaning by introducing the concept of social space.

Let W be the set of states of the world, Ω its power set, and ω a subset of W (an element of Ω). Correspondingly, let V be the class of observation acts, and V_s the sth subset of V. We posit the rule

$$(1.4) \qquad s: V_s \to \Omega, \qquad \omega = s(v) \qquad (1.4)$$

We may now write (1.3) with explicit reference to social space.

$$\bigwedge_{v \in V_s} \bigwedge_{a \in V_{r_s}} \left[s(v) = \overset{*}{\omega} \to r(a) \in \bigcap_i F_i \right]^5 \qquad (1.5)$$

where $r \neq s$ and $\overset{*}{\omega}$ is some fixed social space.[6]

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[2] The argument is restricted to structures, but could easily be extended to classes of structures or models. Incidentally, the term "model" will be given a different meaning in this paper than the customary one. See A. G. Papandreou, *Economics as a Science* (Lippincott, 1958).

[3] The x, a may be given a vectorial or a scalar interpretation—as the argument requires.

[4] "$\wedge$" stands for the universal quantifier.

[5] In this context "→" means "if . . . then."

[6] It will be noted that in (1.5) we have written A_{r_s} in lieu of A_r as in (1.3). This is intended

It is worth noting that our theory has taken on a taxonomic character. It is supposed to relate to generically described (through rule s) social space $\overset{*}{\omega}$. Given the state of our knowledge, it seems inevitable that we have to fall back on taxonomic theory.

II. *On the Impossibility of Refuting Taxonomic Universal Propositions*

Given the present state of our thinking, it is not possible to specify completely rule s, and the expression '$s(v)=\omega$' in (1.5) must remain uninterpreted.[7] Thus, in lieu of (1.5) we should write

$$\bigvee_{v\in V_s} \bigvee_{a\in A_{r_s}} \left[(s(v) = \overset{*}{\omega}) \rightarrow r(a) \in \bigcap_{un.} F_i \right] \tag{2.0}$$

In order to refute (2.0) we would have to establish in some particular substitution instance that $r(a)\in\bigcap_i F_i$ is true while $s(v)=\overset{*}{\omega}$ is false. Since we generally are not in a position to characterize adequately rule s, (2.0) may either be confirmed or may be found inapplicable—but it cannot be refuted. We choose to call (2.0) a model rather than a theory.

III. *On the Possibility of Deriving Refutable Descriptive Statements from Models*

The economist may employ expressions of form (2.0) in the formulation of refutable descriptive statements. Thus we may transform (2.0) into a descriptive statement by writing

$$\bigwedge_{a\in A_r} \left[a\ P\ H^* \rightarrow r(a) \in \bigcap_i F_i \right] \tag{3.0}$$

where $a\ PH^*$ conveys the notion that observation acts, a, take place in H^*, H^* being a given historical individual (a given segment of space-time).

(3.0) is capable, in principle, of being refuted by reference to empirical data, but it lacks universality which is characteristic of theory.

If (3.0) has been confirmed for H^*, then we say that it is an explanation of H^*, while, if (3.0) has been posited but has not been tested, we say that it is a prediction about H^*.[8]

IV. *On a General Formulation of the Taxonomic Approach*

It was suggested already that (1.5) and, therefore, (2.0) and (3.0)

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to convey the fact that rule r is in some subset of rules R which is the image of rule s under some unspecified mapping.

[7] Generic specifications of social space involve the social scientist in what must be admitted to be insurmountable difficulties. I am not prepared to argue, however, that the problem is insoluble in principle.

[8] It should be kept in mind that the term "prediction" has a rather special meaning in this context—since H^* is treated as unique instance rather than as an element of a generically defined class.

are taxonomic in character. It may be useful to make this taxonomic aspect explicit and general.

We define the mapping

$$g\colon \Omega \rightarrow \Gamma, \qquad g(\omega) = \gamma \tag{4.0}$$

where $\gamma \in \Gamma$ and γ is a unit vector of J dimensions. We also define a vector of structures

$$(\phi^1, \phi^2, \cdots, \phi^j, \cdots, \phi^J), \tag{4.1}$$

and, correspondingly, the vector

$$\left(\bigcap_{i_1} F^1_{i_1}, \ \bigcap_{i_2} \ F^2_{i_2}, \cdots, \bigcap_{i_j} F^j_{i_j}, \cdots, \bigcap_{i_j} F^J_{i_j} \right) \tag{4.2}$$

or, more simply, the equivalent vector

$$\overset{*}{F} = (\overset{*}{F}{}^1, \overset{*}{F}{}^2, \cdots, \overset{*}{F}{}^j, \cdots, \overset{*}{F}{}^J).$$

It is clear that

$$\overset{*}{F}g(\omega) = \overset{*}{F}g(s(v)) = \overset{*}{F}h(v) = \overset{*}{F}{}^j. \tag{4.4}$$

Our theory may now be cast in the following form:

$$\bigwedge_{v \in V_s} \ \bigwedge_{a \in A_{r_s}} [r_s(a) \in \overset{*}{F}h(v)] \tag{4.5}$$

where $r \neq s$.

This is an example of an all-inclusive taxonomic theory. A theory of this type is capable, in principle, of being refuted by reference to empirical data. Furthermore, it is especially well suited for interstructural comparisons. Since we do not possess such a theory (again because of the difficulty in characterizing s), we proceed along more modest lines.

We may think of ω as being equal to (ζ, ξ), where ξ is its interpreted component, and ζ its uninterpreted component. Thus, we may redefine rules s, g, and h in the following manner.

$$\overset{*}{s}\colon \ U\overset{*}{s} \rightarrow \Xi, \qquad \xi = \overset{*}{s}(u) \tag{4.6}$$

$$\overset{*}{s}{}'\colon U'\overset{*}{s}{}' \rightarrow Z, \qquad \zeta = s^{*\prime}(u') \tag{4.7}$$

$$g^{*}\colon \ \Omega \quad \rightarrow \Gamma \text{ for } \zeta = \zeta^{*}, \qquad \overset{*}{g}(\overset{*}{s}(u)) = h^{*}(u) \tag{4.8}$$

We are in a position now to construct a model corresponding to theory (4.5).

$$\bigwedge_{u'\in U'^{*'}_{s}}\ \bigwedge_{u\in U_s^{*}}\ \bigwedge_{a\in Ar_s^{*}} [(\overset{*}{s}'(u') = \overset{*}{\zeta}_{un\cdot}) \rightarrow r_s(a) \in \overset{**}{Fh}(u)] \tag{4.9}$$

(4.9) is a model because the expression '$\overset{*}{s}'(u') = \zeta^{*}$' remains uninterpreted, a fact that makes it impossible to refute it by reference to empirical data.

We take one more step and resort to description.

$$\bigwedge_{u\in U_s^{*}}\ \bigwedge_{a\in Ar_s^{*}} [u\ P\ \overset{*}{H} \rightarrow r_s^{*}(a) \in \overset{**}{Fh}(u)] \tag{4.10}$$

(4.10) corresponds to (3.0). It has, however, distinct advantages over it. First, it makes explicit reference to the connection between economic behavior, as expressed by $\overset{*}{F}$, and the social framework within which it takes place, as expressed by h^{*}—thus making it possible to deal directly with institutional changes in relation to their impact on economic behavior. Second, it provides an avenue for increasing the generality of $\overset{*}{H}$. This second point can be presented quite rigorously.

Let

$$\omega = (\omega_1, \omega_2, \cdots, \omega_m, \cdots, \omega_M) \tag{4.11}$$

where M is large but finite, and let

$$\zeta = (\omega_1, \omega_2, \cdots, \omega_{m'}, \cdots, \omega_{M'}) \tag{4.12}$$

$$\xi = (\omega_1, \omega_2, \cdots, \omega_{m''}, \cdots, \omega_{M''}) \tag{4.13}$$

where $M' + M'' = M$.

$$\frac{M''}{M} = w, \qquad 0 < w \leq 1, \tag{4.14}$$

is a measure of the generality of (4.10). It is clear, of course, that for $w = 1$ we revert to (4.5), but with s fully interpreted. In that case we may say that we possess a general theory of the social process. Its taxonomic character may then be suppressed and we arrive at (1.3). For $w = 0$ (a possibility we have excluded) the formulation (4.10) makes no sense—and we may just as well use (3.0).

V. *On Classes of Empirically Meaningful Statements in Economics*

If (1.0) is sectional its solution set $\bigcap_i F_i$ is characterized by open sentences of the form

$$y_i = y_i(z), \qquad i = 1, 2, \cdots, I \tag{5.0}$$

The partial derivatives of $y_i(z)$, if they exist, are the well-known multipliers. Since Samuelson's *Foundations* it has been assumed that assertions concerning the signs of multipliers constitute the main body of meaningful statements in qualitative economics.

It cannot be denied that they constitute meaningful statements—as long as the relevant model has been anchored to some specific space-time segment. We are prepared to argue, however, that the class of meaningful statements is much more inclusive than is explicitly recognized.

A. For "unsystematically" taxonomic models or descriptions of types (2.0) and (3.0), respectively, we recognize the following classes of meaningful statements.

1. Statements concerning the properties of $\bigcap_i F_i$, that is to say, statements about the properties of some "equilibrium" set.

2. Statements concerning the causal ordering of variables in a structure. Such statements fall into two distinct categories.

a) Statements concerning the impact of changes in the data on the endogenous variables of the structure.

b) Statements concerning the impact of changes in the instrument or policy variables on the endogenous variables of the structure.

B. For "systematically" taxonomic models or descriptions of types (4.9) or (4.10), respectively, we recognize the following classes of meaningful statements (in addition to those reported upon under A).

1. Statements involving comparisons of two or more distinct space-time segments (relating to features reported upon either under $A1$ or under $A2$).

2. Statements involving the impact of organizational (institutional) change on any of the properties of some structure in a given segment of space-time.[9]

It should be quite clear by now that the empirically relevant aspects of welfare economics, of comparative systems analysis, of economic history, and of developmental policy must fall under heading B.

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[9] In this context the formulation of the policy problem changes to take account of induced structural changes and may well be called "structural policy" in contrast to policy which is based on the premise that the structure is given.

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ASSUMPTIONS IN ECONOMIC THEORY

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Sound conclusions are sometimes supported by erroneous arguments, and the error is compounded when a sound conclusion is declared to be mistaken on the ground that the argument for it is mistaken. This general observation must serve as my *apólogia* for venturing to discuss an important and much debated methodological issue in economics, though not myself an economist. In his well-known essay, "The Methodology of Positive Economics,"[1] Professor Milton Friedman defends the use of abstract (and in particular, neoclassical) theory in economic analysis, in effect by defending the principle that the adequacy of a theory must be judged, not by assessing what he calls the "realism of its assumptions," but rather by examining the concordance of the theory's logical consequences with the phenomena the theory is designed to explain—a principle which many economists continue to reject, frequently because arguments similar to his seem to them mistaken. I also think that his argument provides no firm support for this principle; and, indeed, my paper is a critique of his defense of it. However, the relevance of my paper is not, I think, limited to Professor Friedman's essay, for I hope to show that despite the inconclusiveness of his argument his conclusion is sound.

I

Since the notions of theory and assumption are central in discussions of the principle at issue, it is convenient to begin by noting some distinctions.

1. The word theory is often used in the social sciences (including economics) rather loosely, to designate almost any general statement, however narrow its intended range of application may be. Thus, the label is commonly given to empirical generalizations (often stated in the form of equations obtained with the help of techniques of curve fitting) that are simply extrapolations from observed statistical regularities, and are asserted to hold only for behaviors occurring in a given community during some particular historical period. On the other hand, many economists (including Professor Friedman) employ the word far more selectively, and approximately in the sense associated with it when it occurs in such phrases as "the Newtonian theory of motion."

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[1] It is published in his *Essays in Positive Economics* (Chicago, 1953). All page references, unless otherwise noted, are to this book.

It is in this second sense that theory will be used in this paper. Accordingly, an economic theory (e.g., the neoclassical theory of consumer choice) is a set of statements, organized in a characteristic way, and designed to serve as partial premisses for explaining as well as predicting an indeterminately large (and usually varied) class of economic phenomena. Moreover, most if not all the statements of a theory have the form of generalized conditionals, which place no spatiotemporal restrictions on the class of phenomena that may be explained with their help. For example, the law of diminishing returns can be expressed in this form: If the quantity of a factor of production is augmented by equal increments, but the quantities of all other factors are kept constant, then the resulting increments in the product will eventually diminish. Space is lacking for discussing adequately the anatomy of theories, but a few additional features distinctive of them must be briefly mentioned.[2]

2. In a given codification of a theory, the statements belonging to it can be divided into three subgroups. The first consists of statements which count as the fundamental ones, and are often called the theory's "assumptions" (or basic "hypotheses"); the second subgroup contains the statements that are logically deducible as theorems from statements in the first. However, the term "assumption" is sometimes also used to refer to the antecedent clause of a conditional theoretical statement in either of these subgroups. This is the way Professor Friedman seems to use the word when, in discussing Galileo's law for freely falling bodies (i.e., "if a body falls toward the earth in a vacuum, its instantaneous acceleration is constant"), he asks whether this law does in fact "assume" that bodies actually fall through a vacuum.

The third subgroup of theoretical statements can also be readily characterized, if we recall that many (and perhaps all) statements in the first two subgroups contain expressions which designate nothing actually observable and are not explicitly definable in terms of expressions that do. Familiar examples of such expressions (for easy reference I will call them "theoretical terms") are "vacuum" in Galileo's law, "gene" in biological theory, and "elasticity of demand at a point" in neoclassical economic theory. Theoretical terms signify either various entities that cannot be specified except by way of some theory which postulates their existence, or certain ideal limits of theoretically endless processes. It is therefore evident that statements containing such terms cannot possibly explain or predict the course of actual events, unless a sufficient number of theoretical terms (but not necessarily all of them) are co-ordinated with observable traits of things.

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[2] A more detailed analysis is contained in my *The Structure of Science* (New York, 1961), especially Chaps. 5 and 6.

Thus, although the theoretical terms "instantaneous acceleration" and "perfectly divisible commodity" describe nothing that can be identified in experience, the expressions do in fact correspond to empirically determinable features in certain actual processes as a consequence of various rules employed (usually tacitly) by physicists and economists. In addition to the two subgroups already mentioned, a theory will in general therefore also contain a third subgroup of statements (though commonly not fully formulated) that indicate among other things such correspondences. It must be emphasized, however, that these statements do not define theoretical terms by way of terms signifying observable traits, so that theoretical terms cannot be eliminated from formulations in which they occur with the help of these statements.[8]

3. One further point deserves mention in this connection. In most disciplines, theoretical formulations (particularly those in the first two subgroups) are normally treated as statements about some subject matter, so that as in the case of other statements questions about the truth or falsity of such formulations are regarded as significant though difficult to answer. On the other hand, theoretical formulations are sometimes denied the status of "genuine" statements and are said to be simply rules which are instrumental for drawing inferences from genuine statements but which cannot be properly characterized as true or false. It is impossible in the space available to examine the merits of these opposing views on the status of theories. I have mentioned them to call attention to the fact that a defense of the methodological principle under discussion is intelligible only on the supposition that economic theory is a set of genuine statements, so that considerations of their truth or falsity are not irrelevant to the objectives of economic analysis.

II

Professor Friedman rests his argument for the methodological principle on some general reflections concerning the nature of theories *überhaupt*. He notes that a theory cannot explain a class of phenomena, unless it abstracts a small number of "common and crucial elements" (in terms of which the phenomena may be predicted) from the mass of differing circumstances in which the phenomena are embedded. Accordingly, the assumptions of a satisfactory theory are inescapably "descriptively false" or "unrealistic," so that it is pointless to assess the merits of a theory by asking whether or not its assumptions are realistic. The relevant question is whether or not the theory yields predictions which are "sufficiently good approximations for the purpose at hand."[4]

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[8] This point is of major importance. Professor Friedman also recognizes a category of statements in a theory roughly equivalent to the third subgroup of theoretical statements distinguished above; but he appears to believe that theoretical terms can be eliminated with the help of statements in this category. The point at issue cannot be adequately discussed in short compass, but an example will perhaps make clear why such a belief is dubious. Quantum theory is stated in terms of various theoretical terms, referring to such elementary particles as electrons. However, although physicists are certainly able to apply quantum theory to observable processes with the aid of statements in the third subgroup, such statements of correspondence do not permit the elimination of terms like "electron" from quantum theory.

However, an assumption may be unrealistic in at least three senses important for the argument, though Professor Friedman does not distinguish them.

1. A statement can be said to be unrealistic because it does not give an "exhaustive" description of some object, so that it mentions only some traits actually characterizing the object but ignores an endless number of other traits also present. However, no finitely long statement can possibly formulate the totality of traits embodied in any concretely existing thing; and it is difficult to imagine what a statement would be like that is not unrealistic in this sense, or what conceivable use such a statement could have. But in any event, it is with this rather trivial sense of the word in mind that Professor Friedman seems frequently to defend the legitimacy of unrealistic assumptions in economic theory;[5] and although it is not clear whether any economists have maintained a contrary thesis, his defense is fully conclusive.

2. A statement may be said to be unrealistic because it is believed to be either false or highly improbable on the available evidence. Such lack of realism can sometimes be established on the basis of what Professor Friedman calls a "directly perceived descriptive inaccuracy"; but in general, statements can be shown to be false only "indirectly," by first deducing from them some of their logical consequences (or implications), and then comparing the latter with "directly" observed matters of fact. Since it is usually not possible to establish the falsity of theoretical statements directly, Professor Friedman correctly stresses the relevance of this indirect procedure for ascertaining whether a theory is unrealistic. Nevertheless, as he recognizes and even illustrates,[6] the distinction between an assumption and its implications is a sharp one only in a given formulation of a theory—an implication of some assumption in one formulation may in another formulation be a premise implying that assumption. Accordingly, his repeated claim that an assumption can be rightly tested for its realism only indirectly obviously needs qualification.

But in any event, if by an assumption of a theory we understand one of the theory's fundamental statements (i.e., those belonging to the first of the three subgroups previously noted), a theory with an unrealistic assumption (in the present sense of the word, according to which the assumption is false) is patently unsatisfactory; for such a theory entails consequences that are incompatible with observed fact, so that on pain of rejecting elementary logical canons the theory must also be rejected. On the other hand, a universal conditional neither asserts nor presupposes that the conditions explicitly stated in its antecedent clause are actually realized; accordingly, a theoretical statement having this logical form is not proved to be false by showing that the specifications in its antecedent are not embodied in some given spatiotemporal region (or for that matter, in any region). Professor Friedman is therefore quite right in maintaining that a theory is not necessarily erroneous merely because its assumptions are unrealistic—provided that he is taken to mean by an "assumption of a theory," as he sometimes appears to mean, an antecedent clause of some theoretical statement. However, a theory whose assumptions are in this sense unrealistic for a given domain is simply inapplicable in that domain, though it may be applicable in another. But what is to be said of a theory whose assumptions are ostensibly unrealistic for every domain? The aspect of this question that is especially relevant to Professor Friedman's essay is best treated after the third sense of unrealistic has been explained.

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[4] Pp. 14-15.

[5] Pp. 18, 25, 32, 35.

[6] Pp. 26-27.

3. In many sciences, relations of dependence between phenomena are often stated with reference to so-called "pure cases" or "ideal types" of the phenomena being investigated. That is, such theoretical statements (or "laws") formulate relations specified to hold under highly "purified" conditions between highly "idealized" objects or processes, none of which is actually encountered in experience. For example, the law of the lever in physics is stated in terms of the behavior of absolutely rigid rods turning without friction about dimensionless points; similarly, a familiar law of pricing in economics is formulated in terms of the exchange of perfectly divisible and homogenous commodities under conditions of perfect competition. Statements of this kind contain what have previously been called "theoretical terms," which connote what are in effect the limits of various nonterminating series and which are not intended to designate anything actual. Such statements may be said to be unrealistic but in a sense different from the two previously noted. For they are not distinguished by their failure to provide exhaustive descriptions, nor are they literally false of anything; their distinguishing mark is the fact that when they are strictly construed, they are applicable to nothing actual.

However, laws of nature formulated with reference to pure cases are not therefore useless. On the contrary, a law so formulated states how phenomena are related when they are unaffected by numerous factors whose influence may never be completely eliminable but whose effects generally vary in magnitude with differences in the attendant circumstances under which the phenomena actually recur. Accordingly, discrepancies between what is asserted for the pure case and what actually happens can be attributed to the influence of factors not mentioned in the law. Moreover, since these factors and their effects can often be ascertained, the influence of the factors can be systematically classified into general types; and in consequence, the law can be viewed as the limiting case of a set of other laws corresponding to these various types, where each further law states a modified relation of dependence between the phenomena because of the influence of factors that are absent in the pure case. In short, unrealistic theoretical statements (in the third sense of the word) serve as a powerful means for analyzing, representing, and codifying relations of dependence between actual phenomena.

III

Professor Friedman's discussion of unrealistic assumptions in examples of theoretical statements drawn from physics and biology sheds important light on his defense of such assumptions in economic theory. It will therefore be useful to examine his account of one of these examples.

1. In his discussion of Galileo's law, Professor Friedman notes that the law is stated for bodies falling in a vacuum, but also declares that the law "works" in a large number of cases (i.e., it is in sufficiently good agreement for certain purposes with the actual behavior of bodies in these cases), though not in others. He therefore suggests that the law can be restated to read: Under a wide range of circumstances, bodies that fall in the actual atmosphere behave *as if* they were falling in a vacuum. Indeed, he seems to think that the law can be rephrased without mentioning a vacuum, as follows: Under a wide range of circumstances, the distance a body falls in a specified time is given by the formula $s = \frac{1}{2}gt^2$. Accordingly, he maintains that the circumstances in which the law works (and is therefore acceptable) must be specified as "an essential part" of the law, even though this specification (and in consequence also the law) may need revision in the light of further experience.[7]

However, as has already been indicated, the term "vacuum" is a theoretical one, so that Galileo's law in its standard version is formulated for pure cases of falling bodies. Professor Friedman's proposed paraphrase which omits all mention of a vacuum thus rests on the supposition that theoretical terms can in general be replaced by nontheoretical ones, without altering the meaning and function of the statements containing them. But the possibility of such a replacement is dubious on formal grounds alone; and what is more important, the suggestion that unless theoretical terms can thus be eliminated the statements containing them are scientifically otiose, overlooks the rationale for stating laws in terms of pure cases. In point of fact, the proposed paraphrase mistakenly assumes that Galileo's law can be assigned the functions actually performed by statements of correspondence (belonging to the third subgroup of theoretical statements) without impairing the effectiveness of the standard formulation for achieving systematic generality in theoretical physics.

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[7] Pp. 18-19.

2. The example Professor Friedman uses for the most part in his defense of unrealistic assumptions in economics is the familiar "rational maximization of returns" hypothesis in the theory of the firm. However, he states it as follows: "Under a wide range of circumstances, individual firms behave *as if* they were seeking rationally to maximize their expected returns and had full knowledge of the data needed to succeed in this attempt."[8] He freely admits that as a rule businessmen lack such knowledge and do not perform the intricate calculations required for ascertaining the indicated maximum. Indeed, he declares that "the apparent immediate determinants of business behavior" could be anything at all; e.g., ingrained habit or a chance influence. He nevertheless claims that these admitted facts do not affect the validity of the hypothesis. The relevant evidence, according to him, is the large set of facts in good agreement with various implications of the hypothesis, including the fact that firms whose actions are markedly inconsistent with it do not survive for long.

It is pertinent to ask, however, whether the operative premise from which these implications really follow is perhaps the supposition, suggested by Professor Friedman's discussion, that is rendered by: "Under a wide range of circumstances, the behavior of individual firms brings them returns approximately equal to a certain magnitude (called the maximum of expected returns by economists)"; or whether the operative premise is the hypothesis as he formulates it. On the first alternative, most of the matters mentioned in his "as if" formulation are irrelevant to the substantive content of the hypothesis. In particular, the hypothesis must then not be understood as either asserting or implying that firms conduct their affairs in order to achieve some objective. To be sure, the statement of the hypothesis contains the expression "the maximum of expected returns"; nevertheless, this expression simply designates a set of rules used by economists rather than by firms for calculating a certain magnitude. In short, the hypothesis in this case is a somewhat loosely expressed empirical generalization about the returns firms actually receive as the outcome of their overt behavior, and it specifies no determinants in explanation of that behavior.[9] Accordingly, although the hypothesis is not an exhaustive description of anything, it is not clear in what sense other than this trivial one the hypothesis is in this case unrealistic if, as Professor Friedman claims, it is in good agreement with experience. On the second alternative, however, it is difficult to avoid reading the hypothesis as saying that firms do seek to maximize their returns in a rational manner, since otherwise it appears to be asserting nothing whatsoever. But the hypothesis must then be understood as dealing with pure cases of economic behavior, requiring the use of theoretical terms in its formulation which cannot be replaced by nontheoretical expressions. Accordingly, the various facts Professor Friedman freely admits but thinks are irrelevant may in this case be quite pertinent in assessing the merits of the hypothesis.

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[8] P. 21.

Professor Friedman's essay does not indicate explicitly which alternative renders the hypothesis as he understands it. In consequence, the essay is marked by an ambiguity that perhaps reflects an unresolved tension in his views on the status of economic theory. Is he defending the legitimacy of unrealistic theoretical assumptions because he thinks theories are at best only useful instruments, valuable for predicting observable events but not to be viewed as genuine statements whose truth or falsity may be significantly investigated? But if this is the way he conceives theories (and much in his argument suggests that it is), the distinction between realistic and unrealistic theoretical assumptions is at best irrelevant, and no defense of theories lacking in realism is needed. Or is he undertaking that defense in order to show that unrealistic theories cannot only be invaluable tools for making predictions but that they may also be reasonably satisfactory explanations of various phenomena in terms of the mechanisms involved in their occurrence? But if this is his aim (and parts of his discussion are compatible with the supposition that it is), a theory cannot be viewed, as he repeatedly suggests that it can, as a "simple summary" of some vaguely delimited set of empirical generalizations with distinctly specified ranges of application.[10]

Curiously enough, something like the notion that theories can be viewed in this manner underlies one criticism of Professor Friedman's defense of the maximization-of-returns hypothesis. Thus Professor Koopmans argues that if (as Professor Friedman holds) the fact that firms whose behavior diverges from it are not likely to survive is a basis for accepting the hypothesis, "we should postulate that basis itself and not the profit maximization which it implies in certain circumstances."[11] This seems like a recommendation that since a basis for accepting Newtonian gravitational theory is the fact that observed regularities in the motions of the planets are in agreement with various special laws deduced from the theory, we should postulate those regularities rather than the theory—a recommendation that would replace the theory by the empirical evidence for the theory. Such a proposal not only rejects the conception that theories have an explanatory function; it also overlooks the irreplacable role theories have in scientific inquiry in suggesting how empirical generalizations may need to be corrected, as well as in directing and systematizing further empirical research. Unless I have seriously misunderstood Professor Friedman's essay, he would reject a proposal of this sort. Nevertheless, at various points in his argument he seems to construe theoretical statements in a manner that is almost indistinguishable from what is implied by such a proposal. I have therefore tried in this paper to show where his argument lacks cogency, as well as to indicate why the main thesis he is ostensibly defending is nonetheless sound.

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[9] In particular, the hypothesis does not include the assumption, integral to many formulations of neoclassical theory, that firms are purposive agents, whose decisions are based on rationally formed estimates of the relative advantages and risks associated with alternative courses of action open to them. See, for example, Frank H. Knight, *Risk, Uncertainty and Profit* (London, 1957), and Paul A. Samuelson, *Foundations of Economic Analysis* (Cambridge, Mass., 1947), Chap. III.

[10] P. 24.

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[11] Tjalling C. Koopmans, *Three Essays on the State of Economic Science* (New York, 1957), p. 140.

ANALYTIC ECONOMICS AND THE LOGIC OF EXTERNAL EFFECTS*

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The complexity of subject matter in a science often creates difficulties. But difficulties arise from relationships between concepts as well as from the relationships between things. A problem of this kind is external effects or externalities in microeconomic price theory. Externalities involve statements about the properties of aggregates, when these properties are not wholly deducible from their atomic elements. It is a difficulty which lies at the boundaries of microeconomic price analysis and which concerns the scope of some familiar derivations in deductive economic theory. Therefore, I will discuss some logical limits of deduction in microeconomic theory. External effects seem to elude normal derivations of microeconomic theory as well as to receive less than their share of attention. I believe these difficulties may become more understandable through analysis of the logical ambiguities of the concept.

This paper consists of two parts. First, I will introduce a few ideas, familiar to philosophers, in order to explain the logical character of the problem of externalities—a type of problem common to many deductive theories. Second, I will try to relate the economic side of the problem to these logical and philosophical considerations.

Composition Laws and Discontinuous Deductive Chains

Pure economic analysis is a deductive form of explanation. Ordinarily, deductive formulation of a science proceeds by postulating simple, primary units and simple laws of combination for these units. Gravitation for mechanical physics or maximization in economic analysis provide the basic concepts from which the laws are generated. The laws of mechanical physics follow from assumptions concerning mass-points and describe their relations to one another; maximizing units are the atomic elements of economic analysis. In deductive systems, combinations of units obey the same laws as individual units. Theorems governing the operation of aggregates are deduced from laws operating in the smaller units and their combinations. The scope of a theory describes the range of application of its theorems. Thereby, theorems which apply to the more elementary units of the system are presumed

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to apply throughout all further deductions within the system. Ideally, the scope of a theory is coextensive with the range of its subject matter.

Among the axioms that determine theoretical scope are the composition laws. Composition laws govern the extension of relationships derived from the micro-units. In its simplest form a composition rule permits a theorist to say that the laws that apply to two units are also true for 200, or 2,000,000. Thus the axioms that apply to one or two individuals, along with composition rules, permit the extension of relationships derived from these axioms to larger sectors.

The type of composition rule which may be applied to units depends on the relationships assumed to hold between those units—in particular, their interdependence. An example of a composition rule is the law of constant returns to scale. According to this law if all input factors are added in the existing ratio, there will be proportionate increases in the volume of production. Because it is assumed that no other external factor affects the ratio of the inputs, the law implies that they are independent of such external factors. Moreover, the action of an input A on input B is assumed to have no significant feedback. In microeconomic theory the independence of the units within some designated range permits the same axioms to apply to all units of the system; that is, all units are subject to the same composition law. As I said before, theorems derived from units, with composition laws, may be applied to aggregates of these units. For example, the laws that apply to the firm and the theorems that derive from these laws are theoretically applicable to the industry and the economy.

But if one of the factors becomes lumpy, the law of constant returns to scale ceases to apply because the logical requirements of the law are no longer being fulfilled. The point at which a factor becomes lumpy is where the input factors influence one another in a way not accounted for by the composition law of constant returns to scale. Thus, composition laws may cease to apply at certain levels of complexity or aggregation. When this occurs, the implications that are deduced from the individual units (say, the firm) apply only with important reservations and conditions to the larger fields (say, the industry).

It is obvious, therefore, that the laws of composition may at some point stop yielding significant results. In physics, for example, theorems based on the independence of mass and velocity are no longer useful as velocities approach the speed of light. In economics, theorems based on the independence of firms are no longer useful when firms enter into collusion. Collusion, as a kind of interdependence, cannot be explained by equilibrium analysis which assumes the independence of the units. Interdependence arises which is not deducible from the initial postulates. The laws and relationships that apply at the new level of analysis are frequently undefined. When this occurs, the theorems may have reached their outer limits, their boundaries.

A theory may be extended beyond the limits created by its initial axioms by the addition of new variables. That is, the feedback of the new relationship may be interpreted as a new variable introduced into the initial situation. Marshall's "external economies" are such an addition (an addition by means of which the situation ceased to be held constant). His definition of external economies concerned "how far the full economies of division of labor can be obtained by the concentration of large numbers of small business of a similar kind in the same locality" [8, page 277]. External economies are present only when units are aggregated. Marshall described external economies as a characteristic of the industrial aggregate. He released new variables from his pound of *ceteris paribus* to account for interdependence due to external economies, thereby preserving the main theorems of his analysis. Increasing returns resulting from economies internal to the firm imply monopoly; if progress can be accounted for by increasing returns due to external economies, competition has no necessary restriction. Although Marshall described external economies, he did not deduce them from his micro-units. In fact, his use of the conceptual device, "the representative firm," enabled statements about properties of the firm to be derived from a description of the industry. The industry had been described by theorems deduced from the firm in combination with externalities. In turn, these relationships were then incorporated in the representative firm. He defines this as a firm which has normal access to the economies, external and internal, "which has normal access to the economies, external and internal, taken of . . . the economic environment generally" [8, page 317].

Where composition rules are not entirely adequate, the addition of new variables is one solution. An alternative way of handling the theoretical problem posed by the emergence of new relationships is the modification of the composition rules. A new composition rule may be introduced when at some level, or for some range of problems, the assumption of independence is inadequate. Here, the modified theoretical system retains many of the elementary axioms and laws of the original theory, although the manner in which the units are combined is altered at the new level. Some axioms, however, may be abandoned and controversy usually arises regarding selection. A familiar illustration of this is the socialist model of Oscar Lange, in which many of the axioms of microequilibrium theory are retained, although crucial problems are submitted to collective rather than private criteria for judgment.

We have seen that there are limits to the scope of deductive theories. The introduction of new variables and the modification of composition laws are two methods of extending the scope of a deductive system. The question is one of bridging the gap at the outer limits of deductive explanation.

However, some problems cannot be handled adequately no matter how a theory is adjusted. While it may be true that some explanations are impossible within a given theory, it is also true that poor explanations occur when sound theories are pushed too far.

But how are these logical considerations relevant to external effects? Externalities are not simple observables; they are not things. They arise as a consequence of certain microeconomic concepts and they reflect conceptual difficulties at the boundaries of microeconomic theory.

Externalities and Deduction

The problem of externalities concerns the interdependence that emerges when individual units are aggregated with consequences not predictable under theorems derived from the individual units. Externalities are introduced at the point in theory where deductive explanation becomes unsatisfactory. This point I have called a boundary. The failure of the main theorems to resolve important problems requires at a minimum either the introduction of new variables or the incorporation of new composition laws into the axioms of the theory. Where the failure is complete, the theory itself must be abandoned.

The problem in logical analysis may be illustrated by the following examples from the field of economics: externalities in relation to rising supply price; externalities in relation to the distinction between marginal private and marginal social cost; and externalities in relation to direct physical interdependence.

Rising supply price is defined as the increase in price that is necessary to bring forth an increase in the scale of output. The scale of output for the industry is external to the decisions of any one firm. The shape of the supply curve, therefore, may not be directly deducible from the production functions of individual firms. Its shape depends upon the relative elasticity of the supply of factors. A rising supply function is commonly attributed to diminishing returns or rising transfer payments. What is important in this familiar illustration is that the industry supply curve can be deduced only when the external diseconomies have been added to the sum of the cost curves. At this point a break has occurred in the deductive chain. The original axioms are not sufficient for predicting the new relationships. The theory has accounted for some complexities but not for others. New variables must be released from the pound of *ceteris paribus* to account for the influence on output of a large number of firms in an industry because the joint effect on output of a large number of firms is not the simple sum of their individual effects. Although implications deduced from the independently conceived micro-units are valid laws, the inclusion of external effects on the micro-units yields results different from those obtainable by Walrasian methods alone. Shifting the focus to the industry-wide situation thus requires refinements of logical procedure. The addition of a new variable may adequately comprehend this new situation—the relationship between the firm and society—or it may not. If not, a new level of theoretical complexity may be required. The possibility of such a level is one of the implications to be drawn from Pigou when he said that industries operating under increasing or decreasing returns make greater or lesser contributions to the social product [10, pages 3-249].

The concept of externalities can act as a bridge between the private and social points of view. Because conditions external to an industry are internal to society, externalities focus on the relationship between the individual firm or industry and the whole of society. Allyn Young has used the concept of externalities to describe a crucial factor in economic progress. According to Young, industrial aggregates themselves facilitate the development of new products, new tools, and new industries: "... the progressive division and specialization of industries is an essential part of the process by which increasing returns are realized. What is required is that industrial operations be seen as an interrelated whole" [15, page 539]. The concept of externalities facilitates this vision of a unified economic field by acting as a bridge between microdeductions and inferences about the larger aggregates. It introduces a kind of interdependence which is not directly deducible from microtheory. Since microtheory develops from the private point of view, and since economic progress is a social concept, a term that will permit the transformation of one into another is extremely important. For Young, externalities provided such a concept.

The relation between marginal private product and marginal social product again brings the logical problem of composition rules to the fore. Marginal private product is the value of an incremental increase in output from the point of view of the producers of that product. Marginal social product is the value of the increase in total output from the point of view of society. This change in reference alters the structure of the problem. It may be handled by changing the composition laws. Such a change would be unnecessary if the derivations from the micro-units yielded theorems that applied equally to the aggregate. If the theorems do not apply equally, a change in the composition law may be necessary.

In microanalytic theory investment is assumed to flow to the sectors of highest private return. External effects may make the marginal private product of this investment diverge from the marginal social product. For example, marginal private gains from basic research seem to bear no necessary relation to marginal social product greater than the marginal private product. If there is no way of deducing consequences in the public sector from operations in the private sector, no single composition law can be said to apply. It may not be necessary, however, to reject altogether the assumption of independence and the composition laws dependent on it. Schumpeter's concept of progress, for example, derives from internal economies in the micro-units [12, pages 57-94]. The sum of the effects of the micro-units yield aggregate economies. The laws of the aggregate, for Schumpeter, could be deduced from the laws of the units. However, if for some ranges of theory deductions from the micro-units do not apply, new laws of composition may be required.

Where direct physical or social interdependence exists, the aggregate of firms produces effects other than those expressed by price: industrial smoke affects real estate values; ecological changes are introduced by pesticides; a hospital complex reduces absenteeism; education raises productivity beyond the increased efficiency of the recipient of education. In these examples, the activities of one sector contribute benefits or losses to third parties in the form of side effects which are not reflected in the price paid for the economic activity under consideration. Microeconomic theory fails to provide an adequate theory of the economic consequences of direct interdependence. New complexities are introduced by the combined effects of interdependence—complexities which the addition of single variables may not satisfactorily explain. Whether these consequences are trivial or not, the failure of microeconomic theory to provide a theoretical basis for evaluating them virtually disregards their existence. Consequently, they are often neglected, sometimes by inattention and sometimes by intent. The theory does not force consideration of the economic consequences of physical or social interdependence. Might not adequate treatment of these relationships require abandoning the assumption of independence logically required by certain basic postulates of the system as its stands? The theoretical problem posed by the concept of external effects, which Scitovsky has called "one of the most elusive in economic literature" [13, page 143], is most strongly presented by direct physical or social interdependence.

Conclusion

The application of microeconomic theory to instances of externality constitutes a challenge to the scope of existing theory. When theoretical development permits the adaptation of a theory to new extensions of scope, the range of the theory is increased and the validity of its premises is reinforced. The value of an explanatory system is demonstrated by the theory's capacity to grow in coherence and breadth, accounting for more phenomena more accurately. At present, the difficulty of adapting theorems drawn from axioms about independent units to permit handling phenomena of common interdependence must be recognized as a major structural problem in microtheory.

External effects derive from interdependence. As we have seen, microeconomic theory has adapted differently to diverse kinds of interdependence. In the case of rising supply price, theoretical scope can be extended by interpreting the externality as a variable and adding it to the main theorems of the system. Direct interdependence permits no such solution. Moreover, if interdependence becomes crucial for the analysis of marginal social product, the inability of microeconomic theory to deal with this aspect of economic behavior would be a severe limitation to the usefulness of the theory.

The use of the concept of marginal social product, it has been suggested, may require modification of the composition laws before certain kinds of externality can be included in the scope of microeconomic theory. Problems in deductive economic explanation arise when interdependence emerges which is not directly deducible from the initial postulates of the theory. The applications of microeconomic price analysis to larger fields is limited by these forms of interdependence. How to make direct application of unmodified microanalysis to larger fields is still an open question as the theory stands.

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DISCUSSION

G. C. Archibald: I will confine my remarks to Papandreou's paper, which I think provides a very elegant and interesting formalization of theory construction. Friedman pointed out, in his "Methodology of Positive Economics," that the specification of the conditions in which a theory is to hold is a necessary part of the theory, and Papandreou shows clearly the consequences of failure to provide that specification: either the theory is irrefutable or it is far too easily refuted because it is unbounded. Clarification of this point is extremely helpful.

It is interesting to ask if a formalization of this sort can help to resolve any of our current difficulties and controversies. Can we, for example, apply it to the dispute over "testing assumptions" (the subject of Nagel's paper)? I once suggested (*British Journal for the Philosophy of Science*, May, 1959) that the word "assumption" is used very loosely in economics, to include statements of very different sorts (about motivation, overt behavior, conditions in which the theory is to hold, etc.), and that the argument about assumptions might be more easily settled if we distinguished the different sorts of statements. After reading Papandreou's paper, I feel that we can drop the word entirely: we can refer directly to the components of theory without using vague portmanteau words. The main components are the structural relations (which may or may not be deduced from postulates about motivation) and the specification of context. On the other hand, it seems to me that the adequacy of a theory (or the performance of the structure in a given context) is, to some extent at least, a quantitative matter. What I would like to do is see if Papandreou's treatment sheds any light on the problem of quantitative adequacy. Let us start by considering a little further the question of specifying the context in which a theory is to hold.

The specification of the rule s provides the context in which the theory is supposed to hold in generic rather than time-space form; e.g., "in competitive markets in capitalist economies." When the context is so specified, we may say that the theory is "generically anchored." Now, says Papandreou, even when, as is commonly the case, s is inadequately specified,we may formulate a refutable "descriptive" statement (3.0) that the theory holds in some individual space-time, H^*. Here we may say that the theory is "locally anchored." Papandreou's (3.0), however, is really derived from (1) the general theory (2.0) in which the rule $s(v) = \omega^*$ is uninterpreted and (2) the statement of a "local anchorage," that $H^* \epsilon \omega^*$ which together give the local test statement (3.0).

What we now have, however, is the composite statement "if a PH^* and if $H^* \epsilon \omega^*$, then $\chi \epsilon \bigcap_t F_t$." Now where do we stand in the event of a refutation? Plainly it is possible either that the structure ϕ is wrong or that the local anchorage is wrong. It is the latter possibility that is the source of so many alibis for prima facie refutations: we may maintain our faith in structures by asserting, usually with hindsight, that the local anchorage was inappropriate.

Thus the proper specification of s is required to eliminate this source of alibis. On the other hand, a test with H^* is not valueless: a refutation at least limits the social space to which the theory might be anchored.

To test a theory, however, we always require a local anchorage, a given historical space-time. Thus even if a theory is generically anchored, it is necessary to identify a given H^* as an element of ω^* before a test can be performed. Now s is a rule for mapping observation acts; so if s is fully specified there should be no difficulty. But consider the usual case, discussed by Papandreou, in which s has an uninterpreted component. We may posit a local anchorage to obtain the test statement (4.10) (my remarks about the derivation of (3.0) apply equally to (4.10)), which lies partway between the purely local, (3.0), and the complete statement (1.5). In the case of (4.10) we still have potential alibis of the sort we met with (3.0). Clearly, the best we may usually expect in economics is a test statement of the (4.10) type. Yet much of our theory appears to have a generically specified anchorage; e.g., in market structure. I suggest that our difficulty often lies in identifying a given space-time as an element of the relevant general social space. The perfectly competitive market is an example. The relevant theory is generically anchored, but can we identify a local example? Do we know, in other words, that this social space belongs to the set of actual states of the world (in which case identification is possible) or only to possible states? This is really only another way of saying that we may have specified a generic context without making s operational.

This leads to my quantitative problem. Let us suppose that we have a theory anchored to a "conceptual" social space c (I do not use Papandreou's γ here because it is a component of ω, a subset of actual states of the world). We want to know if c does correspond to states of the world. This is the same question as: can we identify an appropriate H^* (or, nearly enough, can we make s operational)? Now "conceptual" and "real" states of the world are never in exact correspondence. So how do we judge? The attempt to settle the question on its own merits, so to speak, is surely a case of arguing about the realism of assumptions! It seems to me that the correspondence between c and ω, or the appropriateness of the H's selected for test, is very hard to settle except *ex post,* in the light of the results of the test. And the results of a test are, in general, quantitative: the adequacy with which a given structure performs in a given context is normally a question of "how much error." So then the correspondence of c and ω is itself a quantitative matter. This I take to be Friedman's position. And we still have no guidance in deciding "how much" is a failure. All we can say is that failure, however it is judged, does not refute the basic structure, which is, without context, irrefutable. It does, however, limit the range of its applicability. So we either learn to specify the context better, or we can maintain the structure without operational context—if we like ideal forms.

I am so intrigued by Papandreou's formalization that I should like to go on to consider many other questions in its light. We could, for example, formulate precise definitions of complementary and rival theories; or consider Friedman's proposition that different theories may be applied to the same phenomena for different purposes (the answer, I think, is: only if Papandreou's rule *g*, which provides the index to the filing cabinet of theories, has been defined, which it has not in the example over which Friedman and I are arguing). Unfortunately, time is limited, so I must content myself with one last point. Papandreou asserts, I think correctly, that statements about the properties of the equilibrium set, as well as its derivatives, are meaningful statements. But he says that this is true for models of the (2.0) type. (2.0), however, is completely unanchored since *s* is unspecified. If by meaningful we mean refutable, at least in principle, this is a mistake. Neither statements about equilibrium conditions nor about directions of change are refutable if they are unanchored. Given an anchorage, however, I agree that both types of statement are potentially meaningful. Which we try to test is a matter of expediency.

HERBERT A. SIMON: I find methodological inquiry interesting and instructive to the extent to which it addresses itself to concrete problems of empirical science. Thus, while I find myself in general agreement with almost everything that has been said in the previous papers and by discussants, I should like to pitch my remarks at a level less abstract than theirs.

The Relation of Premises and Conclusions in Economic Theory. Professor Nagel has pointed out that whether a particular proposition is a fundamental assumption of a theory or one of its derived conclusions is relative to the formulation of the theory. If this were the whole story, then asymmetry between assumptions and derivations in Friedman's position—what Professor Samuelson called the F-Twist, and what I like to think of as Friedman's "principle of unreality"—would be entirely arbitrary. Professor Krupp's remarks on composition laws and the relation of microscopic to macroscopic theories suggest, however, that something more is at issue.

Since the prefixes "micro" and "macro" have rather special meanings in economics, let me talk instead of theories of economic actors and theories of economic markets, respectively. In the present context, the relevant theory at the actor level can be approximated by the propositions: *X*—businessmen desire to maximize profits; *Y*—businessmen can and do make the calculations that identify the profit-maximizing course of action. The theory at the market level may be summed up as: *Z*—prices and quantities are observed at those levels which maximize the profits of the firms in the market. (For simplicity, let us assume that we mean the maximum of perfect competition theory.)

Defending the theory consisting of *X*, *Y*, and *Z*, Friedman asserts that it doesn't matter if *X* and *Y* are false, provided *Z* is true. Professors Nagel and Samuelson have already exposed the logical fallacy in using the validity of *Z* to support *X* and *Y*, or to support consequences of *X* and *Y* that do not follow from *Z* alone. But there are other equally serious difficulties in Friedman's position.

That *X* and *Y* are taken as premises and *Z* as a conclusion is not just a matter of taste in formulation of the theory. The formulation fits our common, if implicit, notions of explanation. We explain the macroscopic by the microscopic (plus some composition laws)—the market by the actors. We do this partly because it satisfies our feeling that individual actors are the simple components of the complex market; hence proper explanatory elements. We do it partly because *X* and *Y*, plus the composition laws, allow us to derive other propositions at the market level—say, about shifting of taxes, or other policy matters—which we are not able to test by direct observation.

The logical fallacy in Friedman's principle of unreality has exerted so much fascination—both in this session and elsewhere—that attention has been distracted from its other errors. Most critics have accepted Friedman's assumption that proposition *Z* is the empirically tested one, while *X* and *Y* are not directly observable. This, of course, is nonsense. No one has, in fact, observed whether the actual positions of business firms are the profit-maximizing ones; nor has anyone proposed a method of testing this proposition by direct observation. I cannot imagine what such a test would be, since the tester would be as incapable as business firms are of discovering what the optimal position actually is.

If, under these circumstances, *Z* is a valid theory, it must be because it follows from empirically valid assumptions about actors together with empirically valid composition laws. Now we do have a considerable body of evidence about *X* and *Y*, and the vast weight of evidence with respect to *Y*, at least, is that it is false. The expressed purpose of Friedman's principle of unreality is to save classical theory in the face of the patent invalidity of *Y*. (The Alchian survival argument that "only profit-maximizers survive," does not help matters, since it, like *Z*, cannot be tested by direct observation—we cannot identify the profit-maximizers.)

The remedy for the difficulty is straightforward, although it may involve more empirical work at the level of the individual actors than most conventionally-trained economists find comfortable. Let us make the observations necessary to discover and test true propositions, call them *X'* and *Y'*, to replace the false *X* and *Y*. Then let us construct a new market theory on these firmer foundations. This is not, of course, a novel proposal. The last two decades have seen it carried a long distance toward execution.

Ideal Types and Approximations. My final comment is related to the previous one. There has been much talk at this session of ideal types: perfect vacuums and perfect competition. I am not satisfied with the answers to Friedman's argument that he has as much right as the physicists to make unreal assumptions. Was Galileo also guilty of using the invalid principle of unreality? I think not. I think he was interested in behavior in perfect vacuums not because there aren't any in the real world, but because the real world sometimes sufficiently approximates them to make their postulation interesting.

Let me propose a methodological principle to replace the principle of unreality. I should like to call it the "principle of continuity of approximation." It asserts: if the conditions of the real world approximate sufficiently well the assumptions of an ideal type, the derivations from these assumptions will be approximately correct. Failure to incorporate this principle into his formulation seems to me a major weakness in the interesting approach of Professor

Papandreou's paper. Unreality of premises is not a virtue in scientific theory; it is a necessary evil—a concession to the finite computing capacity of the scientist that is made tolerable by the principle of continuity of approximation.

Working scientists employ the principle of continuity all the time. Unfortunately, it has no place in modern statistical theory. The word "significant" has been appropriated by the statisticians to mean "unlikely to have arisen by chance." Now, in testing extreme hypotheses—ideal types—we do not primarily want to know whether there are deviations of observation from theory which are "significant" in this sense. It is far more important to know whether they are significant in the sense that the approximation of theory to reality is beyond the limits of our tolerance. Until this latter notion of significance has been properly formalized and incorporated in statistical methodology, we are not going to accord proper methodological treatment to extreme hypotheses. The discussion at this session has not provided the solution, but it has identified this problem as one of central methodological importance for economics.

PAUL A. SAMUELSON: When Maxwell's Demon rank orders scientific disciplines by their "fruitfulness" and by their propensity to engage in methodological discussion, he finds a negative correlation and a strong inverse relationship. It is as if a science could lift itself by its own bootstraps: by maintaining a superlative silence on method, a science can become superlatively fruitful and accurate. Like many "as if" statements this is nonsense. It is more correct, albeit not very informative, to say that soft sciences spend time in talking about method because Satan finds tasks for idle hands to do. Nature does abhor a vacuum and hot air fills up more space than cold. When libertines lose the power to shock us, they take up moral pontification to bore us.

But, of course, I jest. Methodological discussion, like calisthenics and spinach, is good for us, and Dr. Nagel deserves our thanks for taking the time away from other sciences to help straighten us economists out. It is the Lord's work, and we are grateful.

As I understand his paper, Nagel comes to save Milton Friedman from himself. Nagel believes that "theory" does have an important role to play in economics and any discipline, but that Friedman's attempt in his essay on positive economics to vindicate the importance of abstract theory involves mistakes which might themselves be wrongly held against theory's establishable role.

I think Nagel's paper is valuable in pointing out certain errors in the stated claims for theory. I think, within the limits imposed by his need for brevity, it performs the constructive function of sketching some valid arguments for the useful role of theories in an empirical science. But Professor Nagel is too polite. He has not, to my mind, vindicated against itself that which was special and distinctive in the Friedman methodology; instead he seems to have jettisoned what might be called the special "Friedman Twist." And rightly so, I am afraid.

Let me first state some valid interpretations of the "as if" character of using theory to help organize our descriptions of empirical reality. Then point out some illegitimate interpretations.

When a writer on positive economics says that hypotheses or theories should be judged on their "consequences"—or their ability to describe well and organize well empirical observations—he is saying something valuable. Valuable, but perhaps not new. Pragmatists have long insisted that a theory's worth is measured by the consequences of believing it rather than something else or nothing else. Scientists and philosophers who never read Peirce, James, Dewey, Mach, Bridgman, or Carnap have enunciated this same view.

Heinrich Hertz said that a belief in Maxwell's theory of light meant nothing more and nothing less than that the observable measurements agreed with the partial differential equations of Maxwell. (With the advent of quantum mechanics and wave theory the situation became one of *reductio non ad absurdum:* physicists didn't know or much care what it was that was waving in Schrodinger's equation, a probability or what not, so long as the facts of refraction and emission could be described well by this mnemonic model.) Poincaré said that the whole content of classical dynamics was summed up in the hypothesis that certain sets of second-order differential equations exhibited solutions that to a good approximation duplicated the behavior of celestial bodies and terrestrial particles. Pascal made generous use of Occam's Razor in his "explanation" of why "nature abhors a vacuum [period or up to 30 inches of mercury and 30 feet of water]" was an inferior theory to one which assumes that there is an equilibrium balance reached between the "weight" of the unseen atmosphere and the seen mercury and water columns. When Newton wrote down his system of the world, he explicitly said what would have to be translated into modern terminology as, "I don't care to speculate why *n*-bodies behave in accordance with the inverse-square law of gravity and acceleration; I am content to show what are the implications of this law in contrast to the implications of variant hypotheses, and to present my calculations demonstrating agreement with the observations of moons, apples, and planets."

So long as light rays continue to act so as to go from place to place by the paths of least time, except as a figure of speech no one insisted that they exercised conscious, self-conscious, deliberative will. At worst, some scientists who were Deists, or Sunday poets, said that God or Nature acted like a Great Economizer.

None of the above is banal or trite. As against other authorities who insisted on seeking "more ultimate explanations," these writers said what needed to be said and Professor Friedman is a welcome recruit to their camp. But what I and other readers believe is his new twist—which from now on I shall call the "F-Twist," avoiding his name because this may be, and I hope it is, a misinterpretation of his intention—is the following: A theory is vindicable if (some of) its consequences are empirically valid to a useful degree of approximation; the (empirical) unrealism of the theory "itself," or of its "assumptions," is quite irrelevant to its validity and worth.

At points, the F-Twist seems to go even farther and claim: It is a positive merit of a theory that (some of) its content and assumptions be unrealistic since only if it is not tailored closely to one small bit of reality can it give a useful fit to a wide spread of empirical situations. Unless we explain complex reality by something simpler than itself we have accomplished little (period or by theorizing).

The last part of this F-Twist is separable from its basic part. While I believe that this last part is misphrased and that its germ of truth should be stated in other terms, brevity forbids my discussing it here and forces me to concentrate on the basic F-Twist, which is fundamentally wrong in thinking that unrealism in the sense of factual inaccuracy even to a tolerable degree of approximation is anything but a demerit for a theory or hypothesis (or set of hypotheses). Some inaccuracies are worse than others, but that is only to say that some sins against empirical science are worse than others, not that a sin is a merit or that a small sin is equivalent to a zero sin.

To a philosopher or scientist, the F-Twist is of no great moment and its discussion might perhaps be bypassed. To present-day economics—and I daresay to Professor Friedman—its validity would be of considerable moment. For, as Rotwein (*Q.J.E.*, 1959) and others have hinted, the nonpositivistic Milton Friedman has a strong effective demand which a valid F-Twist brand of positivism could supply. The motivation for the F-Twist, critics say, is to help the case for (1) the perfectly competitive laissez faire model of economics, which has been under continuous attack from outside the profession for a century and from within since the monopolistic competition revolution of thirty years past; and (2), but of lesser moment, the "maximization-of-profit" hypothesis, that mixture of truism, truth, and untruth.

If Dr. Friedman tells us this was not so; if his psychoanalyst assures us that his testimony in this case is not vitiated by subconscious motivations; even if Maxwell's Demon and a Jury in Heaven concur—still it would seem a fair use of the F-Twist itself to say: "Our theory about the origin and purpose of the F-Twist may be 'unrealistic' (a euphemism for 'empirically dead wrong'), but what of that. The consequence of our theory agrees with the fact that Chicagoans use the methodology to explain away objections to their assertions."

This, however, is cheap humor. It is hard lines to hoist a man on his own petard, while at the same time arguing that there exists no such valid petard. I must be brief in explaining why the F-Twist lacks validity. Many of these arguments can actually be found in Friedman's essay, as Nagel has noted; but that may only indicate a noble inconsistency rather than invulnerability. Besides, I am discussing the F-Twist, not any person's views, and by any other name, such as the S-Twist, it would be just as bad.

1. Define a "theory" (call it B) as a set of axioms, postulates, or hypotheses that stipulate something about observable reality. (If no conceivable observation can even in principle refute, confirm, or touch or bear upon the axiom system taken as a whole, then B is not economics, astronomy, physics, biology, or anything properly called science. It might be a model of language,

logic, mathematics, mathematical probability or geometry, or game-playing—but that is something different.)

2. A reader of Friedman might be forgiven for lapsing into thinking that the thing called B has consequences (call them C) that somehow come after it or are implied by it and (*sic*) are somehow different from it.

3. That same reader might be forgiven for thinking that just as B has consequences C that come after it, it also has some things which are somehow antecedent to it called its "assumptions" (and which we can label A).

4. The F-Twist says that the empirical realism, at least up to some "tolerable degree of approximation," of C is important. If C is empirically valid (realistic) then B is important even if A—and for that matter B itself—is not empirically valid (is unrealistic in the sense of being empirically at variance with known or knowable facts, at any tolerable level of approximation).

5. If C is the complete set of consequences of B, it is identical with B. B implies itself and all the things that itself implies. There can be no factual correctness of C so defined that is not also enjoyed by B. The minimal set of assumptions that give rise to B are identical with B, and if A is given this interpretation, its realism cannot differ from that of the theory B and consequence C.

6. But now consider a proper subset of C, which contains some but not all of the implications of B and which we may call $C-$. And consider a widened set of assumptions that includes A as a proper subset, so that it implies A (and B and C and $C-$) but is not fully implied by A. Call this A+.

In symbolic notation we can say

$$A+\supset A\equiv B\equiv C\supset C-$$

7. Now, suppose that C has complete (or satisfactory) empirical validity. Then bully for it. And bully for the theory B and for its assumption A.

8. We cannot say bully for $A+$ in the same sense—unless its full content, which we may call $A+\equiv B+\equiv C+$, also have empirical validity. If that part of $C+$ which is not in C is unrealistic in the sense of being impirically false at the required level of approximation, then $A+$ is definitely the worse for it. The invalidity of part of $A+$ is not irrelevant to its worth. If only the A subset of $A+$ is valid, then so much the worse for $(A+) - (A)$ and for $A+$.

If as often happens we do not have evidence on the factual inaccuracy or accuracy for $A+$, we simply reserve judgment about it, and keep saying bully for A. If no evidence can bear on $(A+) - (A)$, then we use Occam's Razor and concentrate on $A\equiv B\equiv C$ above, forgetting $A+$.

9. It should be unnecessary for me to explain why the empirical validity of $C-$ does not, of itself, import any luster to $A\equiv B\equiv C$—unnecessary because this is the same logical case as I have just disposed of.

This completes my demonstration that the F-Twist is fallacious. I shall illustrate briefly with some examples, primarily economic.

Let B be maximizing ordinal utility (satisfying certain regularity conditions) subject to a budget constraint defined by given income and prices.

Let C be the Weak and Strong Axioms of revealed preference, which are stated in testable form involving $\Sigma P_j Q_j$, price-quantity data. My above arguments will show how misleading it is to think such tests are in any genuine sense "indirect" ones.

It happens that C implies B as well as being implied by it. It is nonsense to think that C could be realistic and B unrealistic, and nonsense to think that the unrealism of B could then arise and be irrelevant.

But suppose the Weak Axiom, $C-$, is valid and the Strong Axiom is definitely not. If the F-Twist means anything, it says, "Never mind that B is unrealistic; its consequence $C-$ is realistic and that is all that counts."

Surely this is nonsense. B has been shown to be empirically false. That $C-$, one of its implications, is valid does not in any way atone for the fact that $(C) - (C-)$ is definitely false. Only that part of B which is $C-$ has been vindicated by the validity of $C-$. That other part $(B) - (B-)$, has been refuted. The only sensible thing to do, I mean the only thing to do, is jettison $(B) - (B-)$ and replace as your theory $B-$. If you say, "But $B-$ is a truncated fragment of the organic whole B, and it is odd to call $B-$ my theory," I simply reply: "How do you define organic wholes, and anyway I'd rather have the valid tail of a theory than have an invalid dog's body attached to that tail. What is required is not Occam's Razor so much as God's Hatchet."

Similar examples could be given where it is a question of maximizing profit and not utility. Let me add as an aside that I should be astonished to find a beast who consistently satisfied the Weak Axiom and consistently violated the Strong Axiom. That beast has a nonintegrable preference field. While I can see why a man with a mind should exercise it consistently, I fail to see why a beast with no mind should satisfy the Weak Axiom or even consistency of demand choices. I am here applying Samuelson's Razor, which, unlike Occam's which is primarily aesthetic, is based on a lifetime of sad experience: All economic regularities that have no common-sense core that you can explain to your wife will soon fail. This cannot be said of all that you can explain to her; so my statement is not an empty one. It is considerations like this which make me think that the Alchian doctrine of survival adds something to the maximization hypothesis.

Almost all the remarks about the $S = \frac{1}{2}gt^2$ law for falling bodies that Friedman thinks support his thesis seem to me misleading. They could as well, or poorly, apply to a purely empirical theory that says: The first terms in a Taylor's expansion for motion of a body at rest released at $S = 0$ are of the form $S = 0 + 0t + \frac{1}{2}gt^2 +$ remainder. Galileo's simple theory, $S''(t) = +g$, has a subset of consequences that is in tolerable agreement with some facts; e.g., for t "small," $S''(t) = +g$. But it, B, is vastly inferior as every parachute jumper, golfer, and schoolboy knows, to B^* which says $S'' = -f(S') + g, f(0) = 0, f'(S') > 0$ and which correctly predicts $S'''(t) \neq 0$ and, $t \overset{\lim}{\rightarrow} \infty S'(t) =$ a constant; etc., etc.

To reject, as I was taught to do in Chicago, monopolistic competition on the ground that it is not a "nice, simple, unified" theory like that of perfect competition, is like insisting that $f(S') \equiv 0$ because that is simpler and more manageable. If perfect competition is the best simple theory in town, that is no excuse for saying we should regard it as a good theory if it is not a good theory. To use the F-Twist to minimize its imperfections or irrelevancies is, as I have argued, simply wrong.

We must not impose a regularity—or approximate regularity—in the complex facts which is not there. Good science discerns regularities and simplicities that are there in reality—I almost said "out there." Epicycles are more horrid than perfect circles, but the ancient astronomers were right to abandon perfect circles and not say, "Well, even if wrong or imperfect, they are the best wheels in town."

Post-Copernicans were also wrong to go to the stake for the belief that Keplerian ellipses, B, were a more correct theory than epicycles, B^*. Relativism should have told both sides that this was a nonsense issue. Actually, B^* is merely a representation of B and deductively $B \equiv B^*$. However, to imperfect human minds the B^* formulation "looks" simpler and has the great mnemonic virtues of "economical description" which Mach rightly recognizes as the essence of good science. Mach has few friends today: physicists who confuse the psychological process of arriving at notions with the validity of those notions find him sterile. I should record that my experience with economics led me to notions that seem much like Mach's.

There is a final point, which was perhaps not made explicitly by Nagel, Friedman, or Mach and yet which I feel I share with Einstein and practitioners of harder sciences.

Experience suggests that nature displays a mysterious simplicity if only we can discern it. This is a bonus and need not have been so. And unrealistic, abstract models often prove useful in the hunt for these regularities. (Sometimes they prove misleading to a whole generation of searchers.)

This psychological usefulness should not be confused with empirical validity. Black coffee may be useful to physicists, mathematicians, economists, and artists. But coffee is coffee. Such abstract models are like scaffolding used to build a structure; the structure must stand by itself. If the abstract models contain empirical falsities, we must jettison the models, not gloss over their inadequacies.

The empirical harm done by the F-Twist is this. In practice it leads to Humpty-Dumptiness. Lewis Carroll had Humpty-Dumpty use words any way he wanted to. I have in mind something different: Humpty-Dumpty uses the F-Twist to say, "What I choose to call an admissible amount of unrealism and empirical invalidity is the tolerable amount of unrealism."

The fact that nothing is perfectly accurate should not be an excuse to relax our standards of scrutiny of the empirical validity that the propositions of economics do or do not possess.

TOPICS IN ECONOMIC THEORY

INVESTMENT: FACT AND FANCY

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The title of this paper may seem to suggest that it will present the facts as opposed to the fancy regarding investment. It will rather, however, marshal facts in a new way in order to support one old fancy and argue against another.

The fancy we shall support is the acceleration principle: investment demand depends on, among other things, the rate of change of output. The fancy we shall dispute is that capital expenditures, at least in the large firms which account for most of aggregate investment, are much affected by the rate of total profits. The first fancy, it should be clear, is consistent with the view that investment is motivated by profit maximization, but this implies that investment will be responsive to its expected profitability and this may or may not relate to total profits—past, current, or future.

This is not the occasion to review the considerable quantity of econometric literature on investment.[1] As is well known, however, tracing the thread of the acceleration principle in empirical data has proved uncertain and, in the judgment of some observers, impossible. Many respected economists, while rejecting or accepting the acceleration principle in varying degrees, have thought to see in profits, retained profits, or internal funds a major determinant of business investment.

It is my view, as has been argued elsewhere, that difficulty in confirming the empirical content of the acceleration principle has stemmed frequently from failure to formulate that principle appropriately. The second order difference equation expressing investment as a linear function of the previous one period change in income is a fine pedagogical tool, but one should hardly be surprised to find that it does not describe the real world. Even the earliest formulations of the acceleration principle, such as those of J. M. Clark, pointed out some of the complications introduced by lags and excess capacity. More recently,

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[1] For such a review, see Robert Eisner and Robert H. Strotz, "Determinants of Business Investment," Research Study Number Two in *Impacts of Monetary Policy*, prepared for the Commission on Money and Credit (Prentice-Hall, 1963), pp. 60-333.

the path to significant empirical work was broadened by the discussion of a distributed lag investment function in Hicks's *Trade Cycle.* This path has now been tread by Koyck and others, including the present writer, with what appear to me to be very promising results. Difficulties have remained, and it is to some of these that we shall now address ourselves.

The acceleration principle, in a relatively strong form, may be taken to relate to the proposition that, with a linear homogenous production function and constant factor proportions, the elasticity of capital stock with respect to output is unity. Empirical findings, however, have suggested that this elasticity is less than unity and, in all but the extremely long run, very substantially less than unity. Put another way, the data have indicated that, for example, a 10 per cent increase in sales would generate in a reasonably short period an amount of investment equal to no more than 1 or 2 per cent of capital stock and would even after a period of five or six years generate an investment equal to no more than 4 or 5 per cent of capital stock.

Now it may be noted that this notion that capital stock should vary proportionately with output is not dissimilar to the view that consumption is a linear homogenous function of income—a view which has until recently had a similar abundance of apparently contradicting empirical findings. Certain innovations in our theory and consequent estimation of the consumption function have resulted in major breakthroughs in that area in recent years. I intend to demonstrate in this paper that at least some of those innovations are applicable to the investment function with strikingly similar results.

The innovation that I have particularly in mind involves the hypothesis that consumption is a stable function of "permanent" income and that its relation to "measured" or observed income is an unstable proxy relation which depends in turn upon the relative size of variances of observed and permanent incomes. With regard to the investment function we shall now argue that investment is a stable function of "permanent" changes in output and that the enigmatic results of many past investigations have stemmed from attempts to estimate the unstable proxy relation including large elements of "transitory" changes in output. And even more particularly, as in the case of previous work with consumption, we shall support this view by analysis of variance and covariance involving a comparison of between-group and within-group regression coefficients.[2]

The basic data under analysis are gross capital expenditures, capital expenditure anticipations, sales, profits before taxes, depreciation charges, and gross fixed assets, as gathered from McGraw-Hill capital expenditure surveys from 1955 to 1958 and related financial statements. (These have been made available to the writer on an individual firm basis by code number so that the confidential nature of responses in the McGraw-Hill surveys has not been violated.) Depreciation charges and gross fixed assets are taken as their accounting figures, based on original cost. All other variables were price deflated.[3] The relations estimated have been three, which we shall refer to as the investment function, the anticipations function, and the realizations function. These may be written, respectively:

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[2] Cf. my "The Permanent Income Hypothesis: Comment," *A.E.R.*, Dec., 1958, pp. 972-90.

$$i_t = b_{10} + \sum_{j=1}^{7} b_{1j}\Delta s_{t+1-j} + b_{18}p_{t-1} + b_{19}d_{53} + u_1, \tag{1}$$

$$i_t^{t-1} = b_{20} + \sum_{j=2}^{7} b_{2j}\Delta s_{t+1-j} + b_{28}p_{t-1} + b_{29}d_{53} + u_2, \tag{2}$$

and

$$i_t = b_{30} + \sum_{j=1}^{7} b_{3j}\Delta s_{t+1-j} + b_{38}p_{t-1} + b_{39}d_{53} + b_{310}i_t^{t-1} + u_3, \tag{3}$$

where, with superscripts indicating year at end of which anticipations were expressed, subscripts indicating year to which variables refer, and t going from 1955 to 1958,

$i_t = \frac{I_t}{F_{53}}$ = gross capital expenditures deflated by gross fixed assets, a measure of the relative change in capital stock,

$i_t^{t-1} = \frac{I_t^{t-1}}{F_{53}}$ = capital expenditure anticipations deflated by gross fixed assets,

$\Delta s_t = \frac{S_t - S_{t-1}}{\frac{1}{3}(S_{52}+S_{53}+S_{54})}$ = the change in sales deflated by average sales in a base period, a measure of the relative change in demand,

$p_{t-1} = \frac{P_{t-1}}{F_{53}}$ = previous year's profits before taxes deflated by 1953 gross fixed assets, a measure of the profit rate,

$d_{53} = \frac{D_{53}}{F_{53}}$ = 1953 depreciation charges divided by 1953 gross fixed assets, a measure of (the inverse of) durability of capital and replacement requirements, and

u_1, u_2, and u_3 are disturbance terms.

Observations were divided into eight industry groups: primary metals, metal working, chemical processing, all other manufacturing, mining and petroleum, utilities, transportation and commercial. For each group there were in turn four sets of observations for the data relating to capital expenditures of (or anticipations for) 1955, 1956, 1957, and 1958 (except in the case of utilities, for which the 1955 observations were incomplete). Thus there were thirty-one industry-year groups of observations.

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[3] A further description of the data and methods of price deflation will be found in the writer's "Capital Expenditures, Profits and the Acceleration Principle," in *Models of Income Determination*, a forthcoming volume of *Studies in Income and Wealth*, to be published for the National Bureau of Economic Research by the Princeton University Press.

Our "permanent acceleration hypothesis" implies that firms will invest to the extent that they believe increases in demand are permanent. Hence, for given increases in sales, investment will be less the greater the proportions of the increases which are deemed transitory. Setting up a test analogous to that previously performed with regard to the permanent income hypothesis of the consumption function, we assume that business decision-makers recognize that departures of their own experience from concurrent experiences of the industry are likely to be in large part random and transitory in character, while interindustry and intertemporal differences will reflect in much greater proportion long-run or permanent factors. In a sense this is merely a reflection of the law of large numbers. For example, chemical processing firms making investment decisions geared to expected future demand are likely to consider the average experience of sixty chemical processing firms, rather than their own deviations from that average, a reliable predictor of future demand in the industry, of which they can realistically only count on keeping their accustomed share.

This suggests that ordinary, over-all cross-section regressions (such as those presented by Eisner in "A Distributed Lag Investment Function," *Econometrica,* January, 1960) of the form of our investment and anticipations functions (1) and (2), reflecting in part the transitory within-group variance of sales and only in part the permanent between-group variance, will be underestimates of the parameters of the structural, permanent accelerator relation. Regressions *within industries* (such as, for example, those of Meyer and Kuh, reported in *The Investment Decision: An Empirical Study,* Harvard University Press, 1957) will tend to underestimate sales change or accelerator coefficients even more. But appropriately weighted regressions based on group means, on the assumption that the means of transitory elements in sales experience in each group are all zero or, at least, do not differ between groups, will yield unbiased estimates of the parameters of the accelerator relation.

More specifically, the permanent acceleration hypothesis implies that *apparent* contradictions of the argument that capital stock is a linear homogenous function of output would be noted in regressions of investment on sales changes in which the variance of sales changes is in part transitory. Where the variance in sales changes is essentially

TABLE 1

INVESTMENT FUNCTION

CAPITAL EXPENDITURES AS FUNCTION OF SALES CHANGES, PROFITS, AND DEPRECIATION

(1)	(2)		(3)	(4)	(5)	(6)
Variable or Statistic	Regression Coefficients and Standard Errors				Correlation Coefficients	
	Within-group		Over-all	Between-group	Over-all: Simple and Partial	Within-group and Between-group: Simple
	(a) Weighted Mean	(b) Pooled				
Constant term	.033 (.005)	—	.035 (.004)	.010	—	—
Δs_t	.084 (.013)	.080 (.012)	.085 (.011)	.091 (.071)	.233 .199	.223 .303
Δs_{t-1}	.108 (.014)	.097 (.012)	.116 (.011)	.263 (.089)	.272 .269	.261 .345
Δs_{t-2}	.088 (.014)	.093 (.012)	.092 (.011)	.145 (.054)	.168 .228	.219 −.100
Δs_{t-3}	.073 (.015)	.075 (.012)	.085 (.011)	.177 (.050)	.147 .206	.162 .078
Δs_{t-4}	.075 (.015)	.089 (.013)	.101 (.012)	.197 (.077)	.201 .216	.172 .376
Δs_{t-5}	.041 (.016)	.051 (.014)	.043 (.013)	.008 (.071)	.097 .088	.112 −.008
Δs_{t-6}	.028 (.016)	.038 (.014)	.052 (.013)	.047 (.081)	.168 .109	.136 .344
p_{t-1}	.047 (.010)	.041 (.010)	.033 (.009)	−.058 (.075)	.290 .098	.272 .409
d_{it}	.781 (.087)	.819 (.071)	.771 (.066)	1.419 (.541)	.383 .298	.394 .348
$\Sigma\Delta s$ coefficients t to $t-4$	.429	.434 (.027)	.477 (.026)	.873 (.173)	—	—
t to $t-6$	.499	.523 (.035)	.572 (.033)	.929 (.191)	—	—
d.f.	1,357	1,357	1,387	30	—	—
$\bar{R}^2$	.322	.327	.351	.582	—	—

$F(9,1369) = 3.921$ for differences among pooled-within-group, over-all and between-group regressions.

permanent, as in the case of mean sales changes of industry-year groups, we should expect the sum of sales change coefficients to approximate unity much more closely.

In Table 1 we present estimates of the investment function on the basis of (1) the over-all regression of 1,388 observations in eight industry groups in each of four years, (2) a pooled within-group regression based on variances and covariances around industry-year means, hence a weighted average of within-industry-year regressions, and (3) a regression based on the thirty-one industry-year group means, weighted by the number of observations within each group. Results of the over-all regression are not new.[4] They reveal, in conformity with a distributed lag version of the acceleration principle, that capital expenditures are a function of a number of past yearly sales changes. They also show that while preceding profits are positively associated with investment, the *partial* correlation coefficient of capital expenditures with profits and the corresponding regression coefficient, though significantly positive, are small. The sum of the sales change regression coefficients, however, is only .572, which might suggest that the elasticity of capital stock to output is less than unity (possibly there are increasing returns to scale) or that a substantial portion of the cross-section variance in sales is transitory in nature and is so regarded by those that make decisions with regard to capital expenditures.

Now if the regression coefficients are being biased downward by a transitory component of sales changes, they should be smaller where the variance of sales changes is in larger proportion transitory. Hence, granting our assumption about the nature of variance of sales experience, the weighted means of within-group sales change coefficients[5] should be even less than the over-all coefficients. Table 1 reveals this to be precisely the fact.

While the differences are in a number of instances very small, all seven of the weighted means of the within-group sales change coefficients are less than the over-all coefficients. The sum of these within-group sales change regression coefficients is only .499. The somewhat higher mean within-group profits coefficient is consistent with the view that profits are merely a proxy for the relation between expected future demand and current capacity. Therefore, where the sales change variables capture less of the permanent variation of sales, which would relate to expected future demand, the proxy role of profits is enlarged.

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[4] They have been reported in "Capital Expenditures, Profits and the Acceleration Principle," *loc. cit.*

[5] Weights are the reciprocals of the diagonal elements of the inverses of the individual group product matrices. Pooled-within-group regression statistics are also presented in each of the tables. They have been calculated from the squares and cross products of deviations about individual group means, summed over all groups. The weighted means

Most dramatic of all is the set of between-group regression coefficients. In all cases except the last two, in regard to which it may be argued the lags are too long for sales changes to be important, the between-group sales change coefficients are higher, generally markedly higher, than the over-all coefficients and, a fortiori, higher than the within-group coefficients.[6] The sum of the between-group sales change coefficients is .929, clearly within hailing distance of unity. Finally, with regard to the profits coefficient, we may note that in spite of a simple correlation with capital expenditures of .409, the partial regression coefficient, with permanent sales change variance taking the place of the usual proxy factor in profits, is actually negative.

The markedly higher depreciation coefficient in the between-group regression may be taken to suggest that the depreciation variable is really nothing more than a proxy for replacement requirements and that the over-all regression coefficient of depreciation is biased downward by the "noise" related to individual firm differences in accounting practice (including such elements as accelerated amortization)—a noise which is submerged in the group means. It should be noted that the higher depreciation coefficient in the between-group regression is offset by a lower constant term as well as the negative profits coefficient, so that the effect of increasing sales, as estimated by the sum of the products of mean sales changes and their regression coefficients, accounts for .04 of the mean value of capital expenditures (as a ratio of fixed assets) of .108, some 37 per cent of the total.

Table 2 presents a quite similar set of results for estimates of equation (2), in which capital expenditure anticipations are the dependent variable. It might be argued that anticipations themselves tend to represent a more stable or permanent element in investment demand, less modified than actual expenditures by short-run factors such as those of supply which may constrain the accelerator relation. Be that as it may, the sum of the six sales change coefficients in the between-group capital expenditure anticipations regression is 1.004, as close to unity as any theorist should wish, and significantly higher than the corresponding sum of .503 for pooled within-group coefficients.

Table 3 offers within-group, over-all and between-group estimates

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of individual group regression coefficients approximate but are not identical with the pooled within-group coefficients.

[6] The sum of the first five between-group sales change coefficients exceeded the corresponding sum of pooled within-group coefficients by .439; the estimated standard error of that difference is .175 and the value of t is therefore 2.51. F-ratios of relevant residual variances among within-group, over-all and between-group regressions exceed .01 probability points in all relations.

TABLE 2

ANTICIPATIONS FUNCTION

CAPITAL EXPENDITURE ANTICIPATIONS AS FUNCTION OF SALES CHANGES, PROFITS, AND DEPRECIATION

(1)	(2)		(3)	(4)
VARIABLE OR STATISTIC	REGRESSION COEFFICIENTS AND STANDARD ERRORS			
	Within-group		Over-all	Between-group
	(a) Weighted Mean	(b) Pooled		
Constant term	.031 (.006)	—	.036 (.004)	.017
Δs_{t-1}	.118 (.016)	.117 (.013)	.143 (.012)	.275 (.084)
Δs_{t-2}	.104 (.015)	.099 (.012)	.097 (.011)	.148 (.049)
Δs_{t-3}	.064 (.016)	.069 (.013)	.081 (.012)	.174 (.048)
Δs_{t-4}	.102 (.016)	.119 (.014)	.134 (.013)	.239 (.052)
Δs_{t-5}	.062 (.017)	.064 (.015)	.063 (.014)	.062 (.066)
Δs_{t-6}	.034 (.017)	.034 (.015)	.062 (.014)	.108 (.077)
p_{t-1}	.046 (.012)	.032 (.010)	.017 (.010)	−.091 (.069)
d_{53}	.976 (.095)	1.007 (.076)	.902 (.072)	1.447 (.509)
$\Sigma\Delta s$ coefficients from $t-1$ to $t-4$	.389	.405 (.027)	.454 (.026)	.835 (.162)
from $t-1$ to $t-6$	.484	.503 (.037)	.579 (.035)	1.004 (.181)
d.f.	1,357	1,357	1,387	30
$\hat{R}^2$	.316	.317	.336	.634

$F(8,1371)=5.708$ for differences among pooled-within-group, over-all and between-group regressions.

of the realizations function (3). One will note here that while capital expenditure anticipations go a long way toward explaining actual capital expenditures, sales changes that occur subsequent to the point of time at which anticipations are formulated, that is the sales encompassed in Δs_t, are positively related to the difference between actual

TABLE 3

REALIZATIONS FUNCTION

CAPITAL EXPENDITURES AS FUNCTION OF CAPITAL EXPENDITURE ANTICIPATIONS SALES CHANGES, PROFITS, AND DEPRECIATION

(1)	(2)		(3)	(4)	(5)	(6)
VARIABLE OR STATISTIC	REGRESSION COEFFICIENTS AND STANDARD ERRORS				MEANS AND OVER-ALL STANDARD DEVIATIONS	WITHIN-GROUP AND BETWEEN-GROUP STANDARD DEVIATIONS
	Within-group		Over-all	Between-group		
	(a) Weighted Mean	(b) Pooled				
Constant term or i_t	.012 (.004)	—	.010 (.002)	−.006	.108 .076	.072 .025
Δs_t	.056 (.009)	.042 (.008)	.048 (.007)	.068 (.026)	.039 .155	.142 .067
Δs_{t-1}	.027 (.009)	.017 (.008)	.015 (.007)	.000 (.039)	.045 .159	.146 .067
Δs_{t-2}	.017 (.009)	.022 (.008)	.019 (.007)	−.003 (.023)	.050 .160	.144 .075
Δs_{t-3}	.029 (.009)	.022 (.008)	.024 (.007)	.010 (.023)	.043 .155	.138 .073
Δs_{t-4}	.015 (.010)	.012 (.009)	.015 (.008)	−.015 (.033)	.039 .143	.129 .066
Δs_{t-5}	.004 (.010)	.012 (.009)	.004 (.008)	−.046 (.026)	.062 .130	.122 .049
Δs_{t-6}	.009 (.010)	.015 (.009)	.007 (.008)	−.058 (.030)	.038 .138	.121 .069
p_{t-1}	.019 (.007)	.018 (.006)	.019 (.006)	.023 (.028)	.249 .202	.182 .093
d_{53}	.121 (.063)	.119 (.049)	.130 (.044)	.056 (.225)	.051 .027	.024 .013
i_t^{t-1}	.702 (.019)	.712 (.016)	.723 (.016)	.962 (.080)	.113 .081	.077 .026
$d.f.$	1,357	1,357	1,387	30	—	—
R^2	.722	.720	.741	.946	—	—

$F(10,1367)=2.691$ for differences among pooled-within-group, over-all and between-group regressions.

and anticipated capital expenditures. Curiously, however, while previous sales changes have significantly positive (though small) coefficients in the within-group and over-all regressions, the earlier sales change coefficients in the between-group regression, though generally small relative to their standard errors, show some tendency to be negative. These results, taken in conjunction with the larger sales change coefficients in equation (2) than equation (1), suggest that industry-year mean anticipations overstated the amount of expenditures related to prior sales changes. One may conjecture that anticipations reflected essentially investment demand and that actual investment was less than anticipated when high aggregative or industry sales induced sufficient investment to bring on shortages of supply.

As to the role of profits, the small positive over-all and mean within-group coefficients may lend some comfort to the views of those who argue that if not the total at least the timing of investment or its distribution among firms is partially influenced by recent or current profits. But doubt is placed on even this limited role when it is recognized that the intragroup sales change variance may be in such large part transitory as to allow profits to serve as a proxy for the ratio of expected demand to capacity. The true structural role of profits is then seen in the low (even negative) coefficients of the profits variable in the between-group regressions.

Fact and fancy can thus after all live together, if we have the right fancy and the right facts. The right fancy is the acceleration principle. The right facts must be those that relate investment to changes in demand which may be viewed as permanent.

CAPITAL THEORY AND INVESTMENT BEHAVIOR*

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Introduction

There is no greater gap between economic theory and econometric practice than that which characterizes the literature on business investment in fixed capital. According to the neoclassical theory of capital, as expounded for example by Irving Fisher, a production plan for the firm is chosen so as to maximize utility over time. Under certain well-known conditions this leads to maximization of the net worth of the enterprise as the criterion for optimal capital accumulation. Capital is accumulated to provide capital services, which are inputs to the productive process. For convenience the relationship between inputs, including the input of capital services, and output is summarized in a production function. Although this theory has been known for at least fifty years, it is currently undergoing a great revival in interest. The theory appears to be gaining increasing currency and more widespread understanding.

By contrast, the econometric literature on business investment consists of *ad hoc* descriptive generalizations such as the "capacity principle," the "profit principle," and the like. Given sufficient imprecision, one can rationalize any generalization of this type by an appeal to "theory." However, even with the aid of much ambiguity, it is impossible to reconcile the theory of the econometric literature on investment with the neoclassical theory of optimal capital accumulation. The central feature of the neoclassical theory is the response of the demand for capital to changes in relative factor prices or the ratio of factor prices to the price of output. This feature is entirely absent from the econometric literature on investment.

It is difficult to reconcile the steady advance in the acceptance of the neoclassical theory of capital with the steady march of the econometric literature in a direction which appears to be diametrically opposite. It is true that there have been attempts to validate the theory. Both profits and capacity theorists have tried a rate of interest here or a price of investment goods there. By and large these efforts have been unsuccessful; the naïve positivist can only conclude, so much the worse for the theory. I believe that a case can be made that previous attempts to "test" the neoclassical theory of capital have fallen so far

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short of a correct formulation of this theory that the issue of the validity of the neoclassical theory remains undecided. There is not sufficient space to document this point in detail here; but I will try to illustrate what I would regard as a correct formulation of the theory in what follows.

Stated baldly, the purpose of this paper is to present a theory of investment behavior based on the neoclassical theory of optimal accumulation of capital. Of course, demand for capital is not demand for investment. The short-run determination of investment behavior depends on the time form of lagged response to changes in the demand for capital. For simplicity, the time form of lagged response will be assumed to be fixed. At the same time a more general hypothesis about the form of the lag is admitted than that customary in the literature. Finally, it will be assumed that replacement investment is proportional to capital stock. This assumption, while customary, has a deep justification which will be presented below. A number of empirical tests of the theory is presented, along with an analysis of new evidence on the time form of lagged response and changes in the long-run demand for capital resulting from changes in underlying market conditions and in the tax structure.

Summary of the Theory

Demand for capital stock is determined to maximize net worth. Net worth is defined as the integral of discounted net revenues; all prices, including the interest rate, are taken as fixed. Net revenue is defined as current revenue less expenditure on both current and capital account, including taxes. Let revenue before taxes at time t be $R(t)$, direct taxes, $D(t)$, and r the rate of interest. Net worth, say W, is

$$W = \int_0^\infty e^{-rt}[R(t) - D(t)]dt.$$

We will deduce necessary conditions for maximization of net worth for two inputs—one current and one capital—and one output. The approach is easily generalized to any number of inputs and outputs.

Let p be the price of output, s the wage rate, q the price of capital goods, Q the quantity of output, L the quantity of variable input, say labor, and I the rate of investment; net revenue is

$$R = pQ - sL - qI.$$

Let u be the rate of direct taxation, v the proportion of replacement chargeable against income for tax purposes, w the proportion of interest, and x the proportion of capital losses chargeable against income; where K is capital stock and δ the rate of replacement, direct taxes are

$$D = u[pQ - sL - (v\delta q + wrq - x\dot{q})K]$$

Maximizing net worth subject to a standard neoclassical production function and the constraint that the rate of growth of capital stock is investment less replacement, we obtain the marginal productivity conditions

$$\frac{\partial Q}{\partial L} = \frac{s}{p},$$

$$\frac{\partial Q}{\partial K} = \frac{q\left[\dfrac{1-uv}{1-u}\delta + \dfrac{1-uw}{1-u}r - \dfrac{1-ux}{1-u}\dfrac{\dot{q}}{q}\right]}{p}.$$

The numerator of the second fraction is the "shadow" price or implicit rental of one unit of capital service per period of time. We will call this price the user cost of capital. We assume that all capital gains are regarded as "transitory," so that the formula for user cost, say c, reduces to

$$c = q\left[\frac{1-uv}{1-u}\delta + \frac{1-uw}{1-u}r\right].$$

Second, we assume that output and employment on the one hand and capital stock on the other are determined by a kind of iterative process. In each period, production and employment are set at the levels given by the first marginal productivity condition and the production function with capital stock fixed at its current level; demand for capital is set at the level given by the second marginal productivity condition, given output and employment. With stationary market conditions, such a process is easily seen to converge to the desired maximum of net worth. Let K^* represent the desired amount of capital stock, if the production function is Cobb-Douglas with elasticity of output with respect to capital, γ,

$$K^* = \gamma\frac{pQ}{c}.$$

We suppose that the distribution of times to completion of new investment projects is fixed. Let the proportion of projects completed in time τ be w_τ. If investment in new projects is I_t^E and the level of starts of new projects is I_t^N, investment is a weighted average of past starts:

$$I_t^E = \sum_{\tau=0}^{\infty} w_\tau I_{t-\tau}^N = w(L) I_t^N ,$$

where $w(L)$ is a power series in the lag operator, L. We assume that in each period new projects are initiated until the backlog of uncompleted projects is equal to the difference between desired capital stock, K_t^*, and actual capital stock, K_t:

$$I_t^N = K_t^* - [K_t + (1 - w_0) I_{t-1}^N + \cdots],$$

which implies that:

$$I_t^E = w(L)[K_t^* - K_{t-1}^*].$$

It is easy to incorporate intermediate stages of the investment process into the theory. For concreteness, we consider the case of two intermediate stages, which will turn out to be anticipated investment, two quarters hence, and anticipated investment, one quarter hence. A similar approach can be applied to additional intermediate stages such as appropriations or commitments. The distribution of completions of the first stage, given new project starts, may be described by a sequence, say $\{v_{0\tau}\}$; similarly, the distribution of completions of a second stage, given completion of the first stage, may be described by a sequence $\{v_{1\tau}\}$. Finally, the distribution of investment expenditures, given completion of a second intermediate stage is described by a sequence $\{v_{2\tau}\}$. Where $I_t^{S_1E}$ represents completions of the first stage, $I_t^{S_2E}$ completions of the second stage, and I_t^E actual investment, as before, we have:

$$\begin{aligned}
I_t^{S_1E} &= \sum_{\tau=0}^{\infty} v_{0\tau} I_{t-\tau}^N = v_0(L) I_t^N ,\\
I_t^{S_2E} &= \sum_{\tau=0}^{\infty} v_{1\tau} I_{t-\tau}^{S_1E} = v_1(L) I_t^{S_1E} ,\\
I_t^E &= \sum_{\tau=0}^{\infty} v_{2\tau} I_{t-\tau}^{S_2E} = v_2(L) I_t^{S_2E} .
\end{aligned}$$

where $v_0(L)$, $v_1(L)$, and $v_2(L)$ are power series in the lag operator.

Up to this point we have discussed investment generated by an increase in desired capital stock. Total investment, say I_t, is the sum of investment for expansion and investment for replacement, say I_t^R:

$$I_t = I_t^E + I_t^R .$$

We assume that replacement investment is proportional to capital stock. The justification for this assumption is that the appropriate model for replacement is not the distribution of replacements for a single investment over time but rather the infinite stream of replacements generated by a single investment; in the language of probability theory, replacement is a recurrent event. It is a fundamental result of renewal theory that replacements for such an infinite stream approach a constant proportion of capital stock for (almost) any distribution of replacements for a single investment and for any initial age distribution of capital stock. This is true for both constant and growing capital stocks. Representing the replacement proportion by δ, as before,

$$I_t^R = \delta K_t;$$

combining this relationship with the corresponding relationship for investment in new projects, we have:

$$I_t = w(L)[K_t^* - K_{t-1}^*] + \delta K_t.$$

Using the assumption that capital stock is continued in use up to the point at which it is replaced, we obtain the corresponding relationships for gross investment at each of the intermediate stages, say $I_t^{S_1}$ and $I_t^{S_2}$:

$$\begin{aligned} I_t^{S_1} &= v_0(L)[K_t^* - K_{t-1}^*] + \delta K_t, \\ I_t^{S_2} &= v_1(L)v_0(L)[K_t^* - K_{t-1}^*] + \delta K_t; \end{aligned}$$

we can also derive the following;

$$\begin{aligned} I_t^{S_2} &= v_1(L)[I_t^{S_1} - \delta K_t] + \delta K_t, \\ I_t &= v_2(L)[I_t^{S_2} - \delta K_t] + \delta K_t, \\ I_t &= v_2(L)v_1(L)[I_t^{S_1} - \delta K_t] + \delta K_t. \end{aligned}$$

For empirical implementation of the theory of investment behavior, it is essential that each of the power series—$v_0(L)$, $v_1(L)$, $v_2(L)$—have coefficients generated by a rational function; for example,

$$w(L) = v_2(L)v_1(L)v_0(L) = \frac{s(L)}{t(L)},$$

where $s(L)$ and $t(L)$ are polynomials. We will call the distribution corresponding to the coefficients of such a power series a rational power series distribution. The geometric and Pascal distributions are among the many special instances of the rational power series distribution.

Empirical Results

To test the theory of investment behavior summarized in the preceding section, the corresponding stochastic equations have been fitted to quarterly data for U. S. manufacturing for the period 1948–60. The data on investment are taken from the OBE-SEC Survey; first and second anticipations of investment expenditure as reported in that Survey are taken as intermediate stages.[1] With two intermediate stages, six possible relationships may be fitted. First, for actual investment and both intermediate stages, the level of investment is determined by past changes in desired capital stock. Second, investment is determined by past values at each intermediate stage and the second anticipation is determined by past values of the first anticipation. The first test of the theory is the internal consistency of direct and derived estimates of the coefficients of each of the underlying power series in the lag operator.

The results of the fitting are given in Table 1. For each of the fitted relationships coefficients of the polynomials $s(L)$ and $t(L)$ in the ex-

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[1] Data on capital stock were obtained by interpolating the capital stock series for total manufacturing given in the U.S. national accounts between 1949 and 1959, using the formula

$$K_{t+1} = I_t + (1 - \delta)K_t.$$

Given an investment series, a unique value of δ may be determined from the initial and terminal values of capital stock. Investment data from the OBE-SEC Survey were used for the interpolation. For desired capital stock, the quantity pQ was taken to be sales plus changes in inventories, both from the *Survey of Current Business*. User cost depends on a number of separate pieces of data. The quantity q is an investment deflator, δ is, of course, a fixed parameter (taken to be equal to .025), r is the U.S. government long-term bond rate. The tax functions vary with time; as an example, the tax rate u is the ratio between corporate income tax payments and corporate profits before taxes as reported in the U.S. national accounts. A detailed description of the data underlying this study will be reported elsewhere.

[2] To derive the form of the functions used in the actual fitting, we take $v_2(L)v_1(L)V_0(L)$ as an example. First:

$$I_t = \frac{s(L)}{t(L)}[K_t^* - K_{t-1}^*] + \delta K_t.$$

Secondly,

$$I_t = s(L)[K_t^* - K_{t-1}^*] + [1 - t(L)][I_t - \delta K_t] + \delta K_t.$$

The coefficient t_0 may be normalized at unity so that:

$$1 - t(L) = - t_1L - t_2L^2 - \cdots$$

The a priori value $\delta = .025$ was used to compute $I_{t-\tau} - \delta K_{t-\tau}$. An estimate of δ is given by the coefficient of K_t. If δ is different from its a priori value, the process of estimation can be reiterated, using a second approximation to the value of δ.

The parameter γ is estimated using the constraint:

$$\sum_{\tau=0}^{\infty} w_\tau = 1.$$

TABLE 1

Regression Coefficients and Goodness of Fit Statistics, Unrestricted Estimates

Regression	γs_0	γs_1	γs_2	t_1	t_2	δ	R^2	s	Δ^2/s^2
$v_2v_1v_0$			.00102 (.00049)	−1.51911 (.09945)	.63560 (.10098)	.02556 (.00163)	.94265	.10841	2.14039
v_1v_0		.00132 (.00073)		−1.25242 (.12667)	.36656 (.12977)	.02618 (.00240)	.89024	.16229	2.00431
v_0	.00109 (.00085)			−1.26004 (.13044)	.37281 (.13138)	.02549 (.00278)	.87227	.18974	2.37298
v_2v_1			.81357 (.03492)			.01962 (.00175)	.92729	.11955	1.16294
v_2		.90024 (.02722)				.02295 (.00127)	.96234	.08604	1.47693
v_1		.89462 (.03145)				.02337 (.00155)	.95121	.10597	1.70179

Regression Coefficients and Goodness of Fit Statistics, Restricted Estimates

Regression	γs_0	γs_1	γs_2	t_1	t_2	δ	R^2	s	Δ^2/s^2
$v_2v_1v_0$			.00106 (.00049)	−1.52387 (.09925)	.63100 (.10074)		.94156	.10830	2.10778
v_1v_0		.00133 (.00073)		−1.25704 (.12509)	.36769 (.12862)		.88986	.16087	2.00549
v_0	.00109 (.00084)			−1.26395 (.12942)	.37300 (.13051)		.87128	.18848	2.53442
v_2v_1			.82764 (.04037)				.89995	.13883	.87127
v_2		.91545 (.02933)					.95406	.09409	1.23560
v_1		.90271 (.03276)					.94538	.11100	1.53759

pression for each power series as a rational function are given.[2] For example, the power series $v_2(L)v_1(L)v_0(L)$ is expressed as:

$$v_2(L)v_1(L)v_0(L) = \frac{.00106L^2}{1 - 1.52387L + .63100L^2}$$

The value of the replacement proportion δ estimated from data on capital stock is .025. Two sets of regressions were run, one with δ fitted from the data (unrestricted), the other with $\delta = .025$ (restricted). Throughout, the coefficient of multiple determination R^2, the standard error of estimate for the regression s, and the VonNeumann ratio Δ^2/s^2 are presented as measures of goodness of fit.

The first set of tests of the theory is the comparison of alternative estimates of each of the fundamental power series. As an example, one may take the hypothesis that the direct estimates of the power series $v_2(L)$ and $v_1(L)$, when combined, give an estimate of $v_2(L)v_1(L)$ which is close to that obtained by direct estimation. Using the unrestricted estimates, the result of this comparison is:

$$.91545L \odot .90271L = .82639L^2;$$

the derived estimate, which may be compared with the direct estimate, $.82764L^2$. The difference between the two estimates is slightly over .03 standard errors. A similar test of the hypothesis that the direct estimates of the power series $v_1(L)$ and $v_0(L)$, when combined, yield an estimate of $v_1(L)v_0(L)$ which is close to that obtained by direct estimation results in

$$.00109 \cdot \frac{.90271L}{1 - 1.26395L + .37300L^2} = \frac{.00098L}{1 - 1.26395L + .37300L^2},$$

which may be compared with the direct estimate,

$$\frac{.00133L}{1 - 1.25704L + .36769L^2}.$$

The coefficient of the numerator is within half a standard error of the derived estimate. The coefficients of the denominator are within .06 and .04 standard errors of the derived estimates. The similarity of derived and direct estimates for the power series $v_2(L)v_1(L)v_0(L)$ is less striking. The three possible derived estimates are extremely similar to each other, but they differ considerably from the direct estimate. Nevertheless, using any of the derived estimates as the null hypothesis for a test of the direct estimates would probably lead to acceptance of the null hypothesis. In general, the theory of investment behavior is strongly confirmed by the set of tests of internal consistency. Of course, given the internal consistency of the alternative estimates, it is possible to improve efficiency of estimation for the model as a whole by combining information from the various sources.

The tests of internal consistency just described are tests of the theory of investment in new projects. A test of the theory of replacement investment is a test of the consistency of the empirical results with the hypothesis $\delta = .025$. This hypothesis is borne out in two ways. First, for all but one of the regressions, the usual null hypothesis is accepted; a much stronger result is that for the first three regressions, estimates of the relationships under the restriction that $\delta = .025$ results in a reduction in the standard error of estimate for the regression. Finally, each of the standard errors of the estimates of δ is less than one-tenth the size of the corresponding regression coefficient. We conclude that the hypothesis that replacement is a constant fraction of capital stock, specifically, that $\delta = .025$, is strongly validated by the empirical results.

We turn now to comparisons of the fitted regressions with some simple alternatives. First, as alternatives for the first three regressions, we take the naïve models:

$$I_t = I_{t-1},$$
$$I_t^{S_2} = I_{t-1}^{S_2},$$
$$I_t^{S_1} = I_{t-1}^{S_1}.$$

Simple as these models may be, they are quite stringent standards for comparison for seasonally adjusted quarterly data, much more stringent, for example, than the corresponding models for annual data. The appropriate statistics for comparison are the standard errors of estimate and the VonNeumann ratios. Results of this comparison are given separately for the periods 1948–60 and second quarter 1955 to 1960 in Table 2.[3] For the period as a whole, each of the regression models has a standard error well below that for the corresponding naïve model. For the later subperiod the advantage of the regression models is even greater. Turning to the VonNeumann ratios, there is practically no evidence of autocorrelated errors for the fitted models and very clear evidence of autocorrelation for the naïve models. Of course, this test is biased in favor of the fitted regressions. Even with this qualification, the fitted regressions are clearly superior in every respect to the corresponding naïve models.

As a standard of comparison for the second three regressions, we take the forecasts actually used by the Department of Commerce in presenting the results of the OBE-SEC Survey. These alternative models take the form:

$$I_t = I_{t-2}^{S_1},$$
$$I_t = I_{t-1}^{S_2},$$
$$I_t^{S_2} = I_{t-1}^{S_1}.$$

Despite the high level of performance of the OBE-SEC anticipations data, the fitted regressions constitute a substantial improvement in both goodness of fit as measured by standard error of estimate and absence of autocorrelation of residuals. The test for autocorrelation is not biased in favor of the fitted regressions, so that the evidence is unequivocal; the fitted relationships are clearly superior to the corresponding forecasting models for the period as a whole and for the subperiod since second quarter 1955.

A further comparison of the fitted regressions with the corresponding naïve and forecasting models is given in the second half of Table 2, where an analysis of the conformity of turning points of each of the

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[3] Data for both anticipations and actual expenditures on a revised basis are available from the Department of Commerce only since the second quarter of 1955. Anticipations data for the earlier period were revised by multiplying each observation by the ratio of revised to unrevised actual investment for the period in which the observation was made.

TABLE 2

Goodness of Fit Statistics: Fitted, Naive, and Forecasting Models

Model	1948I–1960IV			1955II–1960IV			1948I–1960IV			1955II–1960IV		
	R^2	s	Δ^2/s^2	R^2	s	Δ^2/s^2	TP Error	Over-estimate	Under-estimate	TP Error	Over-estimate	Under-estimate
$I_t=f(\Delta K_t^*)$	.94156	.10830	2.10778	.94298	.11378	1.95800	29%	47%	24%	23%	41%	36%
$I_t^{a2}=f(\Delta K_t^*)$	.88986	.16087	2.00549	.94757	.09368	1.84699	29	43	27	41	32	27
$I_t^{a1}=f(\Delta K_t^*)$	.87128	.18848	2.53442	.91921	.14372	2.25394	39	33	27	41	27	32
$I_t=I_{t-1}$	.86193	.15950	.66900	.81929	.18410	.52078	22	39	39	23	32	45
$I_t^{a2}=I_{t-1}^{a2}$	.84966	.18058	1.04366	.81297	.19435	.94465	24	37	39	32	27	41
$I_t^{a1}=I_{t-1}^{a1}$	.83854	.20282	1.31901	.81161	.19947	.83580	35	25	39	32	27	41
$I_t=f(I_t^{a1})$	.92729	.11955	1.16294	.92169	.12996	1.09855	16	43	41	23	41	36
$I_t=f(I_t^{a2})$	.96234	.08604	1.47693	.95931	.09368	1.65030	14	45	41	18	45	36
$I_t^{a2}=f(I_t^{a1})$	.95121	.10597	1.70179	.96477	.09045	2.26525	16	45	39	23	41	36
$I_t=I_t^{a1}$	.83673	.17391	.69933	.77138	.20707	.46505	25	47	27	27	50	23
$I_t=I_t^{a2}$	.93504	.10969	.99342	.91854	.12359	.91146	14	51	35	18	50	32
$I_t^{a2}=I_{t-1}^{a1}$	.93380	.11983	1.45737	.92833	.12031	1.25062	20	49	31	18	45	36

Note: Total percentages may not add to 100% because of rounding error.

"forecasts" to the turning points of the actual data is presented. In general, the first set of fitted regressions is slightly inferior to the naïve models and the second set slightly superior to the forecasting models on the basis of this criterion. A final comparison is between the fitted regression of investment on changes in desired capital stock and the forecast of investment from its second anticipation. The comparison favors the fitted regression; however, the anticipations data used in a fitted relationship between investment and second anticipation provide a model which is superior to the simple forecasting model and to the fitted regression of investment on changes in desired capital stock.

Structure of the Investment Process

In the preceding sections, only those aspects of the theory of investment behavior relevant to testing the theory were presented. In this section certain further implications of the theory are developed. Specifically, we will characterize the long-term response of investment to changes in the underlying market conditions and the tax structure and the time-pattern of response of investment to changes in demand for capital.

First, using the facts that gross investment is determined by the relationship:

$$I_t = w(L)[K_t^* - K_{t-1}^*] + \delta K_t$$

and that capital stock is determined by past investments, we obtain:

$$\begin{aligned} I_t &= [1 - (1 - \delta)L]w(L)K_t^*, \\ &= y(L)K_t^*, \end{aligned}$$

where $y(L)$ is a power series in the lag operator. We define the τ-period response of investment to a change in market conditions or tax structure as the change in gross investment resulting from a change in the underlying conditions which persists for τ periods. More precisely, suppose that desired capital remains at a fixed level for τ periods to the present; then,

$$K_t^* = K_{t-\nu}^*, \qquad (\nu = 1, 2 \cdots \tau),$$

and

$$\begin{aligned} I_t &= \sum_{\nu=0}^{\infty} y_\nu K_{t-\nu}^*, \\ &= s_\tau K_t^* + \sum_{\nu=\tau+1}^{\infty} y_\nu K_{t-\nu}^*, \end{aligned}$$

where $\{z_\tau\}$ is the sequence of cumulative sums of the coefficients of $y(L)$. As an example, the response of gross investment to a change in the rate of interest is:

$$\frac{\partial I}{\partial r} = z_\tau \frac{\partial K^*}{\partial r}.$$

The coefficients $\{z_\tau\}$ characterize the time pattern of response. Obviously,

$$\lim_{\tau \to \infty} z_\tau = \lim_{\tau \to \infty} \sum_{\nu=0}^{\tau} y_\nu = \delta,$$

so that the long-term response of gross investment to changes in, say, the rate of interest, is

$$\frac{\partial I}{\partial r} = \delta \frac{\partial K^*}{\partial r}.$$

Clearly, the short-term responses approach the long-term response as a limit; the approach is not necessarily monotone, since the coefficients of the power series $y(L)$ are not necessarily non-negative.

Long-term response and elasticities of gross investment with respect to the price of output, price of capital goods, and the rate of interest are given in the top half of Table 3. The corresponding responses and elasticities for the income tax rate, the proportion of replacement and the proportion of interest chargeable against income for tax purposes are given in the bottom half of Table 3. It should be noted that the rate of interest and the tax rate are measured as proportions, not percentages. For example, a decrease in the rate of interest by 1 per cent increases manufacturing gross investment by \$.15178 billions per quarter in the long run, at least to a first approximation.

The time pattern of response is presented in Table 4, where the functions $w(L)$, $y(L)$, and $z(L)$ are derived from the fitted regressions. The

TABLE 3

RESPONSES AND ELASTICITIES OF INVESTMENT WITH RESPECT TO CHANGES IN MARKET CONDITIONS AND TAX STRUCTURE

	RESPONSE		ELASTICITY	
	Average	End of Period	Average	End of Period
Market Conditions				
Price of output	.35830	.35299	1.00000	1.00000
Price of capital goods	−.35273	−.32106	−1.00000	−1.00000
Rate of interest	−14.23653	−15.17789	−.29143	−.37866
Tax Structure				
Income tax rate	−.37487	−.33016	−.50959	−.42064
Proportion of replacement	.18729	.20502	.39181	.48565
Proportion of interest	.55656	.79840	.19428	.32659

TABLE 4

TIME FORM OF LAGGED RESPONSE

Lag	$w(L)$	$y(L)$	$z(L)$
0	0	0	0
1	0	0	0
2	.11277	.11277	.11277
3	.14209	.03214	.14491
4	.13700	−.00154	.14337
5	.11965	−.01393	.12944
6	.09969	−.01697	.11247
7	.08101	−.01619	.09628
8	.06491	−.01407	.08221
9	.05159	−.01170	.07051
10	.04081	−.00949	.06102
11	.03219	−.00760	.05342
12	.02535	−.00604	.04738
13	.01994	−.00478	.04260
14	.01567	−.00377	.03883
15	.01231	−.00297	.03586
16	.00967	−.00233	.03353
17	.00760	−.00183	.03170
18	.00597	−.00144	.03026
19	.00469	−.00113	.02913
20	.00368	−.00089	.02824
Remaining..............	.01346	−.00325	
Rate of decline..........	.78531	.78531	

average lag between change in demand for capital stock and the corresponding net investment is, roughly, 6.5 quarters or about a year and a half. Of course, this estimate is affected by the essentially arbitrary decision to set the proportion of the change invested in the same period and period immediately following the change at zero. The coefficients of the power series $z(L)$ are of interest for computation of short-period responses of investment to changes in the demand for capital stock. For example, the 2-period response of manufacturing gross investment to a change in the rate of interest of 1 per cent is:

$$z_\tau \frac{\partial K^*}{\partial r} = \frac{.11277}{.02500} \cdot .15178 = .68465 \text{ billions/quarter.}$$

By comparison, the corresponding 10-period response is .37046 billions per quarter. The response dies out, almost to its long-term level of .15178 billions/quarter, by twenty periods from the initial change in demand for capital stock. Similar calculations of the response of gross investment to changes in market conditions or the tax structure may be made for any of the six determinants of demand for capital by combining the responses given in Table 3 with the time pattern presented in Table 4.

THEORY AND INSTITUTIONS IN THE STUDY OF INVESTMENT BEHAVIOR

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In this paper I shall sketch the formal methods of accepted capital theory and the accepted theory of investment, together with some major institutional qualifications. This will serve as a prelude to summarizing the present state of empirical information about the relative importance of profit and acceleration determinants of investment. Having set myself so ambitious a task, I cannot avoid a certain amount of oversimplification of the work I do cover. In addition, many important contributors to the development of these subjects must go unnamed, since I do not intend to present a complete history of economic thought on this subject.

The foundations of capital theory in its modern form have been best articulated by Irving Fisher in *The Theory of Interest* [16]. It is a theory at the level of microeconomic choice and at the level of total market price determination which has the major ingredients, correctly related to each other, that a capital theory should have. The fundamental ingredients are the productivity of capital and thrift. The opportunity to profit from capital requires the sacrifice of present for future income on terms reflected by the market rate of interest. This is an equilibrium theory of capital. It certainly is not a theory of investment.

A theory of investment purports to explain the rate of change in physical capital in order to achieve a desired stock. While Keynes gave only an informal outline in the *General Theory* [27], a dynamic cycle theory of saving and investment was stated by Harrod in 1936 [19]. This was made precise in the Hansen-Samuelson 1939 paper on the multiplier-acceleration principle [36], an approach which was constructed at the macrolevel although it seemed compatible with microbehavior. Saving depends on income alone and the equilibrium capital stock is unrelated to interest rates. The parentage of the acceleration component in fully developed form goes back to J. M. Clark's 1917 article [7] and still earlier to Aftalion [1], Bickerdike [2], and Hawtrey [20]. In the Clark formulation the optimal stock of capital is taken to be a datum and proportional to output. The present versus future choice element so prominent in Fisher's theory was completely thrust aside.

Our intellectual armory thus includes a theory of the determinants of the optimal stock with no adjustment process and an adjustment process without an optimizing basis. It must be observed that J. M. Clark was clearly aware of limitations on the rigid accelerator.

Deficiencies in the dynamic adjustment process of the original acceleration principle formulation have since been largely cleared up, partly through the distributed lag accelerator of Hicks [21] but more particularly through the work of Richard M. Goodwin [17], Hollis Chenery [5], and L. M. Koyck [28]. They have elaborated an adjustment process designed to erase gradually a disequilibrium between desired and actual capital stock according to a distributed lag pattern rather than instantaneously according to the rigid original acceleration theory. Kaldor had earlier presented a graphic and verbal description of investment behavior with a flavor similar to that of the gradual capital stock adjustment theories [24]. A variety of complex issues usually submerged in deriving the adjustment process have also been analyzed by Haavelmo [18]. Finally, in presently unpublished work, Dale Jorgenson [23] has sought to generalize the adjustment lag structure and at the same time to take explicit account of entrepreneurial net worth maximization subject to a production function constraint.

According to Fisherian capital theory, the individual saver and the real asset purchaser can be the same individual. Keynes and the multiplier-accelerator devotees seem rather to have assumed a strict dichotomy between the consumer-saver and the entrepreneur-investor. Neither theory considers third party intermediation with still different attitudes toward capital productivity and consumption, like the modern business corporation, for example. Both theories also assume perfectly competitive operation of the capital markets and tend to ignore the effect of taxes on investment decisions.

The business corporation has been on the landscape for about a hundred years and its existence is pretty hard to ignore, or at least it should be. Its presence poses a real problem for the savings-investment dichotomy, and thus for much of the theory described above. If corporations did not save a substantial fraction of their net income, this problem might not arise. Corporations would finance increases in their net assets through the capital markets and pay out all net income as dividends; thereby in fact preserving the motivational or physical distinction between saver and real asset purchaser which the theory assumes. Alternatively, corporations could save but act exactly as a hypothetical individual seeking to maximize intertemporal utility.

But corporations do save, and plenty at that. Net corporate saving currently averages about half of total personal saving and gross corporate saving is roughly twice the rate of personal saving in the United

States. The distinction between gross and net saving is fairly important here because, in theory, depreciation is the current account (noncash) outlay corresponding to the given period's "using up" of capital. Despite the highly arbitrary element in gross corporate saving, we draw attention to the distinction between gross and net saving, even though a complete capital theory would encompass both aspects on the demand side.

It is also worth noting that students of economic development have bestowed on corporate savings a central position in the development process. This importance depends on the proposition that high marginal corporate or entrepreneurial saving propensities out of profit are likely to provide the main source of private abstinence required to generate capital in the poverty stricken regions of the world [6] [35].

In addition, we have a further factual observation: that corporate saving and profits are positively correlated with each other and with investment. Despite these correlations—and of course we are all so sophisticated that we know that correlation does not imply causation even though some believe the converse holds—profits or retained earnings need not be a basic investment determinant since cyclical fluctuations in output, the main ingredient of the acceleration principle (along with the stock of capital) are also correlated with profit fluctuations. Furthermore, the causal direction might run from investment to profits, a proposition developed by Kaldor [25] to explain full employment income distribution, partly along lines Kalecki [26] had suggested earlier.

In light of existing knowledge of prevalent corporate pricing policies and cost characteristics we would expect corporate profit changes to be heavily determined by changes in sales. Most students of the subject would accept the existence of rather rigid price behavior in the manufacturing sector. This phenomenon perhaps is best explained in the short run by markup policies on unit variable cost modified by fluctuations in the degree of capacity utilization reflecting variations in excess demand. In addition, moderate curvature at most in short-run marginal cost functions for wide ranges of operating outputs appears to be a widespread production characteristic. Under these assumptions an approximately linear relation between profits and sales would result. This short-run relationship is, I believe, quite generally thought to exist, particularly in manufacturing [30] [31]. Its existence makes it very difficult to discriminate between hypotheses which have investment moved mainly by sales and those which have investment moved mainly by profits.

The familiar fact should be recalled that something like three-quarters of total fund sources in the manufacturing sector come from internal funds. Because of a greater resort to external funds by public utilities, the average for all corporations is now just under two-thirds. Nevertheless, widespread empirical observation of the real world, much of it of a casual sort (like much theorizing) and some of it quite intense and systematic (also like some theory), has led to the view that corporate managers have a definite preference for internal funds [12] [22] [34]. Using currently fashionable terminology we could, perhaps, say they act as if they preferred internal to external funds. An alternative formulation is to say that the imputed cost of internal funds is less than the cost of external funds [30] [31]. What would be the implication of such attitudes on the part of corporate management?

The main propositions that emerge are quite simply these: first, the greater are gross profits, the greater will be the level of internally generated funds, given normal dividend behavior [32] and, second, the greater are internal funds, the greater will be the rate of investment. These propositions, perhaps derivable in part from the theory of uncertainty, perhaps derivable from preference theory where preference structures between stockholders and corporate managers differ systematically, are certainly an elaboration on the existing corpus of theory even though not intrinsically antithetical to it. Imperfectly competitive operation of capital markets could yield similar consequences, as could a tax structure with different marginal rates for personal income and capital gains. Furthermore, since a substantial amount of investment is for '"modernization" or replacement, cost of capital considerations of the sort described here could have an important influence on this component of gross investment. It is inappropriate for serious students of the subject to reject these propositions as inconsistent with existing theory on a priori grounds, particularly since the predictive and explanatory capacity of existing theory is far from perfect.

The previous discussion of hypotheses about the effects of profits and internal finance on investment has treated the productivity of capital as given. The assertion can be defended that the propensity to invest is almost certainly not related dollar for dollar to the availability of internal funds. Some naïve interpretations of the internal funds approach assert that excess internal funds are "hot money" that must wind up in physical assets irrespective of the productivity of capital. While this could happen in some instances, there are no reasons to believe that such behavior is the norm, particularly since mergers, dividends, liquid asset accumulation and refinancing often provide valid and apparent alternatives for corporate management.

Indeed, there is reason to believe that corporate income retention is motivated by expected favorable investment opportunities and, further, that the extent to which firms do in fact use external funds depends upon the rate of output growth and its profitability [11] [30] [34]. To the extent that net retentions are dependent upon expected investment opportunities, an outcome parallel to competitively determined behavior will result. Furthermore, when direct competition is operative, be it pure, oligopolistic or from close product substitutes, possibilities that the distribution of real capital among firms will depart markedly from the market outcome through investing retained earnings will be strictly limited. Profitable investment opportunities will generate net profits. These profits will tend to be driven toward zero as capital accumulation absorbs the most profitable investments so that reinvestment of quasi-rents will diminish as the rents themselves diminish. Where market forces operate imperfectly—and there is reason to believe that such circumstances prevail in several important sectors of the economy—heavy reliance upon internal funds clearly could lead to the misuse of resources through avoidance of the market place.

Let us now turn to the quantitative evidence. The main conclusion to be drawn from an examination of studies that have tested both propositions concurrently is this: Capacity accelerator motivation is more important than profits or internal funds in explaining the cyclical path of investment although profits still have a significant, if secondary, role to play. The role could arise either through the internal funds cost approach which has been advanced here, or possibly for expectational reasons. However, when profits are viewed primarily as an expectational variable, expected output is probably the dominant motivation to which profits are closely related.

The studies to be cited come from diverse sources, and some are not yet published. One of the most interesting results has been provided by Robert Eisner [14], who began by dividing about 200 firms into three groups, according to whether their recent profits were relatively high, moderate, or low. Within each of the three profit-experience categories, cross-section 1955 firm investment was regressed against three preceding year sales changes and three preceding year profit rates and two other variables. The squared multiple correlation coefficient was nearly twice as great for the high profit group as it was for the other two profit groups, while the regression coefficients were roughly of the same magnitude. Thus, a strong interaction exists between the effectiveness of the accelerator and recent profit experience—an effect similar to that noted by Meyer and myself. Eisner neglected to pursue this provocative result further. From numerous other regression analyses he finally concluded that "profits variables, while showing positive zero order relations with capital expenditures, do not generally reveal similarly significant coefficients in our multiple regressions; profit coefficients are indeed frequently negative" ([14, page 28]).

In more recent work using related data and techniques, Eisner [15] found profit coefficients to be statistically relevant in four industries, but not in four others; in four out of four years using pooled moments regressions and none out of four using industry year mean data; in two out of four years and for firms with asset size less than $100 million but not for larger firms. The last mentioned result is ascribed to capital market imperfections. Continued insistence on the "proxy" role of profits seems to rely on using theory to test the facts rather than facts to test alternative theories. As I have argued elsewhere [29], however, cross sections are unlikely to be the correct sort of data on which to test essentially dynamic phenomena, so that it would be imprudent to place exclusive reliance on Eisner's interesting negative conclusion.

A distributed lag investment relation has been estimated recently for the Commission on Money and Credit by four authors: Robert M. Solow, E. Cary Brown, Albert Ando, and John Kareken [3]. They assert that "we do not wish to choose once and for all between a profits theory on one side and an acceleration theory on the other—nor do we believe the two to be necessarily mutually exclusive. . . . We seem to get stabler empirical results when we include corporate profits as an independent variable and exclude a rough measure of 'capacity' (meant as a surrogate for the stock of capital itself)" ([3, pages 77-78]). They estimated the lagged dependence of quarterly orders for producer durable equipment orders on the following variables: output measured by the Federal Reserve Board Index of Industrial Production, a private long-term interest rate and corporate profits.

High elasticities of investment orders with respect to gross profits appeared, the smallest impact elasticity being .50 with a corresponding long-run elasticity of 1.20. Corresponding output elasticities are .27 and .67. Even when two lagged dependent variables were included, profits continued to be a major statistical explanatory variable.

In some unpublished work [33], John R. Meyer and Robert Glauber have estimated time series investment functions for about fifteen two-digit manufacturing industries and the aggregate, using a capacity-accelerator formulation along with internal funds and several other variables, with postwar quarterly data. The capacity-accelerator variable for total manufacturing has a short-run impact elasticity of .68 in contrast to one of .28 for gross internal funds. From related quarterly postwar data for manufacturing, Frank de Leeuw came to similar conclusions for the best fitting "inverted V" lag structure, one of several that he explored in some detail [8]. His capacity requirement variable, whose short-term movements are governed by output variations, has an impact elasticity of about four-fifths and the gross internal fund flow impact elasticity ranges from about one-fifth to two-fifths. Like my results, but unlike those of Meyer and Glauber, De Leeuw found capacity requirements coefficient estimates statistically more reliable than those for internal funds. For two other lag structures, the internal funds variable was statistically insignificant, but capacity requirements remained highly significant. Using annual time series for sixty individual capital-producing firms for the period 1935 to 1955 (excluding the war years), I came to similar conclusions; namely, that the capacity-accelerator influence was substantially greater than that for internal funds [30]. The typical elasticity of investment with respect to sales was about 1.0 on an annual basis for my firms and about .15 to .10 for the internal fund flows.

Earlier John R. Meyer and I, using cross-section data for 1946-50, reported that the capacity accelerator was the primary determinant of investment in two different circumstances [34]. First, in the "long run," where long-run data were represented by five-year data averages (which substantially reduce short-run transient influences), and also when capacity pressures were extremely high, using short-run annual data. We also found that during periods of less intense capacity pressures, profits and depreciation, interpreted primarily as measures of internal liquidity flows, turned out to be relatively more influential. In these circumstances the statistical explanatory power of profits was not overwhelming, but much more evident than the accelerator influence. The last conclusion was dependent on year to year comparisons of changes in the degree of cross-section explanation, in order to extract as much time series content from the cross sections as seemed feasible. Others have since found broadly similar results beyond our original sample period for the years 1950 to 1955 [4] [33].

In another study which elaborated on some aspect of Eisner's work, James J. Diamond reports results that would lead us to believe that both motivations have some validity: "This study also tended to substantiate Eisner's findings on the proxy role of profits; however, we did find an occasional significant relationship between profits and investment when the acceleration principle broke down" ([9, page 372]).

Investment theory and empirical knowledge about it seem to be left in the following situation. Received doctrine which emphasizes the gradual adjustment of the capital stock to a desired level dependent on expected output levels and lagged capital stock provides the greater part of the empirical substance, as measured by usual statistical relations.[1] It should be added that interest rates are now showing up in quarterly econometric models as statistically significant investment determinants. Nevertheless, internally generated funds or profits appear to have an observable effect on investment for given levels of output, capital stock, and interest rates, although the effect is a subordinate one. On both prior and posterior grounds both motivations seem relevant, acceleration being dominant and *ex post* profits, on the whole, being secondary.

Most discussion in this paper on quantitative research has been limited to the cyclical determinants of investment behavior. A great deal of confusion on cyclical behavior during the past fifteen years has been generated and then cleared up. Capital theory and production function variants of that theme provide numerous qualitative insights into the structure of the long-run determinants of capital; yet we still have only modest knowledge about the way cyclically influenced investment is translated into long-run desired capital assets. One need only recall the uncertainty with which the recent investment tax credit proposal was and is surrounded to illustrate this observation. The theoretical and most enlightening analysis of capital by Irving Fisher, as greatly amplified in the linear programming work of Dorfman, Samuelson, and Solow [10], has yet to be implemented or empirically measured on a substantial scale.[2] Perhaps it is time for more economists to turn their attention away from the short-run problem and once more address themselves to the classical long-run problems of capital accumulation. Some modifications of that theory seem to be required in order to encompass the institutional structure and important aspects of the business decision process not fully reflected in a straightforward maximizing approach.

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[1] This position does not represent a major departure from previous views although investigators of investment behavior have tended more often than not to give excessive prominence to the Meyer-Kuh profit results. We then said: "Apparently, in the postwar years a capacity (accelerator) concept provided a reasonably adequate explanation of investment as long as all the usual accelerator assumptions or enabling conditions were met; once these conditions were not satisfied, however, the enabling factors themselves, particularly liquidity, assumed a dominant role in conditioning the investment decision" ([34, pp. 124–25]). And: "In the long run, on the other hand, investment outlays seemed less sensitive to financial considerations and more closely conditioned by the kind of technological relationships between capital and output central to acceleration theories" ([34, p. 135]).

[2] First steps in the direction of quantitative measurement of Fisherian concepts have been taken by Eckaus and Lefeber [13] following the linear programming elaborations of Dorfman, Samuelson, and Solow [10].

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DISCUSSION

CARL F. CHRIST: The invitation to take part in this session was especially welcome because Messrs. Eisner, Jorgenson, and Kuh are among those who have made and are making the greatest contributions to the econometric study of investment behavior.

One theme running through the session concerns the relation of actual current profits to investment. Kuh has summed up the situation well, on the whole. As to the future, it can be expected that actual current profits will lose explanatory power once all the relevant variables have been properly accounted for; e.g., expected rate of return on new investment, cost of capital (both internal and external), wage rates, expected future output, etc.

The papers share a second theme, to which I shall devote the remainder of my comments. It concerns the employment in empirical investment studies of theoretically relevant concepts such as the acceleration principle, relative prices, and expectations.

I agree that in theory the acceleration principle should work, in that the elasticity of capital's productive services with respect to the long-run expected level of output should be approximately 1, provided that there are constant returns to scale, no technological changes, no changes in relative prices, and no cases of excess capacity to begin with. I also agree with Eisner that the observed elasticity of capital stock with respect to observed output should be lower insofar as the changes in output are regarded as transitory rather than permanent.

Ordinarily we have better data for the capital stock than for the flow of capital's services. Eisner's procedure implicitly assumes that the flow of services is proportional to K_G, the gross stock of capital. Then the acceleration principle would require $\Delta K_G/K_G = \Delta S^e/S^e$ (where S^e stands for expected sales). Let I_G be gross investment and R be retirements. Then $\Delta K_G = I_G - R$, and so Eisner's dependent variable becomes

$$\frac{I_G}{K_G} = \frac{\Delta K_G + R}{K_G} = \frac{\Delta K_G}{K_G} + \frac{R}{K_G} = \frac{\Delta S^e}{S^e} + \frac{R}{K_G}$$

This is similar to Eisner's equation (1) with the sum of the coefficients of Δs_{t-1} being 1 and with the coefficient of the retirement variable also being 1.

Suppose instead that the flow of capital services were proportional to the net stock of capital K_N (which equals about $.6K_G$); then $\Delta K_N/K_N = \Delta S^e/S^e$. Then, if D is depreciation, we have for Eisner's dependent variable

$$\frac{I_G}{K_G} = \frac{\Delta K_N + D}{K_G} = \frac{K_N}{K_G}\,\frac{\Delta K_N}{K_N} + \frac{D}{K_G} \cong .6\,\frac{\Delta S^e}{S^e} + \frac{D}{K_G}$$

In this case the coefficient of $\Delta S^e/S^e$ (and hence also the sum of Eisner's coefficients of Δs_{t-1}) should be about .6 instead of 1. Since capital services presumably are proportional to something between the net and gross capital stock, the sum of Eisner's coefficients of Δs_{t-1} ought not to equal 1 if the sales changes used are permanent, but ought to lie between about 0.6 and 1.

Jorgenson's approach has the advantage of allowing for changes in the desired capital stock K^* in response to changes in relative prices, corporate tax rates, and the ratio of the capital service flow to the capital stock. His concept of the cost of capital services is the correct one.

Jorgenson's investment equation, to a first approximation that is valid if the relative price c/p of capital relative to output is constant, is like Eisner's in being a function of past changes in output Q (Eisner uses sales, not output). This is shown easily: Jorgenson's desired capital stock K^* equals $\gamma pQ/c$, whence

$$\Delta K^* = \gamma \Delta \left(\frac{pQ}{c} \right) \cong \gamma \frac{p}{c} \Delta Q$$

Hence substituting this for ΔK^* in Jorgenson's investment equation yields

$$I \cong \gamma \frac{p}{c} w(L) \Delta Q + \delta K$$

This is quite similar to Eisner's equation (1) except that (*a*) Eisner uses sales S, not output Q, and (*b*) Eisner's equation is deflated by gross capital stock K_G while Jorgenson's does not need to be because it is a time series equation with the data for any variable being of the same order of magnitude in all quarters. Jorgenson constrains his coefficients of ΔK^*_{t-i} to add up to 1, while Eisner does not constrain the sum of his coefficients of Δs_{t-i}. The time-shapes of the 2 graphs of Eisner's between-groups coefficients of Δs_{t-i} (in his Table 1) and Jorgenson's coefficients of ΔK^* (in his Table 4, "*w*" column) are quite similar after allowing for the fact that one is annual and constrained to be zero after 6 years, while the other is quarterly and not so constrained.

I close with a few questions. For Eisner: Do you not agree that part of the sales changes in the between-group data is regarded by firms as transitory? In the light of this and my earlier comments, would you not expect the sum of coefficients of Δs_{t-i} in the between-group case to be less than 1? Should not the depreciation coefficient be approximately 1 instead of 1.4 in Table 1, column 4 (see comments above)? Should not the constant term in your equation be zero? Why do you use sales not output? For Jorgenson: Why do you use output, not sales? Since the second and third stages of your investment process are essentially one-quarter-delayed versions of the preceding stage (i.e., $v_{1\tau}=0=v_{2\tau}$ except for $\tau=1$), should not $s_1=1$, instead of about 0.9 as you show in the estimated results for the v_1 and v_2 regressions in Table 1? How does your assumption of a fixed distribution of times from investment start to maturity allow for changes of investment plans after starts have been ma[illegible]?

The time limit precludes further discussion of these three excellent papers.

EDWIN MANSFIELD: Eisner, Jorgenson, and Kuh are to be congratulated for presenting three very interesting papers regarding the investment function. The papers complement one another in many ways, Eisner being concerned principally with expectational phenomena, Jorgenson being concerned with the dynamic adjustment process and the implications of profit maximization in a world devoid of uncertainty, and Kuh being concerned primarily with the apparent preference of entrepreneurs for internal funds as a means of financing investment.

Jorgenson's paper—certainly the most ambitious and probably the most important of the three papers—is very difficult to discuss or evaluate in detail because so many vital links in the argument are omitted because of limitations of space. In the copy I received, there is no discussion of the estimating procedures and basic data used to derive most of the empirical results. Before the paper is published, I hope that Jorgenson will add a more complete description of his model, methods, and data. It would make the paper, which is very interesting even in its present form, more convincing.

Beyond saying that Jorgenson's work is very interesting, I shall comment on two of his assumptions and compare some of his empirical results with those obtained by other methods. First, it may be misleading to assume that the distribution over time of the investment stemming from an investment "start" is fixed. I would expect that this distribution would vary considerably over the business cycle, because of variation in the rate of utilization of capacity in the capital-goods industries and because of the stretching-out of investment projects during periods of slack business. For obvious reasons, I would also expect this distribution to vary with changes over time in the product mix of the manufacturing sector.

Morever, unless Jorgenson makes the unlikely assumption that the desired capital stock, K_t^*, always exceeds the existing capital stock, K_t, plus the backlog of unfinished projects, $(1 - W_0)I_{t-1}^n + (1 - W_0 - W_1)I_{t-2}^n + \ldots$, the new projects initiated at time t may be negative. Making the reasonable assumption that Jorgenson interprets a negative amount of new projects as a cancellation of projects already started, it follows that he assumes that, if an ongoing project is canceled, the distribution over time of the resulting decrease in expenditures is the same as the distribution over time of the increase in expenditures that results when a project is begun. This does not seem very reasonable.

Second, the proposition in renewal theory that he uses to justify the hypothesis that replacement investment is proportional to capital stock assumes that the probability distribution for the length of life of a unit of new capital is the same over time. With technical change, shifts in factor and product prices, and changes in product mix, there may instead have been appreciable shifts over time in this probability distribution, and this assumption may not have been a very good approximation.

Turning to Jorgenson's empirical results, his success in fitting the 1948-60 data is quite impressive. However, it is by no means obvious that his model fits better than others suggested last year at these meetings by Okun (*Ameri-*

can Economic Review, May, 1962). To see how much additional accuracy results from the extra complications he includes, Jorgenson ought to compare the goodness-of-fit of his model with that of Okun's regressions, rather than with the unadjusted forecasts, $I_{t-1}^{S_2}$, and $I_{t-2}^{S_1}$. More important, it would be very useful if he would compare the predictive value of these models for 1960-62.

Comparing his estimates of $W(L)$, the time form of lagged response, with the lead times for fixed investment reported by Mayer and Sonenblum (*Review of Economic Statistics,* August, 1955), one finds that his average lag is considerably longer than theirs. Apparently, this is due in large part to the fact that his figures include the time it takes to make decisions, get bids, etc., as well as the time it takes to construct and install plant and equipment. His figures are about the same as Mayer's estimates in the *Journal of Business,* April, 1960. (Of course, his figures are influenced too by his somewhat arbitrary decision to set $W(0)$ and $W(1)$ equal to zero.)

Kuh's paper is a summary of the literature on the investment function, with special reference to the longstanding controversy between "profit" and "acceleration" theories. His conclusion seems eminently reasonable. Profits play a secondary role in influencing investment, the more important role being played by the accelerator. Relating Kuh's conclusions to the standard theories of equilibrium and dynamic adjustment, perhaps past profits or cash flow may be regarded as one important determinant of the rate at which the firm's capital stock increases toward a desired level which is based largely on profit maximization. Another important determinant of the rate of adjustment may be the size of the gap between actual and desired capital stock. Hypotheses of this sort seem to be useful in explaining investment in R and D, and I should think that they might also be useful here.

Eisner's paper, part of his continuing study of investment based largely on the McGraw-Hill capital expenditure surveys, contains some very interesting empirical results. In particular, the closeness to unity of the sum of the estimated sales-change coefficients in the between-group regression is striking.

Finally, I should call attention to the fact that technical change is ignored in all three papers, Jorgenson's paper assuming explicitly that the production function did not shift between 1948 and 1960. In a forthcoming paper, I show that the timing of innovation seemed to have a statistically significant and quantitatively important effect on the level and timing of expenditures on plant and equipment in the steel and petroleum industries. Since technical change is obviously important, it is unfortunate that none of these authors tried to incorporate it into their investment functions. In Jorgenson's paper, it could probably have been introduced into the model without too much difficulty.

Karl Borch: All the three papers express dissatisfaction with the present state of the theory of investment behavior. The authors seem particularly worried over the apparent success of much current econometric work which pays little attention to economic theory. However, a realistic theory of investment must be exceedingly complex; so the present unsatisfactory state should not really be a cause of surprise.

Professor Jorgenson suggests that firms seek to maximize an integral over discounted values of future net receipts. This is an attractive, but obviously very crude, model. There are at least two generalizations which immediately suggest themselves as desirable: to replace the constant discount rate by a utility function which expresses a preference for the timing of payments and to introduce a probability distribution or a stochastic process to allow for the uncertainty associated with future payments.

If it should be possible to construct a realistic theory of investment along these or other lines, it will be a very complex model, and it may contain so many parameters that it will be virtually impossible to test the theory against observations. Against this background, it is not surprising that current econometric work should show a healthy disrespect for theory. Why should we not take intuition as our guide when deciding which sets of economic data we should analyze? We may always find unexpected and significant correlations which can guide the next steps in our search. That this approach is fruitful is demonstrated in an eloquent manner by the three papers under discussion.

The three papers contain only a few short remarks about uncertainty. It seems, however, that by assuming complete certainty, the authors "assume away" an element which must be essential to any realistic theory of investment.

Professor Kuh discusses briefly corporate saving and the strong preference for internal funds shown by most corporations. Present theory appears unable to explain why such things exist. It seems, however, that by introducing uncertainty in the model, one should be able to provide a simple and natural explanation. The only way of being certain not to miss a favorable investment opportunity is to keep sufficient cash at hand. If we assume that a corporation seeks to maximize some expression of expected utility, we should be able to explain the "liquidity preference" and "propensity to save" which we observe in real life.

Professor Jorgenson's investment theory is based on two behavioral assumptions: (1) Capital stock is taken as fixed, and the firm determines the optimal level of output. This is a very reasonable short-run assumption. (2) The output level determined under the first assumption is taken as fixed, and the optimal amount of capital stock is determined. This seems to be a rather doubtful assumption.

The two assumptions together imply that investment is a trial-and-error process. Professor Jorgenson states that it is easy to see that the process will converge. However, if this is easy to see, why does not the firm see it? It seems natural to assume that intelligent management will see more than one step ahead in this process and try to optimize output level and capital stock simultaneously. This seems particularly natural under the perfect certainty assumed by Professor Jorgenson.

The model which Professor Jorgenson constructs on the basis of his questionable assumptions fits the data very well. The explanation may be that his model obviously can be derived from a number of different and more plausible behavior assumptions. His paper is therefore a good illustration of the point I made earlier: that theory should not be allowed to interfere with practical econometric work.

The converse is probably also true. We should not refrain from exploring theoretical models, even if their consequences sometimes seem to contradict observations from real life. I agree with the three authors that it is desirable that economic theory and empirical work should be brought closer together. I do, however, think it would be harmful to force the two together at the present stage in the development of our science.

INDUSTRIAL CAPACITY

AN APPRAISAL OF MEASURES OF CAPACITY

By Almarin Phillips
University of Virginia

In pure concept, the labor force and capital are much the same. Each is a stock of an economic resource available for the production of goods. Similarly, both the number of unemployed and the volume of excess capacity indicate the extent to which society is failing to use these resources. While the underlying social objectives are somewhat different, the recent interest in the measurement of capacity and the degree to which capacity is being utilized is not unlike that in measuring the labor force and unemployment a few decades ago. As was true of labor force measures in the late twenties and early thirties, there is today a growing feeling that more comprehensive and more current measures of industrial capacity are required. Knowledge of the magnitude of and reasons for unemployed resources are felt to be essential for remedial public policies.

Current research in capacity measurement can be more realistically appraised when cast in relation to the history of labor force statistics. In the latter, obvious deficiencies in the necessarily crude methods employed by private researchers and gross differences in their estimates eventually led to a large-scale government program. However valuable was the private research, it tends to be forgotten today, seemingly obscured over the years by masses of newer, more reliable data. Moreover, while few would now argue for the abandonment of labor force statistics, their availability has doubtlessly done less towards curing our economic ills than had been hoped for by the early workers. It should be recognized, then, that current measures of capacity are indeed no more than very early forerunners of comprehensive capacity measurements and, in addition, that should the latter appear in the future, they too will be something less than a Rosetta Stone which suddenly unravels vast economic mysteries.

Five separate but not completely independent research projects in capacity measurement will be reviewed. These are, in order, those of the McGraw-Hill Department of Economics, the National Industrial Conference Board, *Fortune* magazine, the Wharton School Econometrics Unit, and the Division of Research and Statistics, Federal Reserve System. This selection excludes many important but apparently discontinued measures attempted by others, many trade association estimates, and some research being done in connection with national security and post-attack resource management.[1] The methods and resulting estimates are presented in the next section. Following this, each is criticized and, in conclusion, observations on a possible government program are made.

Five Measures of Productive Capacity

McGraw-Hill Department of Economics. The McGraw-Hill capacity measures are derived from their Annual Surveys of Business' Plans for New Plants and Equipment. Precise data on the size and design of the sample and on the response are not published. Firms are selected from fifteen major industrial classifications and, while companies "that participate . . . are usually the larger companies in their industries," the surveys have become larger and "a concerted effort is made to include more medium-sized and smaller firms."[2] The survey is said to be "a representative cross section" of the industries involved and the companies included account for "about 40 per cent of total employment in all industry."[3]

The McGraw-Hill surveys began in 1947 and have included questions relative to recent and planned additions to capacity since 1948. The surveys are now conducted during March and April of each year. One of the questions asked is, "At the end of the [past] year, how did your capacity, measured in terms of physical volume, compare with what it was at the end of the previous year?"[4] Another question, which is now included in a fall survey as well, asks for the rate at which the companies were actually operating at the end of the year and occasionally a question is asked concerning the rates at which the companies would "prefer to operate."

No effort is made by McGraw-Hill to define capacity: ". . . companies set their own definitions" and "follow a commonsense definition of capacity, such as maximum output under normal work sched-

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[1] No effort has been made to provide a complete listing of these studies. In just the past decade, however, they include: Donald C. Streeter, *Capacity Utilization and Business Investment,* Univ. of Illinois Bulletin Series No. 86 (Mar., 1960); the Harvard Economic Research Project's capacity estimates for I-O industries in Leontief's dynamic model; studies by several universities and government agencies for the same purpose (some of which appear in *Problems in Capital Formation,* Studies in Income and Wealth, vol. 19, Princeton, 1957), studies by the National Planning Association in connection with their Program Analysis for Resource Management; and Edward Zabel, "Concepts and Measurement of Productive Capacity" (unpublished Ph.D. thesis, Princeton, 1955).

[2] *Business' Plans for New Plants and Equipment, 1961-1964,* 14th Annual Survey by the McGraw-Hill Department of Economics (New York, 1961), p. 19.

[3] *Ibid.* It appears that this means 40 per cent of total employment in manufacturing industries. See statement of Douglas Greenwald, *Measures of Productive Capacity,* Hearings before the Subcommittee on Economic Statistics of the Joint Economic Committee, 87th Cong. (May 14, 22, 23 and 24, 1962), p. 7. This source is cited as *Hearings* hereafter.

[4] *Hearings, loc. cit.,* p. 10.

TABLE 1

McGRAW-HILL ESTIMATES OF CAPACITY AND OPERATING RATES IN MANUFACTURING INDUSTRIES 1950–61

End of Year	Capacity Index (1950=100)	Actual Operating Rate	Preferred Operating Rate
1950	100	n.a.	n.a.
1951	105	n.a.	n.a.
1952	113	n.a.	n.a.
1953	124	n.a.	n.a.
1954	130	84	89
1955	139	92	n.a.
1956	148	86	90
1957	156	78	n.a.
1958	163	80	n.a.
1959	170	85	94
1960	177	77	n.a.
1961	182	83	90

SOURCE: *Business' Plans for New Plants and Equipment*, Annual Surveys of the McGraw-Hill Department of Economics.

ules."[5] Similarly, no attempts are made to define "physical volume" or to indicate what is meant by "preferred." The reports of individual companies are aggregated to industry figures through the use of employment weights. Industries are aggregated to the "All Manufacturing" category through the use of the value-added weights used in the Federal Reserve Index of Industrial Production.[6]

Table 1 presents the numerical results of the McGraw-Hill surveys for total manufacturing. Similar data for each of the fifteen industrial categories can be found in their published reports. The estimated increase in manufacturing capacity during the eleven years between the end of 1950 and the end of 1961 averages to 5.6 per cent per year. Plotted year by year, the index is nearly linear on an absolute scale, showing a generally declining percentage rate of increase, especially after 1955. During the seven years between the end of 1954 and the end of 1961, the increase in manufacturing production indicated by the operating rate relative to the capacity index amounted to about 4.7 per cent per year.

National Industrial Conference Board. The capacity estimates of the Conference Board have grown from the pioneering work of Daniel Creamer on capital-output ratios which was begun as a study at the National Bureau of Economic Research.[7] For purposes of the capacity measures, data on the value of fixed capital as reported on balance-sheet items of corporate income tax returns and published in *Statistics of Income* are used. Manufacturing industries are grouped into twenty-three major classifications. For each industry, the value of fixed capital used is the book value in 1929 prices, computed as original cost less straight-line depreciation deflated by an implicit price deflator based on the length of life and prices of capital goods.

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[5] *Ibid.*, p. 4.

[6] *Ibid.*, p. 12.

[7] Daniel Creamer, "Capital and Output Trends in Manufacturing Industries, 1880-1948," *Occasional Paper 41*, N.B.E.R. (1954), and Daniel Creamer, Sergei Dobrovolsky, and Israel Borenstein, *Capital in Manufacturing and Mining: Its Formation and Financing* (Princeton, 1960).

The estimation of constant dollar book value of fixed capital is an enormous and complex task. Values gross of depreciation are rejected because of the implicit assumption this would involve that capital is "fully used until the moment of retirement."[8] Book values net of depreciation assumes a gradual loss in value as capital loses its "economic effectiveness."[9] Straight-line depreciation over the "normal" length of life is regarded as the best approximation of this loss in value and, accordingly, it has been necessary to adjust the reported depreciation methods and the rapid amortization permitted since the Korean war.[10]

The imputed deflator used to derive the constant dollar net book value of machinery and equipment for each industry is based on (1) the average length of life for equipment in that industry as determined by depreciation studies, (2) the constant dollar value of machinery and equipment produced in each year during this average length of life, and (3) a price index for machinery and equipment. It is assumed that machinery and equipment were acquired during the span of years indicated by length of life in proportion to machinery and equipment production in those years. Then the price index of machinery and equipment for the years of this life is weighted in these proportions to obtain a weighted average price index for the industry. The index for structures is obtained in a similar fashion, but here an average length of life of fifty years is used for all industries. Finally, the indexes for machinery and equipment and for structures are combined according to the proportions of these items in total capital, and the net book value figures are divided by the combined index to yield constant dollar figures.

The output measures used are the gross operating receipts of the same industrially classified corporations, each adjusted for inventory changes and deflated to constant prices with wholesale price indexes. The capital-output ratios computed from these two deflated values are then examined for cyclical peak periods and a bench mark year is selected for which independent evidence and the capital-output ratio

[8] Daniel Creamer, *Capital Expansion and Capacity in Postwar Manufacturing*, Studies in Business Economics No. 72, Nat. Ind. Conf. Bd. (New York, 1961), p. 19n.

[9] *Ibid.*

[10] For details, see *Capital Expansion and Capacity in Postwar Manufacturing, loc. cit.*, Appendix B, and *Capital in Manufacturing and Mining, loc. cit.*, p. 219.

indicate "virtually full capacity" utilization. This particular ratio is then taken as the capital-capacity output ratio and is used to estimate the extent to which subsequent output rates depart from an imputed capacity output rate. For each industry, the computations are:

$$(1)\quad FC_1 \div \frac{FC_0}{CO_0} = CO_1$$

$$(2)\quad \frac{O_1}{CO_1} = \text{percentage of capacity utilized at time 1, where } FC_1 \text{ is}$$

fixed capital at time 1, FC_0 is fixed capital in bench mark period, CO_0 is the capacity outut in bench mark period, CO_1 is the estimated capacity output at time 1, and O_1 is actual output at time 1. If at subsequent cyclical peaks a capital-output ratio lower than the indicated capacity ratio appears, it is adopted as the capacity ratio from then on. The industries are aggregated to an "All Manufacturing" category by summing the fixed capital, estimated capacity outputs, and actual outputs for the several industries.

Table 2 gives the results of the Conference Board measures for selected years for all manufacturing. While the estimates need not be so restricted, these measures refer to the per cent of capacity utilized during the quarter of the year in which the seasonally adjusted annual output rate was the highest. Because of an unavoidable lag of nearly two years in the availability of *Statistics of Income* for corporate returns, reasonably current estimates using these methods cannot be made. The Conference Board does, however, extrapolate its

TABLE 2

NATIONAL INDUSTRIAL CONFERENCE BOARD ESTIMATES OF CAPACITY UTILIZATION RATES 1953–61

Year	Percentage of Capacity Utilized at Peak Output During Year
1953	100
1954	n.a.
1955	97
1956	91
1957	88
1958	87
1959	94
1960	93
1961	92

SOURCE: Daniel Creamer, *Capital Expansion and Capacity in Postwar Manufacturing*, Studies in Business Economics No. 72, National Industrial Conference Board (New York, 1961), revised and updated by information supplied to the Joint Economic Committee and reported in *Measures of Productive Capacity*, Report of the Subcommittee on Economic Statistics (July 24, 1962), page 16.

output series by using the Federal Reserve Board Index of Production and, similarly, extrapolate the capital stock estimates with the Federal Trade Commission—Security Exchange Commission Quarterly Financial Reports on capital expenditures.[11]

Fortune. It is difficult to summarize the methods used by *Fortune* magazine in estimating capacity because of the lack of a published report detailing the approach,[12] because the admitted use of subjective judgments in the determination of the data that are published,[13] and because of the apparently preliminary nature of what has so far been done.[14] It appears, however, that two estimates of the undepreciated stock of capital in manufacturing industries are made. In the first, purchases of capital goods are cumulated over time, and subtracted from these cumulations are retirements arrived at by using George Terborgh's "survival curves." Capital is divided into three classes: "plant," "capacity machinery," and "other machinery and equipment."[15]

The second method is through the use of balance sheets and income statements of *Statistics of Income*. Starting with a base of 1945, capital expenditures in each year are estimated by adding depreciation charges in that year to the change in the depreciated assets account between the beginning and the end of the year. The difference between capital expenditures in the year and change in gross capital is the retirements for the year, expressed in terms of undepreciated values.[16] Both methods yielded "about the same result."[17]

The capital stock series were then compared with selected other measures. For some industries, trade association, and other private measures of capacity or physical output were found. In addition, both Federal Reserve and McGraw-Hill output data were used for industry-by-industry comparisons of changes in peak output rates and the derived index of capital stock. The final *Fortune* index of capacity output "about split the difference" between the growth in stocks and McGraw-Hill's capacity figures for the metalworking industries; for the nonmetalworking group, the *Fortune* capacity output series was closer to the capital stock growth than to the McGraw-Hill capacity data.[18] In several industries, the differences between the *Fortune* and the McGraw-Hill capacity growth rates are large. In nonelectrical machinery, for example, *Fortune* found a capacity increase of 39 per cent between 1950 and 1957 while McGraw-Hill's estimate for the same period is 77 per cent.[19]

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[11] For a test of the extrapolation method for capital, see Creamer, Dobrovolsky, and Borenstein, *op. cit.*, pp. 205-07.

[12] The principal publication is Charles E. Silberman and Todd May, "How Much 'Overcapacity' in U.S. Manufacturing?" *Fortune* (Sept., 1958), in which the "highly technical statistical calculations that back up [the findings are not] detailed" (p. 129). See also Gilbert Burck and Sanford Parker, "The Dynamic Market for Capital Goods," *Fortune* (Sept., 1954); by the same authors, "The Mighty Multiplier," *Fortune* (Oct., 1954); an unsigned article, "The Crucial Customers for Capital Goods," *Fortune* (Nov., 1954); Charles Silberman and Sanford S. Parker, "Reinforcements for the Capital-Goods Boom," *Fortune* (Dec., 1954); by the same authors, "The Capital Goods Outlook," *Fortune* (Aug., 1958); an unsigned piece, "Investment Turn," in the Business Roundup section, *Fortune* (Oct., 1958); Charles E. Silberman and Sanford S. Parker, "Capital Goods: The Energy Explosion," *Fortune* (Nov., 1958); Sanford S. Parker and Todd May, "The Coming Boom in Capital Goods," *Fortune* (Dec., 1958), and Gilbert Burck and Sanford S. Parker, "The Dynamic Force of the Energy Industries," *Fortune* (Oct., 1962). The most detailed account of methodology is in "Statement of Sanford S. Parker . . . presented by Dr. Morris Cohen . . . ," *Hearings, loc. cit.*, pp. 67-82.

[13] *Hearings, loc. cit.*, p. 69.

[14] *Ibid.*

[15] *Ibid.*, pp. 69-70.

[16] For the previous use of the same method, see Raymond T. Bowman and Almarin Phillips, "Conceptual and Statistical Problems in Estimating Capital Coefficients for Four Metal Fabricating Industries," *Problems in Capital Formation, loc. cit.*, p. 372n.

[17] *Hearings*, p. 71.

TABLE 3

Fortune INDEXES OF CAPACITY AND OUTPUT GROWTH 1947–57 (1953 = 100)

Year	Real Capacity	Annual Output	Peak Output Rate of Year
1947	73	72	75
1948	78	75	77
1949	82	71	79
1950	85	83	91
1951	89	91	96
1952	96	91	99
1953	100	100	100
1954	103	94	101
1955	107	106	108
1956	113	109	113
1957	119	109	113

SOURCE: Read from graphs in Charles E. Silberman and Todd May, "How Much 'Overcapacity' in U.S. Manufacturing?" *Fortune* (September, 1958), page 130.

Fortune indexes of manufacturing capacity, annual output, and peak output rates for the years 1947-57 are shown in Table 3. Since each is based on 1953 as 100 and no estimate is provided of the degree of capacity utilization in 1953, it is impossible to compute a series showing the degree of capacity utilization.[20] The *Fortune* capacity index does grow at a lower rate than does McGraw-Hill's—about 5 per cent per year as against more than 6 per cent—for the years between 1950 and 1957 and, hence, the former studies imply "much less overbuilding of capacity."[21]

Fortune is also doing work in nonmanufacturing industries. The energy industries have received particular attention, but some analysis has been done of total stock of capital in the private economy and its relation to privately-produced gross national product.[22] The published results are so scant and the relations to capacity measurement are so vague, that this work is not considered here.

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[18] *Ibid.*, p. 72.

[19] *Ibid.*

[20] Unpublished data have been submitted to the Joint Economic Committee and the latter's estimates of utilization are published in *Measures of Productive Capacity*, Report of the Subcommittee on Economic Statistics (July 24, 1962), p. 16.

[21] Charles E. Silberman and Todd May, "How Much 'Overcapacity' in U.S. Manufacturing?" *loc. cit.*, p. 129.

Wharton School Econometrics Unit. The Wharton School capacity measure is the only one of those under review which has a fairly recent theoretical piece underlying it.[23] One use of the series is in the Wharton School quarterly model of the U.S. economy. Nonetheless, the procedure is extremely simple. Seasonally adjusted monthly values for each of thirty subdivisions of the Federal Reserve Board Index of Industrial Production are averaged into quarterly figures.[24] The quarterly figures are charted and peaks in each of the series are selected by inspection. Each peak is defined as "capacity" and a straight line from peak to peak describes capacity during the intervening periods. For the period after a peak but before another has been reached, the last straight line is extrapolated with the same slope until production intersects the line. After such intersection, capacity is taken to be the line connecting the last peak and the most recent production figure until a new peak is reached.

The results of the Wharton School method for total industrial capacity are shown in Table 4. As is true of the FRB Index from which they are derived, the industries include eight from the mining and utilities fields. The individual industries are combined to the total with the FRB value-added weights, with the mining and utility sectors having a combined weight of 13.711 per cent. Conceptually the combined index can reach 100 per cent as an upper limit, but this requires that each of the thirty groupings peak in an identical quarter. Each of the individual items in Table 4 is, in fact, the FRB Index of Industrial Production taken as a relative to that Index plus the difference between the latter and a straight-line "trend" between two peaks. The quarter-to-quarter changes have two components: the slope of that straight line and the change in the FRB Index from quarter to quarter. The former is, of course, a constant absolute factor between peaks. Secularly, the trend in the capacity index will be the same as that of the FRB Index and there can be no trend in the percentage utilization of capacity so long as the cycles in the FRB Index remain of constant relative magnitude.

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[22] See *Fortune*, Business Roundup (Jan., 1961).

[23] Lawrence R. Klein, "Some Theoretical Issues in the Measurement of Capacity," *Econometrica*, Apr., 1960.

[24] For a description and defense of the Wharton method, see "Statement of Lawrence R. Klein," *Hearings, loc. cit.*, pp. 43-66. For comparison with other measures of capacity and use of the measure in estimating full capacity private gross national product, see K. Krishnamurty, "Industrial Utilization of Capacity," *Proceedings of the Business and Economic Statistics Section*, A.S.A., 1962, pp. 309-19.

TABLE 4

WHARTON SCHOOL ECONOMETRICS UNIT ESTIMATES OF CAPACITY UTILIZATION 1946–61

YEAR	PERCENTAGE UTILIZATION OF INDUSTRIAL CAPACITY			
	1st Quarter	2nd Quarter	3rd Quarter	4th Quarter
1946	88	89	92	95
1947	97	95	94	96
1948	94	94	93	91
1949	86	82	82	81
1950	83	89	94	94
1951	94	94	90	90
1952	91	88	90	95
1953	96	96	95	90
1954	86	86	86	87
1955	91	94	95	96
1956	95	95	94	96
1957	96	95	94	89
1958	82	81	85	88
1959	90	96	92	91
1960	95	93	92	88
1961	85	90	92	93

SOURCE: "Statement of Lawrence R. Klein," *Measures of Productive Capacity*, Hearings before the Subcommittee on Economic Statistics of the Joint Economic Committee (May 14, 22, 23, and 24, 1962), page 59.

Federal Reserve Board. The Division of Research and Statistics of the Federal Reserve Board has two separate measures of capacity and the degree of its utilization. In the first—which is published in the scantest detail—monthly output and annual capacity series have been developed for "major materials" from government and trade association sources.[25] Because of the limited scope of the indexes and since it appears that the discontinuation of the American Iron and Steel Institute's capacity surveys will limit them still more, these indexes will not be discussed here.

The second of the Federal Reserve's capacity measures—which is not an "official" statistic of the Board—is based on a combination of the Index of Production, McGraw-Hill capacity data, and Department of Commerce estimates of constant dollar fixed capital stock.[26] The method is designed for use with the Index of Production. For the seven years, 1955-61, the ratio of the average of December-January values of the FRB Index of Production to corresponding values of McGraw-

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[25] The materials included are iron, steel, aluminum, copper, coke, cement, cotton yarn, synthetic fibers, pulp, paper, paperboard, petroleum, synthetic rubber, sulfuric acid, ammonia, chlorine, and benzine. See "Statement of Frank de Leeuw," *Hearings, loc. cit.*, pp. 123-27. For what may be an illustration of the trade association measures underlying this approach, see Robert S. Schultz, 3rd, "Paper Capacity and Operating Rates," *Proceedings of the Business and Economic Statistics Section*, A.S.A., 1962, pp. 330-36.

[26] The method is outlined in Frank de Leeuw, "The Demand for Capital Goods by Manufacturers," *Econometrica*, July, 1962. Excerpts from this paper appear in "Statement of Frank de Leeuw," *loc. cit.* This outline, however, proved rather difficult to follow and what appears here is more my own interpretation of what must have been done than it is a summary of the De Leeuw outline.

Hill operating rates are derived. This estimate of capacity output contains sampling error, cyclical effects, and, if the operating rates are biased downward, a possible bias towards overstating capacity at each point in time. No trend bias is apparent.[27] The measure is denoted Y_3t at time t.

The Commerce series on capital stocks is labeled Y_1t, and the following equation, converted to logarithms, was fitted by least squares:

$$(3) \quad Y_{3t}/Y_{1t} = a_1 b_1{}^t u_t.$$

Thus, the log form of a capacity output-to-capital stock ratio is described by an equation in which log a_1 is the regression constant, log b_2 is the regression coefficient and log u_t is the error term.

Similarly, the McGraw-Hill capacity output index is called Y_2t and the regression:

$$(4) \quad Y_3t/Y_2t = a_2 b_2{}^t v_2,$$

is run in the log form for the same seven years. From the two estimating equations thus derived and from data for the Commerce capital stocks and McGraw-Hill capacity studies, two estimated Y_3 values are found for every year since 1950. These are averaged to get a single figure. Estimates for 1947-50 are based on only the capital stock series.

The published results of the Federal Reserve study are shown in Table 5. Output as a per cent of capacity is apparently calculated by passing straight lines between the annual capacity points and taking a ratio of the Federal Reserve Index of Production to the points on these lines. The capacity measure grows at a little more than 4 per cent per year, or about 1½ percentage points less than does the McGraw-Hill Index. On the other hand, the capacity measure grows at about 1 percentage point per year faster than does the capital stock series. From 1953 on, there is a trend towards progressively lower rates of utilizations.

Critique of the Capacity Measures

The obvious advantage of the McGraw-Hill survey method is that direct questions relating to capacity are responded to by persons likely to know the answers. This is not an unmixed blessing, however, for there are many ways in which bias and non-random error can enter the results. Four sources of bias have been acknowledged. These include:[28] (1) possible inclusion, despite careful editing, of changes in capacity resulting from mergers and consolidations; (2) possible error

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[27] No trend bias, that is, other than a possible downward bias in the FRB Index of Production, which the capacity measure is intentionally designed to share for purposes of compatibility.

[28] "Statement of Douglas Greenwald," *Hearings, loc. cit.*, pp. 19-20.

TABLE 5

FEDERAL RESERVE CAPACITY ESTIMATES 1947–61

YEAR	CAPACITY INDEX 1953=100*	OUTPUT AS A PER CENT OF CAPACITY			
		1st Quarter	2nd Quarter	3rd Quarter	4th Quarter
1947	73	91	90	90	89
1948	78	88	88	87	85
1949	82	81	77	78	77
1950	85	80	86	92	93
1951	88	94	93	89	88
1952	93	89	88	89	95
1953	100	96	96	94	87
1954	103	83	83	82	84
1955	107	87	90	91	92
1956	112	90	89	87	89
1957	118	88	86	85	80
1958	122	74	73	77	79
1959	124	82	88	84	83
1960	128	87	86	84	80
1961	132	77	82	84	85

* These data are read from Chart 3 of the source. No published figures are available.

SOURCE: "Statement of Frank de Leeuw," *Measures of Productive Capacity,* Hearings before the Subcommittee on Economic Statistics of the Joint Economic Committee (May 14, 22, 23 and 24, 1962), page 129.

at the industry level because of recording expansions in an incorrect industrial classification; (3) possible failure to account for reductions in capacity caused by bankruptcies of firms and retirements of facilities; and (4) possible bias because results depend primarily on replies by large firms.

It is to be suspected that these are among the least important sources of error. Perhaps the most important is a potential (though unprovable) emotive bias due to the feelings businessmen have with respect to capacity. At the level of the firm—and especially in the short run—capacity and share of the market are used as frequently as profits as a measure of business performance. Not only may expansions of respondents tend to be overstated, but also nonresponse may be associated with little or no expansion. Since the McGraw-Hill capacity index cumulatively links the reported increases in capacity, the entire index may grow too rapidly because of such response bias.

The questions asked by McGraw-Hill are far from specific. "Physical output" is a complex concept for large, multiproduct firms. To provide reliable estimates, answers should be in terms of an aggregate product mix, even though the reported expansion may be for only one or a few lines or may, in an incremental sense, be a mix quite different from the total mix of the firm. In view of this difficulty—which for many large firms would amount to an involved production index problem—plus the likelihood that the response may be anticipatory of future mixes rather than related to an historical mix, it can be speculated that the reported increases in capacity weight the products whose capacity is being expanded disproportionately. Again, an upward bias would result.

One interesting aspect of the failure to define capacity is the type of reporting to be expected in the not unusual case in which the capacity of a process is increased for the purpose of reducing or eliminating purchasing or subcontracting for materials or parts. In terms of final output of the firm, capacity is not increased; yet in terms of value-added for the firm and industry, it is. The firm might report a zero increase in capacity based on final output, which is clearly wrong from an economic view of production. It might report some percentage change in the capacity of the particular process or decrease in cost of goods sold, which is again incorrect. Only in the most improbable circumstances would the change in value-added be reported, and even this is wrong when used in conjunction with percentage changes in a gross output concept.

Product mix and balance of facilities among processes may also influence replies to the rate of utilization question. Implicitly, the question seems to assume a given mix and balanced facilities, conditions which are not stipulated for the respondent. Unused facilities do not appear because of insufficient demand alone. In linear programming, for example, it can be demonstrated that except in the unique case in which production process restraints form planes all of which intersect at one point in n-product dimensional space, there will be excess capacity from the point of view of at least one process any time the number of restraints exceeds the number of products.[29] Looking at only this one process, there is excess (or slack) capacity even at the optimal product mix. Changing the mix, given prices, would not only reduce profits but would simply shift the slack from one process to another. When a respondent is asked what per cent of his facilities are being utilized, such slack may be counted as unused even though no increase in demand would reduce the slack. And in a dynamic world of product and process innovation and of cost and price changes, the achievement of continuous balance in facilities is uneconomical if not technically impossible.

Another difficulty could be extreme unbalance in facilities. It is possible that, due to changes in technology or products, the production restraint imposed by the capital related to some process is relaxed to the point that there is no product mix which would fully utilize the process capacity.[30] Some other restraint, that is, would always come into play first. Again in a programming sense, the restraint becomes fully redundant and should be excluded from the consideration of effective capacity. Yet if one were asked what per cent of some undefined capacity is utilized, there is no assurance that this capital—which is still on the books—will not be included in the estimate despite its irrelevance.

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[29] The number of products in the optimum linear programming solution is always equal to or less than the number of restraints. Since there is a restraint for every factor—not just fixed capital stocks—which limits production for any product, the condition of more restraints than products is not unusual.

The linearity and general nonsubstitutability among capital goods assumed in the programming model of the firm may be somewhat unrealistic. Even so, this approach suggests that direct surveys relating to capacity must define terms more clearly if excess capacity arising from inadequate aggregate demand is to be separated from excess capacity arising from changes in relative demand, relative prices and costs, product mix and technological change. As is true of unemployment—where if anything the factor of production is less specialized—excess capacity due to structural factors may be as important as excess capacity due to demand restrictions. If output were defined in terms of the product mix existing at the time of the survey, two measures of excess capacity are possible. One is the difference between the actual output rate of the mix and a capacity output rate of the same mix. The other is the difference between the capital stocks being utilized for actual output, and the total capital stock. When structural factors are significant, the latter would yield a larger estimate of excess capacity than would the former. But if the existing mix reflects market choice for the allocation of the economy's resources, the former seems more relevant. That is, when demand reaches the point that no more of the given mix can be economically produced with existing facilities, capacity utilization should be reported as 100 per cent regardless of the fact that some processes are not fully utilized. Capacity is determined by the most restrictive of the diverse capital stocks, not by all the stocks irrespective of their restrictiveness.

In this context, capacity does not mean simply the "maximum output under normal work schedules." Given factor and product prices, capacity is an output rate which, if sustained with balanced facilities over a period of time, would induce neither net investment nor disinvestment in real capacity in privately-managed enterprise. Whatever the "commonsense definition of capacity" used in response to McGraw-Hill's questions, it can be argued that their capacity figures reduced by the factor indicated in responses to the "preferred rate" questions is a better measure of economic capacity.[31] But this argument is not nearly as convincing as it would be if preferred rate were itself defined to exclude possible consideration by respondents of such things as the existing level of demand, notions of "fair" market shares among competitors, etc.

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[30] A "real world" illustration is the introduction of the oxygen lance on open hearth furnaces. This increases output per hour by as much as 500 per cent, eliminating, one would think, these furnaces as meaningful production restraints among the many process restraints in steel production. See *Steel—The Metalworking Weekly*, Apr. 16, 1962, p. 87.

Unlike the McGraw-Hill work, neither the National Industrial Conference Board nor the *Fortune* studies are involved with the direct estimation of capacity itself. Instead, each assumes a fixed, or slowly changing, relationship between the capital stocks of collections of firms and what would in concept be the extremely complicated horizontal and vertical aggregations of the production constraints of the same firms in the proximate domains of actual product mixes. That changes in the ratio of capital stocks to capacity output are dictated by relatively slow changes in process and product technology and surrounding institutions is not the least reasonable assumption of economics.[32] Still, it should not be forgotten that ratios of capital to output are not, even given technology, uniquely determined by the rate of operations and that what is being measured is not capacity itself.

Some of the problems involved in measuring capital stock are most intractable. Whether stock should be valued net of depreciation, as in the Conference Board studies, or gross of depreciation, as is the practice at *Fortune*, is not easily resolved. In a static situation with no changes in technology, a constant average life for capital goods, an equal initial age distribution of capital, and continuous replacement of capital as it wears out, the ratio of net to gross capital would remain constant, and the choice would be unimportant.[33] When these special conditions do not hold, relations between the two diverge and at least some difference in results will appear depending on which is used.

While capital loses market value with age, this is not necessarily because the older stock items progressively produce physical output at a slower rate. Of perhaps more importance are progressively higher maintenance costs. Viewed in this way, length of life is not a matter of capital gradually being "used up" in production but rather the point at which replacement is more economical than further maintenance. With this view, undepreciated values—as a measure of total equipment in place regardless of age—appear more relevant to physical output capability than do the net values. If the output concept is value-added and the capital is to be valued in terms of the current least cost alternative for producing that output, whether or not that alternative is in fact in use, the depreciated values seem the better. Current incremental capital-to-value-added ratios for balanced expansions multiplied by total current value-added might be a better way to obtain such an economic valuation of capital than any method based on treating historical costs, but the relevance of this to current capability to produce goods is not at all clear.

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[31] See "Statement of Lawrence R. Klein," *Hearings, loc. cit.*, p. 56.

[32] I have purposely included institutional factors to avoid a possible narrow interpretation of technology. It is easy to neglect that today's capital expenditures for expansion include restaurants, kitchens, lavatories (including ones with special keys for executives), recreation areas, protective shelters for guards and the sundry "equipment" items to furnish such "plant." Technical relations, narrowly defined, between these and production are at best obscure.

[33] Edward F. Denison, "Theoretical Aspects of Quality Change, Capital Consumption, and Net Capital Formation," *Problems of Capital Formation, loc. cit.*, p. 235.

Deflation to constant dollar terms of capital stock values—depreciated or undepreciated—poses equally difficult problems. Because capital produces an income stream into the future, the prices of capital goods reflect discounted values of this income stream as well as the cost of producing the capital items. Deflation by the usual methods adjusts to base year prices the various items of capital in subsequent years despite the fact that technological changes may have increased the discounted value (in constant dollars) of these as compared to the base year items. For this, no realistic alternative is apparent.[34]

In the Conference Board's deflation procedure, the proportion of capital acquisitions occurring during the average life span is estimated by the use of an output index for all capital goods. Differences in the final deflators for the several industries arise because of differences in average life; the capital goods output index is common to all. *Fortune*, on the other hand, estimates the timing of capital put in place through use of accounting data for the particular industry and, as between the two, its method seems preferable.[35]

More attention should be given the output series used by both the Conference Board and *Fortune*. It is not clear what concept underlies the work of the latter; gross output is the concept used by the Conference Board. The summing of sales receipts of corporations may involve progressively more double counting through time. Interplant and interfirm sales could result in an increase in the ratio of gross industry output to net industry output through time, giving the appearance of declining capital-output ratios. On this score, it is clear that the appropriate output measure is value-added, not gross output, even if the ratio does not change.

Finally, the use of the lowest capital-output ratio as an indicator of the capacity year is not infallible. While the Conference Board does rely on "independent evidence" in selecting capacity bench marks, a well-designed survey which provided reliable estimates of the rate of utilizations, say, every five years would be an excellent complement to the capital-output ratio method of measuring capacity.

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[34] But see Edward F. Denison, *op. cit.*, and the comments following by Eric Schiff and Simon Kuznets.

[35] It is true, however, that depreciation changes plus the change in depreciated assets is capital expenditures less dispositions (sales) of capital assets (not total capital expenditures as the *Fortune* description implies).

The primary advantage of the Wharton School capacity index is that its computation is quick and easy. A capacity utilization figure emerges as soon as the Federal Reserve Index of Industrial Production for the quarter is available. But if there is danger in accepting the lowest of several capital-output ratios for peaks of output as indicative of capacity, there is more danger—and less logic—in accepting every output peak for every industry as capacity. This simply is not true no matter how useful the results are in improving the fit of an econometric model of the economy. To meet this criticism, the Wharton unit has indicated that it may attempt to assess the intensity of the cyclical peaks, perhaps by applying a diffusion index factor.[36] In terms of the concept of capacity, some such modification seems essential. Indeed, since the definition of capacity employed by the Wharton unit is the minimum point on the firm's average cost curve,[37] the necessary changes should permit cyclical peaks to exceed 100 per cent of capacity as well as to fall short thereof. Another suggestion that the cost curve for "each of several industrial sectors of the economy"[38] be estimated seems doomed to failure. Even if the sectors chosen were at the four-digit industry level, there is so much heterogeneity within an industry—with vertical as well as horizontal relations among firms, various degrees of integration, and diverse product mixes—that it is difficult even to conceive of the "representative firm," let alone measure costs for the aggregated industry group. This is no criticism of the powers of modern econometric methods but rather an expression of the view that there is no unique nor even a dominant cost function to be measured in most industrial classifications.[39]

The technique of connecting peaks with straight lines and the extrapolation of these lines can also be criticized. Capital formation is not spread evenly through the cycle. For the whole economy, more as a cause than as a result of high activity, capital formation and, hence, capacity expansion clusters on the up-phase of the cycle. An alternative "straight-line" method would be to accept the most recent peak as capacity—that is, running a horizontal line from that peak—until production returns to somewhere near this level.[40]

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[36] "Statement of Lawrence R. Klein," *loc. cit.*, pp. 50-51, 58.

[37] *Ibid.*, pp. 54, 61-62, and Lawrence R. Klein, "Some Theoretical Issues in the Measurement of Capacity," *loc. cit.*, pp. 272-79.

[38] "Statement of Lawrence R. Klein," *loc. cit.*, pp. 58, 61-62.

[39] On this point, see Raymond T. Bowman and Almarin Phillips, "The Capacity Concept and Induced Investment," *Canadian J. of Econ. and Polit. Sci.*, May, 1955, and, by the same authors, "Conceptual and Statistical Problems in Estimating Capital Coefficients for Four Metal Fabricating Industries," *loc. cit.*

The regression approach of the most recent Federal Reserve capacity estimates is based on the hope that an average of two measures, both of which are known to possess error, will yield a more reliable figure than either alone. This is justifiable if it can be assumed that error in each is random, but this is precisely what is not known about either the McGraw-Hill capacity or Department of Commerce capital stock series which enter the calculations. In addition, questions do arise concerning the significance of the regression constants and coefficients obtained from seven observations and on this nothing is published. Finally, unless the correlation between time and the ratios which are the independent variables is high, least squares minimization may be a poor method for establishing "true" relationships, especially over so small a range of data.[41]

Conclusions

If the uses of capacity measures are important enough to warrant the use of the resources, the pioneering studies reviewed here should provide a basis for establishing a capacity series as part of the federal statistical program. The present studies seem indicative of trends and cycles in capacity utilization, but the reliability of each and the differences among them are adequate testimony that more precise measures are necessary if they are to be used in a fashion analogous to the use of labor force statistics. If designed with such a purpose in mind, capacity measures could prove more useful than labor force statistics in separating structural effects from demand effects as causes for underemployment of resources.

The amount of additional work necessary to provide a federal program is suprisingly small. The addition of questions relating to capacity and capacity utilization are being considered for the quinquennial *Census of Manufactures* and the *Annual Survey of Manufactures.* The well-designed sample of the latter and the full enumeration of the former eliminate one source of bias inherent in the McGraw-Hill surveys. Moreover, these would be responded to by establishments, not firms, reducing somewhat the product heterogeneity problem for respondents. Further, it is to be expected that specific definitions would be supplied, and these definitions could distinguish the difference between existing output and capacity output for the given mix from differences between facilities utilized and total facilities. Both measures are needed if structural effects are to be isolated.

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[40] This is not without precedent. Peak historic output was used by the Harvard Economic Research Project as capacity for many of the input-output industries in the dynamic version of the Leontief model.

[41] I have been informed by Frank de Leeuw that the correlation coefficients for (3) and (4), above, run on the reciprocals of the dependent variables, are —.88 and +.79, respectively.

The Department of Commerce has published industry net output data in constant prices.[42] These are currently given only for the durable and nondurable manufacturing categories, but presumably industry detail is calculated but unpublished. Here are net output series which, perhaps with modification and improvement, could be used with the same Department's estimates of business capital stocks to provide capital-net output ratios.

The Census, in addition, may record constant dollar peak historic outputs for shipments of each of 1,100 product classes reported in the *Annual Survey of Manufactures*.[43] For each year's *Survey*, an "attained activity" figure—the per cent that that year's shipments are to past peak shipments—would be published for product classes and SIC groups.

From all of this, it is clear that a federal program on capacity is not impossible. That its uses justify the effort has not been considered but may be judged in part by the paper which follows.

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[42] "GNP by Major Industries," *Survey of Current Business*, Oct., 1962. For further discussion of methods, see selections in *Output, Input and Productivity Measurement*, Studies in Income and Wealth, Vol. 25 (Princeton, 1961).

[43] "Statement of Raymond T. Bowman," *Hearings, loc. cit.*, pp. 140-41. The other proposed federal programs are also discussed in this source.

USES OF CAPACITY MEASURES FOR SHORT-RUN ECONOMIC ANALYSIS

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This paper is in the nature of a very preliminary effort to pull together a number of diverse pieces of work—some by the author, some by other people—all of which, directly or indirectly, deal with the influence of capacity utilization rates on the pattern of short-run economic behavior. The analysis is far from complete, in the sense that it does not constitute a single unified model. At the risk of censure from econometric purists, however, I believe that some important implications about over-all economic behavior can be drawn from these individual investigations. In obeisance to the canons of scholarly caution, let us label our speculations "tentative" and move blithely ahead.

There are three major economic variables which are directly affected by changes in the rate of capacity utilization: investment, prices, and productivity. In combination, the movements of prices and productivity affect the share of income going to profits; hence the share going to disposable personal income, and hence the size of the "multiplier." We can rephrase the first sentence, therefore, and say that capacity utilization rates are involved in the determination of investment, consumption, and the price level, albeit—as we shall have occasion to note—in an exceedingly complicated way.

Investment

There is little that we can say here that is not by now familiar—although the familiarity is of quite recent origin. I have nothing new to contribute, except to point out a few of the implications of some recent work in this area. The simple accelerator theory, in which changes in investment are a linear function of changes in sales has always been recognized as weak, because of its failure to take excess capacity into account. Letting Y equal output, K capital stock, and I investment (where $\triangle K = I$), the simple

$$I = a(Y_t - Y_{t-1}) \tag{1}$$

must, at a minimum, be replaced by

$$I = aY_t - K_t. \tag{2}$$

But, of course, this begs the question. Until we specify why the first equation does not hold, we have no warrant for writing the second, which, in any event, collapses into the first without some further restrictions. We can, à la Hicks [1], take account of the limit to the rate of disinvestment set by a zero gross investment rate, and hence provide for excess capacity at certain phases of the recession and recovery. But this is hardly an acceptable solution. Attempts to verify statistically the simple accelerator, even with an excess capacity variable, have consistently proven fruitless. Moreover, a number of theoretical considerations point to the necessity for a much more complicated version of the accelerator. All of these point to the existence of a fairly long and complicated lag between changes in sales or output and the induced change in capacity.

1. Investment is an input which involves the incurring of long-run costs. It is one input which is not likely to be adjusted to changes in demand which are believed to be temporary in nature. However valid the concept of "permanent income" for the analysis of consumption behavior, the concept of "permanent sales" is a necessary component of investment analysis. In turn, it is probably not unreasonable to use a Friedman-type measure of permanent or expected sales; namely, a weighted average of past sales, with more recent experience given greater weight than the more distant past. This, in turn, implies a lagged response of investment to changes in actual sales, and allows for a build-up of excess demand or excess capacity, depending on the strength and persistence of the changes in sales.

2. During periods of rapid increases in "permanent sales," many business firms will find their investment response limited by the absorptive capacity of their management. There is a limit to how fast a given firm can expand without stretching its management so thin as to jeopardize efficient operations.

During boom periods this limit adds still another lag in the response of investment to changes in the capacity utilization rate. (Erik Lundberg, in his examination of investment determinants in Sweden, found this to be a major factor in setting the pace of capacity expansion[2]).

3. The two lags noted above relate to the time period between a change in actual sales relative to capacity and an investment decision. But once that decision is taken, there is, of course, a further set of lags. From an initial decision to the completion of plans, the appropriation of funds, the placing of orders, and the final installation of plant or equipment, there stretches a series of lags, whose length depends upon the nature of the investment being undertaken.

For all of these reasons, the formulation of an accelerator model requires the incorporation of a fairly lengthy lag structure. But taking lags into account pays handsome dividends. Whereas all prior statistical tests of the accelator had proved disappointing, Koyck [3], Eisner [4] [5] [6], De Leeuw [7], and others have, in recent years, found good relationships between investment and capacity utilization (or similar) variables, using rather lengthy lag structures.

The fact that a lagged accelerator can be justified on theoretical grounds and can stand the test of empirical verification does not, of course, imply that accelerator-type variables are the only ones which are relevant in an analysis of investment. I have much sympathy, on theoretical grounds, with the role assigned to profits in Duesenberry's discussion of investment [8]. Changes in technology, in product mix, in tax rates, and, we may even be so daring as to suggest, in interest rates, must also be taken into account. But the major point I wish to make in this paper is that the influence of period-to-period capacity utilization rates on investment is substantially damped by a complicated lag structure.

There are important consequences of accepting distributed lags in the investment function. The simple unlagged accelerator, assigned realistic values of the capital-output ratio, and combined with a multiplier, yields an explosive model. Ceilings and floors are necessary to contain the path given by such a model within reasonable bounds. Within those bounds the model depicts a violently erratic economy. There is no need for me to dwell on the unreality of this picture. The distributed lag accelerator, however, when combined with a reasonable multiplier, gets the model down into the range of damped cycles. In more commonplace terms, it provides, as Duesenberry points out, for investment backlogs. When sales rise relative to existing capacity, only part of the rise is immediately considered "permanent."

When actual sales are rising, permanent sales are lower than actual, and investment decisions are attuned to the lower, permanent sales measure. Further, the other lags reduce the response of actual investment outlays to changes in investment decisions. Hence, during a period of expansion, actual capacity lags behind optimum capacity—a backlog of profitable investment projects builds up to "cushion" the economy against shocks. The phenomenon of continued net positive investment during recessions is inexplicable on simple accelerator grounds, but easily understandable in a long-lagged accelerator model. Too large a shock, however, can reduce sales so far as to eliminate the backlog, remove the cushion, and result in the reduction of net investment to or near zero.

Prices

The rate of capacity utilization is a good proxy measure of the strength of demand relative to long-run supply. If we define capacity to be the output attainable at the minimum point on the cost curve and if we waive the problem of defining a supply curve in less than purely competitive markets, a capacity utilization variable should capture the influence of changes in demand on prices. However, to isolate the impact on prices of changes in demand from that of changes in costs is no mean task, since, as we shall note at a later point, changes in costs, including variable costs, are not independent of changes in the rate of capacity utilization. The following section is a brief report of an attempt to isolate statistically the various factors affecting prices in a number of major industries. Only partial results have been completed so far, and hence this must be considered a progress report rather than a finished product. The research was undertaken as part of a larger project, sponsored by the Social Science Research Council, to construct an econometric model of the U.S. economy.

The specification of the price model is as follows:

1. Prices are assumed to be set by a markup on standard costs; i.e., costs at "normal" levels of operations. To revert to different language, "permanent costs are the relevant ones."

2. These *"ex ante"* markups are modified according to market conditions. More specifically, when the rate of capacity utilization rises, the markup increases, and vice versa. However, the hypothesis is made that a rise in capacity utilization rate has a larger impact on markups than a decline; in other words, prices are more rigid against declining demand than against rising demand.

3. Temporary changes in costs—i.e., deviations of actual costs from standard costs—are also assumed to affect prices, but less than permanent changes.

Quarterly time series for the relevant variables were constructed as follows:

Output. Constant dollar value-added was estimated by multiplying an output index by a base period figure for gross national income originating in the relevant industry. (See [9] for a discussion of the concepts involved.)

Capacity. For durable and nondurable manufacturing the technique used by De Leeuw [7] was followed. This combines McGraw-Hill capacity utilization estimates and capital stock data. For other industries, a capacity series was constructed by calculating a time trend of capital-output ratios in the nonrecession years of the period 1947-56, extrapolating the trend to 1960, and multiplying the trend value of the capital-output ratio by output to get a capacity series. Essentially, this technique uses the capital stock as a measure of capacity, modified by the trend in capacity output per unit of capital stock.

Normal Unit Labor Costs. Compensation per employee man-hour (including both salary and wage employees) was divided by normal productivity. In turn, normal productivity is a twelve-quarter moving average of actual productivity, adjusted upward to reflect the trend in productivity. Let Q_a = actual productivity, b = quarterly trend in productivity in the relevant industry in the postwar period; then normal productivity (Q_n) is:

$$Q_n = 1/12\left[\sum_{a=t}^{t-12} Q_a\right](1+b)^{5.5}$$

Normal Depreciation. Depreciation charges per unit of normal capacity output. Computed by calculating depreciation per unit of capacity output (which, remember, is not physical capacity) in 1947, 1951 and 1960, and straight-lining between these years. This was done after some experimenting showed that this would avoid most of the problems caused by accelerated amortization certificates granted during the Korean war.

Price. Appropriately weighted BLS prices were used for most industries. In a few industries (trade, nonrail transportation, and services) the implicit deflator for constant dollar value-added was used. Raw material prices were constructed by weighting, with input-output weights, prices of the more important materials consumed by the industry. In the case of retail trade, however, the "price" of output is really the gross margin; manufacturing prices, therefore, are not a cost in the construction of retail prices.

The following symbols are used for the relevant variables:

P_p = product prices

P_m = materials prices

C = compensation per employee man-hour

Q = output per man-hour; Q_n = normal and Q_a = actual productivity

D_n = normal depreciation

$\dfrac{C}{Q_n} + D_n$ = normal unit costs

$\dfrac{C}{Q_n} - \dfrac{C}{Q_a}$ = deviation of actual from normal unit labor costs

X = output

K = capacity

$\dfrac{X}{K}(+)$ = *positive* deviation of actual from normal capacity utilization rates (normal being the nonrecession postwar average)

$\frac{X}{K}(-) =$ *negative* deviations of actual from normal capacity utilization rates

The following equation was fit to quarterly data for a number of industry groups and individual one- or two-digit industries, for the period 1948 through 1960:

$$P_p = A_0 + A_1\left[\frac{C}{Q_n} + D_n\right] + A_2\left[\frac{C}{Q_a} - \frac{C}{Q_n}\right] + A_3P_m + A_4\frac{X}{K}(+) + A_5\frac{X}{K}(-)$$

If our hypotheses are correct, we should find that $A_1 > A_2$ (by an amount which exceeds an allowance for the fact that the first term includes depreciation and the second does not) and that $A_4 > A_5$. Table 1 gives the results of fitting the equations for selected industries, and in general our hypotheses are confirmed.[1]

In those industries where the capacity variables had the "right" sign, the coefficient for positive deviations in capacity utilization rates is larger than the coefficient for negative deviations. In other words, the downward influence on *ex ante* markups of a fall in demand relative to capacity is less than the upward influence of a rise in demand relative to capacity. Given costs, prices are more rigid downward than upwards.

In a number of cases—retail trade being the outstanding example—the coefficient of the capacity utilization variable had the "wrong" sign. In other words, a rise in the rate of capacity utilization was associated with a fall in prices, costs given. In other cases the capacity variable was insignificant, durable goods manufacturing being the chief example. For such industries the equations are shown fitted to the remaining variables. It should be remembered that in many of the nonmanufacturing industries the concept of capacity is extremely illusory. Deviations of output from capacity are probably better thought of as deviations from a trend, with no overtones of "optimum" costs. With this caveat in mind, a study of Table 2 will indicate why the capacity variables had the wrong sign or insignificant coefficients in some industries. In retail trade, wholesale trade, and nonrail transportation, changes in actual relative to normal unit labor costs are negatively associated with deviations in capacity utilization above or below normal. High rates of capacity utilization are associated with subnormal labor costs and vice versa, because in the short run, rates

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[1] The time series have been worked up for 21 two-digit manufacturing industries, but only the regressions shown in the table have so far been computed.

TABLE 1

REGRESSION RESULTS; PRICE EQUATIONS

	A_0	A_1	A_2	A_3	A_4	A_5	R^2
Aggregates							
Durable manufacturing	−20.27	1.82 (.55)	.217 (.18)	.112 (.03)			.965
Nondurable manufacturing	−9.43	1.38 (.06)	.37 (.27)	.26 (.02)	.59 (.09)	.15 (.06)	.931
Private nonfarm nonmanufacturing	−.140	2.02 (.05)	.72 (.21)				.990
Individual Industries							
Primary metals	−5.95	1.04 (.07)	.32 (.13)	.30 (.17)	.35 (.18)	.21 (.07)	.961
Nonelectrical machinery	−10.33	.67 (.09)	*	.62 (.07)	.103 (.07)	*	.992
Retail trade	46.68	1.02 (.06)	1.85 (.16)				.910
Wholesale trade	20.39	1.20 (.03)	.73 (.13)				.963
Nonrail transportation	34.62	1.04 (.05)	1.07 (.12)				.910
Services	12.64	1.98 (.02)	1.98 (.22)				.995

* Coefficient practically zero.

NOTE: Price data in index number units, 1954=100. Capacity deviation data in percentage points. Unit labor cost data in index number units, 1954=(100)×(the 1954 share of labor costs in value-added).

of productivity change are positively associated with capacity utilization rates. Column 3 of Table 2 shows that when the deviation of actual from normal costs is omitted from the variables in the above mentioned industries the coefficient of the capacity utilization variable is negative—it is carrying the cost variable. When the cost deviation variable is added, the "wrong signed" coefficient of the capacity utilization variable either becomes insignificant or is substantially reduced in value. In the case of the durable goods industries the capacity variable is insignificant whether or not the cost deviation variable is included. In part this may arise from the fact that, in the case of durable goods, raw material prices are intercorrelated with the rate of capacity utilization. In 1950 and 1951, for example, when the capacity utilization rate rose very sharply, raw materials prices rose even more steeply. Clearly, raw material prices ought also to be subjected to the "normalization" procedure; finished product prices do not fluctuate under the influence of short-run variations in raw materials prices, but do, of course, reflect longer term trends in such raw material costs. Normal raw material price indexes will be constructed for the next stage of the project.

TABLE 2

INTERRELATIONSHIP OF CAPACITY UTILIZATION RATES AND "COST DEVIATIONS," SELECTED INDUSTRIES

		Simple Correlation $\frac{X}{K}$ on $\frac{C}{Q_a}-\frac{C}{Q_n}$	Coefficient of $\frac{X}{K}$		Coefficient of $\frac{C}{Q_a}-\frac{C}{Q_n}$	
			With $\frac{C}{Q_a}-\frac{C}{Q_n}$ in Equation	Without $\frac{C}{Q_a}-\frac{C}{Q_n}$ in Equation	With $\frac{X}{K}$ in Equation	Without $\frac{X}{K}$ in Equation
Durable manufacturing	$\frac{X}{K}(+)$	$-.327$	$-.159$	$-.179$	.273	.217
	$\frac{X}{K}(-)$	$-.591$	.084	.017		
Nondurable manufacturing	$\frac{X}{K}(+)$	$-.177$	.426	.442	.131	$-.776$
	$\frac{X}{K}(-)$	$-.151$	.072	.060		
Primary metals	$\frac{X}{K}(+)$		.346	.397	.318	n.a.
	$\frac{X}{K}(-)$		.214	.080		
Nonelectric machinery	$\frac{X}{K}(+)$	$-.283$	.074	.071	$-.174$	n.a.
	$\frac{X}{K}(-)$	$-.430$	$-.111$	$-.075$		
Retail trade	$\frac{X}{K}(+)$	$-.629$	$-.002$	$-.635$	1.46	1.85
	$\frac{X}{K}(-)$	$-.672$	$-.320$	$-.839$		
Wholesale trade	$\frac{X}{K}(+)$	$-.613$	.197	$-.379$	.786	.734
	$\frac{X}{K}(-)$	$-.468$	$-.049$	$-.270$		
Nonrail transportation	$\frac{X}{K}(+)$	$-.537$	.092	$-.384$	1.103	1.066
	$\frac{X}{K}(-)$	$-.478$	$-.030$	$-.272$		

The hypothesis with respect to the importance of normal unit labor costs is also confirmed for a number of industries, especially the manufacturing industries. If prices were marked up on actual costs, the coefficient of the normal cost and the cost deviation variables ought to be the same (after a minor adjustment for the inclusion of depreciation in the normal cost variable). However, in durable and nondurable manufacturing, in the private nonfarm nonmanufacturing sector as a whole, and in the individual primary metal, nonelectrical machinery, and wholesale trade industries the cost deviation coefficient was substantially lower than the normal cost coefficient. In nonrail transportation and in services the two coefficients were the same—and in both of these industries with strong secular growth trends deviations of actual from normal costs have been extremely small. In retail trade, puzzlingly, the cost deviation was substantially larger than the normal cost coefficient. Why this should be so is unclear. One possible reason is the fact that cost deviations are negatively associated with changes in the capacity utilization rate. The intercorrelation between high demand and low costs may be obscuring the real impact of both variables. This is partly confirmed by the fact that when the retail trade equation is fitted with both the capacity utilization and the cost deviation variable, the coefficient of the latter drops from 1.85 to 1.46 (see Table 1).

Completion of the computations for the remaining manufacturing industries may give different results. But so far the import of the price equation is fourfold: (1) prices are marked up on normal costs; (2) temporary cost changes also affect prices, but in most industries by far less than permanent cost changes; (3) in many, but not all, industries markups over standard costs are modified by demand considerations, with increases in demand above normal capacity having a more significant effect than decreases below capacity; and (4) in the durable goods industries, the highly volatile savings in raw materials prices ought to be "smoothed" since the distinction between temporary and permanent changes in costs probably applies to materials costs as well as to labor costs. Such smoothing would also reduce the intercorrelation of materials prices with the capacity utilization variable and, perhaps, allow a better evaluation of that variable's influence on price markups.

Productivity

In this area, I shall chiefly be reporting the work of others. The studies of Edwin Kuh [10] and Arthur Okun [11] clearly point to a positive relationship between output per man-hour and the rate of capacity utilization. Kuh's work relates changes in output per man-hour to the various phases of the business cycle, not to the change in capacity utilization rates per se. The CEA studies, however, directly relate productivity to the gap between actual and potential, or capacity, output. While substantially differing in detail, both the Kuh and the CEA studies indicate that the rate of productivity growth falls below its longer term trend during periods of recession.

If we define capacity to be the output obtained at the optimum point on the cost curve, then when output is rising *pari passu* with capacity, productivity will rise at a rate determined essentially by long-run forces—the pace of innovations, the rate of change in the capital-labor ratio, changes in the education and skills of the labor force, etc. In the short run, however, as the rate of capacity utilization changes, productivity is affected by a number of other factors.

We noted earlier that the expansion of capital inputs is more clearly related to permanent or expected changes in sales than to short-run or temporary variations. The same considerations apply to labor inputs. In the first place, a very large, and rapidly growing, proportion of employment is overhead employment. In manufacturing, for example, the proportion of total employees classified as "nonproduction" workers has risen from 16 per cent in 1947 to 26 per cent in 1962. Secularly the shift of the employment mix from production line labor to overhead labor is a part of the general advance in technology and is to some extent an accompaniment of the secular rise in capital stock per worker. The contribution of manager, research worker, professional and sales personnel, and clerical help to the production process is not directly tied to the scale of output in the very short run. Such inputs are quasi-fixed. Variations in output, expected to be temporary in nature, are not accompanied by proportional variations in the overhead work force. Indeed, if capacity is rising, while output is falling, the employment of such personnel may continue to rise, so long as the output decline is expected to be very short lived.

Not only overhead personnel but even direct production labor may fail to respond proportionately to short-run changes in output. This is implied in Kuh's regression equations and directly observable from a comparison of output and production worker employment during periods of contraction and early recovery. The reason, of course, is that the hiring and training of new workers is an expensive process. During a period of secular prosperity, there is a direct incentive to retain a core of skilled workers, even during recession periods. Again the concept of permanent sales or output is relevant. We might best think of all inputs as being in a spectrum between those which are highly fixed in nature and those which can costlessly be varied. As capacity and output rise together, this mix of inputs is adjusted in line with technological developments, relative input prices, etc. But when output falls relative to capacity, the various inputs are adjusted in different proportions depending upon expectations as to the future depth and duration of the output decline. Some part of the labor force—chiefly the unskilled and low seniority workers—can, almost costlessly, be hired or laid off. The remainder of the labor force responds in different degrees to a variation in output relative to capacity. The longer and deeper the decline in output relative to capacity, the more the balance of risks and costs dictates a downward adjustment of labor inputs. In a very short recession, for example, overhead labor may not be laid off at all, because it is tied fairly closely to long-term output expectations. Highly skilled groups of production line labor may also escape the full impact of the output reduction. In a longer lasting recession, however, the status of long-term expectations begins to change, so that even those personnel whose employment is tied to longer term output expectations begin to be laid off.

On the upswing, of course, as output rises toward capacity, productivity rises rapidly relative to labor inputs, reflecting the failure to lay off certain groups of employees during the prior recession. Once output has reached capacity and begins to rise more slowly, in line with capacity growth, the rate of increase in productivity is reduced. During this stage of the cycle, productivity growth may be lower than the long-term trend. The appearance of bottlenecks and the time lag in adjusting production lines to large doses of new capital inputs both probably contribute to this result.

In summary, labor productivity is sensitive to fluctuations in the capacity utilization rate. This relationship is a complex one, however. Labor inputs, of different classes, are adjusted to expected output. But the expectations to which inputs are adjusted cover different time spans, ranging from the rather long look ahead which is relevant for major capital investment projects, through an intermediate time span for overhead labor to very short-run expectations for unskilled labor. As a result, any quantitative relationship between productivity and capacity-utilization rates must take into account: (1) the ratio between various categories of employees—at a minimum the ratio between production and nonproduction labor; and (2) a measure of the expected capacity utilization rate, perhaps using a Friedman-type weighted average. Insofar as man-hours of employment are related to such a measure of expected output relative to capacity, the rate of change in the capacity utilization measure plays an important role. For when output is changing rapidly, expected and actual output are likely to diverge significantly, and the statistical measure of actual output per man-hour is apt to be significantly affected.

Profits

The cyclical variation in gross profits (profits plus depreciation) as a share in total income depends upon what happens to the *ex ante* markup on standard costs and how actual costs deviate from standard costs. The price equations, discussed earlier in this paper, indicate that when actual costs deviate from standard costs, prices are not marked up sufficiently to cover the cost increase (this can be seen by a comparison of the coefficients of standard costs with the coefficients of cost deviations). In turn, productivity tends to vary directly, and hence labor costs inversely, with the rate of capacity utilization (albeit in a complicated manner). Thus gross profit margins vary directly with the rate of capacity utilization. This relationship is further strengthened, in a number of industries, by the impact of capacity utilization rates on *ex ante* markups. The markups applied to standard costs and the deviation of actual from standard costs tend to rise as capacity utilization increases above normal and fall as utilization rates decline. Both from the cost and the demand side, therefore, changes in capacity utilization tend to affect gross profit margins.[3]

In a forthcoming paper, prepared for the Twenty-Fifth Conference on Income and Wealth of the National Bureau of Economic Research, the author has fitted gross corporate profit margins to ratio of output to capacity in the corporate sector of the economy for the 1922-41 and the postwar period. The specific equation fitted was:

$$\frac{\pi}{Y_c} = A_0 + A_1 \frac{Y_c - K}{Y_c} + A_2(t), \tag{3}$$

where:

π = gross corporate profits
Y_c = corporate gross product
K = capacity in the corporate sector

(all variables are in constant prices).

The following table summarizes the results of the regression equations for various periods.

The A_0 coefficient represents the share of profits in total corporate income originating when output is at capacity; A_1 carries the impact

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[3] Kuh (*op. cit.*) has shown that the cost influence is much greater than the "demand" influence; i.e., most of the cyclical variations in gross profit margins stem from changes in productivity rather than from changes in the price-wage ratio.

TABLE 3

Period	A_0	A_1	A_2	R^2
1922–41 (annual data)	.279	.195 (.001)	.151 (.052)	.97
1949–59 (quarterly data)				
1. Whole period	.289	.187 (.039)	−.030 (.013)	.55
2. 1948–50	.289	.203 (.037)	.191 (.059)	.88
3. 1953–55	.269	.237 (.044)	.213 (.058)	.83
4. 1957–59	.272	.212 (.033)	.286 (.057)	.87
5. Recessions and recoveries taken together	.287	.200 (.042)	−.021 (.015)	.58

NOTE: Each individual cycle covers only the period of the recession and the recovery back to capacity; the period which occurs between the attainment of capacity operations and the next peak is omitted.

of changes in the capacity utilization rate. The A_2 coefficient in the equations fitted to the 1922-41 and the whole postwar period indicates the long-run changes in the gross profit share which was occurring. In each individual recession and recovery, the large coefficient of the time variable reflects the fact that at the end of the recovery period proper, the profit share was higher than at the prior peak. A more careful specification of the equation, incorporating the rate of change in the capacity utilization ratio, would probably have captured the structural relationship (which is due to the influence of the rate of change in capacity utilization on productivity, as suggested in the last section).

The relationships shown in Table 3 can be converted to marginal relationships. From equation (3) we can develop the marginal ratio of the change in gross profits to the deviation of output from capacity; per one unit deviation in output from capacity the unit change in profits will be:

$$\frac{\Delta\pi}{Y_c - K} = A_0 + A_1, \tag{4}$$

and the elasticity (n) of this relationship will be:

$$n = 1 + \frac{A_1}{A_0}\cdot \tag{5}$$

Table 4 gives the relationships for the various periods covered by the regressions.

TABLE 4

Period	Marginal Ratio, $\frac{\pi}{Y_o - K}$	Elasticity	Normal Profit Ratio, A_0
1922–41	.474	1.70	.279
1948–59			
1. Whole period	.476	1.65	.289
2. 1948–50	.492	1.70	.289
3. 1953–55	.506	1.88	.269
4. 1957–59	.484	1.78	.272
5. Recessions and recoveries taken together	.487	1.70	.287

Table 4 implies, of course, that the proportion of any income decline which is borne by gross profits is much larger than the average ratio of profits to total income. Further, some 50 per cent of the net profit decline is absorbed by corporate taxes. Finally, we know from studies of Lintner [12] and others that the response of dividends to short-lived fluctuations in corporate profits is quite small. Hence a disproportionate share of any decline in total income originating in corporations is borne by profits, and most of that decline is absorbed by taxes and gross business savings. Very little is absorbed by dividends. Personal income recipients, therefore, bear a disproportionately small and corporate savings and taxes a disproportionately large share of cyclical changes in income originating in corporations.

We can go one further step. During moderate cyclical changes a very large share of total output changes occur among commodity producing industries, where the corporate form of activity is most prevalent, and a very small share occurs in the distribution and service industries where the corporation form is less prevalent. In major depressions this characteristic is less pronounced. Hence, combining the points made in this and the immediately preceding paragraphs, we can assert that during small recessions a disproportionately large share of cyclical changes in total gross national income is borne by changes in corporate taxes and gross corporate savings and a correspondingly small share is absorbed by personal income (exclusive of transfer payments). In larger recessions this characteristic still holds, but to a lesser degree.[3]

Cyclical variations in the capacity utilization rate lead to changes in the share of income going to gross profits, which result in the absorption of large parts of any cyclical income change by corporate profits taxes and gross corporate saving. As a consequence, the marginal ratio of changes in personal income to changes in GNP is less than the average ratio of personal income to GNP. In turn, this implies a marginal ratio of changes in consumption to GNP much lower than the average ratio, and hence a sharply reduced short-run multiplier.

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[3] The author's forthcoming paper in Vol. 25 of the N.B.E.R. Studies in Income and Wealth, cited earlier, documents the statements in this paragraph more fully.

Some Conclusions

Our brief examination of the way in which changes in the capacity utilization rate affect economic activity suggests the following tentative conclusions:

First, changes in capacity-utilization rates affect the level of investment through a complicated system of distributed lags. These lags arise chiefly from expectational phenomena and from the time sequence which intervenes between initial decisions and the installation of new plant and equipment. During periods of prosperity, backlogs of profitable investment opportunities are built up which cushion the economy against the kind of violent investment response to output changes depicted in the simple accelerator model. However, the deeper and longer lasting is the gap between capacity and output the more likely is it that the backlog will be eroded and a more violent cumulative movement set in.

Second, through influences on prices and productivity, cyclical changes in capacity-utilization rates affect the share of income going to profits. Given our tax structure and the behavior of dividends, this shift in income shares implies that for changes in output and income relative to capacity the multiplier is relatively small.

The effect of changes in the capacity-utilization ratio on productivity, and hence on profits, is a complicated one. In particular, the longer subnormal capacity-utilization rates continue, the more likely is it that employment will be reduced proportionately with the decline in output. The share of any income decline absorbed by wages and salaries would thus tend to grow larger as the length and depth of the recession increases. Moreover, if the fall in profits is more than temporary in nature, dividends will begin to absorb some of the decline. In other words, the relative impact of a decline in output on personal income and consumption is fairly well damped down for short and relatively moderate recessions. But this damping apparatus may well begin to weaken as the recession grows in length and severity.

As a response to moderate shocks of brief duration, the multiplier-accelerator process appears to be substantially attenuated. But, on a very tentative basis, there is reason to believe that the relative endogenous response to larger and longer lasting shocks may be significantly greater.

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set of rules upon which the taxpayer may rely in determining the useful lives of his facilities.

1. *The New Guideline Lives.* The first innovation afforded by the Revenue Procedure is to assign suggested service lives for depreciable facilities on the basis of the broad industry groupings in which such assets are used rather than on the basis of a detailed identification of the assets themselves. The new guidelines set out four major groups of depreciable facilities, within each of which a number of subgroups are delineated. All together, about 100 separate groups of facilities are listed, compared with the 5,000 or so in Bulletin F.

Group One assets are those which are used by business in general and include office furniture, fixtures, machines and equipment, transportation equipment, land improvements, and buildings—except for special-purpose structures. Group Two assets are those used in certain nonmanufacturing activities; i.e., agriculture, contract construction, fishing, logging and sawmilling, mining, recreation and amusement, services, and wholesale and retail trade. Group Three assets are those used in manufacturing. Thirty major manufacturing subgroups are explicitly designated, in several of which further industry subgroupings are shown. Group Four assets are those used in transportation, communications, and public utilities, grouped into twelve major subgroups in some of which further subgrouping is afforded.

With respect to each group or subgroup, a specific guideline life is given. This guideline life applies to all of the facilities of a taxpayer in that industry group, except for those included in Group One; i.e., assets used by business in general. For example, a tobacco manufacturing company might use the fifteen-year service life suggested in the new guidelines for all of its depreciable assets except its office furnishings, machines and equipment, transportation equipment, land improvements, and buildings. It is anticipated that most business taxpayers will need to use only three or four of the 100 or so guideline groups in maintaining their depreciation accounts.

The new guideline lives are applicable to existing facilities as well as to those acquired after issuance of the Revenue Procedure. In manufacturing, the new lives are estimated to be 32 per cent shorter, on the average, than those provided in Bulletin F, and about 15 per cent lower than those which were being used in actual practice.[3]

Any taxpayer is allowed, as a matter of right, to use the new guideline lives for the first three years following the issuance of the Revenue Procedure. He may continue to use the same lives thereafter if they conform with his replacement practices, as measured by a so-called "reserve ratio" test (see below), or if the difference between his replacement cycle and the guideline life is narrowing. The taxpayer may use a shorter life than that set forth in the guidelines if such a shorter life has already been established or is justified in the future as reflecting the taxpayer's replacement practice.

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[3] See U.S. Treasury Department, *Depreciation Guidelines and Rules, op. cit.*, pp. 11-20, for a listing of guideline classes and lives. See Table 1 in the appendix to this paper for a comparison of the new guideline lives with Bulletin F lives and those estimated to represent actual practice in manufacturing industry.

2. *The Reserve Ratio Test.* The second innovation provided in the Revenue Procedure is the set of rules called the reserve ratio test. The objective in introducing these rules is to afford the taxpayer clearly defined guides upon which to rely in justifying the useful lives he assigns to assets for purposes of computing depreciation allowances, whether or not these lives conform with the guidelines. If the taxpayer observes the rules set forth in the Procedure, he is able, presumably, to change assigned service lives quite substantially without having the new lives subject to challenge by the IRS.

Summarily stated, the reserve ratio test gears useful life of a taxpayer's depreciable facilities to his replacement practices. The test measures the ratio of the accumulated depreciation reserves to the gross amount of depreciable facilities in a depreciation account (which is homogeneous with respect to the service life assigned to it by the taxpayer). Given the service life, the depreciation method used (i.e., straight-line, declining balance, etc.), and the rate at which the gross stock has grown, the value of this reserve ratio may be used as an index of the average age of the assets in the account. A reserve ratio reflecting the assumption that the taxpayer's retirement and replacement practices are in line with the service life assigned to the account may be calculated for any year since the beginning of the account.[3] If the number of years during which assets have been added to the account is equal to or greater than the service life assigned to the account and if the taxpayer's retirement and replacement practices are in fact in line with this service life,[4] then the actual reserve ratio should correspond closely to the calculated ratio. If the taxpayer has been retaining the assets in use over a longer period, on the average, than the assigned service life, the actual reserve ratio—hence the actual average age of the assets in the account—should exceed the computed ratio. On the other hand, if the taxpayer has been retiring and replacing assets more rapidly than the assigned service life calls for, his actual reserve ratio should fall short of the calculated ratio.

Tables setting forth the value of reserve ratios, computed on the assumption that the replacement cycle is the same as the guideline life,

[3] See the attached appendix for the formula for computing the ratio and its derivation.

[4] That is, if the mean length of time during which assets acquired in any given year are retained in the asset account (i.e., in use in the taxpayer's business) before being replaced is the same as the assigned service life.

are provided in the Revenue Procedure; a separate table is provided for each depreciation method; each table is a matrix of reserve ratios for each service—or "test"—life and growth rate. In order to afford the taxpayer some leeway, however, a range of ratios is shown for each cell in the table. The lower value of the range is computed as if the assets in the account were used for a period 10 per cent shorter than the service life assigned by the taxpayer; the upper value is computed as if the assets were used for a period 20 per cent longer. Accordingly, a substantial amount of deviation is possible before the reserve ratio test will be taken as indicative of the taxpayer's assignment of an erroneous service life to a depreciable asset account.

Even if the taxpayer's reserve ratio exceeds the upper end of the range, he is not automatically required to use a longer life in computing depreciation. In the first place, if the assigned life has been justified on other grounds, the taxpayer may continue to use it, regardless of the reserve ratio test. Second, if no such alternative justification has been or can be established, the Revenue Procedure provides that the reserve ratio test will be considered to be met for the first three taxable years to which the Procedure applies. Finally, the taxpayer is given a period of years equal to the guideline life to bring his reserve ratio within the upper limit of the appropriate range. So long as the reserve ratio is moving toward this upper limit, the reserve ratio test will be considered to be met.

A taxpayer may assign to an asset account a service life that is shorter than the guideline life for that class of assets and shorter than the service life he has been using, subject to certain tests. Meeting these tests is taken as evidence that the taxpayer's prior retirement and replacement practices justify assigning the shorter service life he wishes.[5] In addition, he may rely on other facts and circumstances to justify the shorter life, even if the reserve ratio test is unsuccessful.

Appraisal of the New Depreciation Provisions

Appraisal of the impact of these provisions so soon after their issuance is necessarily highly speculative. I hope the following discussion will serve to delineate some of the questions to which longer range empirical inquiry might well be directed.

1. *The New Provisions and the Measurement of Depreciation.* The Treasury Department has emphasized that, whatever other purposes they may serve, the new depreciation rules are aimed primarily at pro-

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[5] The tests are (1) the reserve ratio for the account in the immediately preceding taxable year was below the lower limit of the appropriate reserve ratio range; (2) the same service life as that used in the immediately preceding taxable year has been used for a period of years equal to at least one-half this service life; and (3) the shorter class life is not less than that shown in an Adjustment Table provided in the Revenue Procedure.

viding more realistic (more accurate) determination of annual depreciation charges.

The Revenue Procedure involves two distinct lines of approach to this objective. One is to reduce service lives for most depreciable facilities.[6] The other is to provide, for the taxpayer who regards the guideline lives as incorrect in the light of his circumstances, a set of rules upon which he may rely in assigning service lives which do reflect his special circumstances.

The objective is to afford assurance that depreciation rates will be based on the current and prospective situation respecting asset use rather than on the past. Secretary Dillon was explicit on this point in his statement upon issuance of the Revenue Procedure: "Our depreciation practices have not been realistic for a great many years. Based essentially on taxpayers' past replacement practices, they have inadequately reflected the fast-moving pace of economic and technological change. The new depreciation guidelines correct this fundamental flaw. . . . The rate of depreciation permitted under the rules will not be tied to past history—it is tied to concurrent adoption of replacement practices consistent with the lives which are claimed for tax purposes."

The new guideline lives were based on intensive studies by the Treasury and, it is claimed, represent much closer approximations of the average service lives actually being used by business income taxpayers for tax purposes than those set forth in Bulletin F. Since the new guidelines are somewhat shorter than the average current practice, they appear to reflect the assumption that the economic longevity of depreciable assets will further decline, continuing a trend popularly assumed to characterize the postwar years.[7] Moreover, the Treasury has asserted that the new guideline lives will not be allowed to become out of date but will be frequently revised as circumstances warrant.

The new guideline lives and the subsequent revisions may well serve to eliminate or to reduce materially any such gap between guideline lives and the actual lives of assets now in hand or yet to be acquired. It is in the nature of this approach, however, that these adjustments are made only after a gap has developed and is discernible. The more frequent the revisions of guideline lives and the more up to date the data upon which they are based, the less will be the average gap over time. In this connection, the Treasury's announced determination to keep abreast of the changing circumstances of asset use should be welcome news to business income taxpayers.

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[6] Buildings, other than special-purpose structures, are the major exception. Shorter useful lives were not afforded these assets because gains realized upon their disposition continue to be treated as capital gains, in contrast with the ordinary tax treatment provided by the new Section 1245 of the Internal Revenue Code for personal depreciable property used in the taxpayer's trade or business.

[7] When some rough adjustment is made for changes in the composition of the stock of depreciable facilities through time, *Statistics of Income* data suggest that the service lives used for tax purposes have been declining over the postwar period. These data do not, however, afford an indication of a trend in the duration of asset retention.

The built-in lag in this approach is unavoidable, so long as the Treasury is to offer guideline lives which are thought to be realistic at the time they are offered. The alternative is for the Treasury to be entirely unconcerned with the service lives the taxpayer assigns to his assets, provided only that provision is made for assessment and collection of interest on the taxes deferred by reason of the taxpayer's assigning too short service lives. Proposals for such a system of "free" depreciation are by no means novel, but it seems evident that this system involves much too drastic a revision both in tax accounting practices and in revenues in the short run to be given serious consideration.

The second approach, incorporated in the reserve ratio test, presumably recognizes the fact that the broad industry experience reflected in the new guidelines may be unrealistic for particular taxpayers. It aims further at affording each taxpayer the freedom necessary to adjust his depreciation practice as the circumstances peculiar to him change.

As enunciated in the Revenue Procedure: "The fundamental concept underlying the new Procedure is that the depreciation claimed will not be disturbed if the taxpayer's retirement and replacement practices are consistent with the life used. The reserve ratio test provides a technique for establishing the consistency between the tax life used and the *subsequent* retirement and replacement policy of the taxpayer." (Revenue Procedure 62-21, page 4; italics added.)

An illustration may serve to make clear the change in policy implied by this statement. Consider a taxpayer who has been using a service life for his facilities longer than the newly applicable guideline life. Under the provisions of the Revenue Procedure, the taxpayer may switch to the new life for both existing facilities and those he subsequently acquires with confidence that the resulting depreciation changes will not be disputed, if he accelerates retirement and replacement of existing facilities so as to bring his reserve ratio within the range shown in the published tables by the end of an ensuing period equal to the new guideline life. He may, similarly, switch to a service life shorter than the new guideline life or shorter than both the life he has been using and the new guideline, if his subsequent retirements and replacements bring his actual reserve ratio within the appropriate range by the end of the new replacement cycle. Before the new provisions were adopted, on the other hand, the taxpayer would have had either to convince the revenue agent that such a reduction in service life would subsequently prove to be warranted, despite the contrary evidence of his recent replacement practices and the lack of official criteria to support him, or he would have had first to accelerate retirements and replacements and to do so over a period sufficiently long to establish the normality of the new replacement cycle as the basis for assigning an equivalently short service life to his facilities. In other words, for some considerable time he would have had to retire assets over a period shorter than the life assigned to them for purposes of computing depreciation charges. Since under group or composite depreciation accounting such premature retirements do not ordinarily give rise to tax deductions for unrecovered depreciable basis, this policy tended to require the taxpayer to incur losses not recoverable for tax purposes in order to establish an historical justification for assigning shorter service lives to his facilities. In contrast, the new rules permit the taxpayer to adjust replacement practices subsequent to, rather than in anticipation of, reduction in service lives.

If the taxpayer fails to make an adequate adjustment in his retirement and replacement policies, no effort is to be made to recompute the depreciation allowances which proved excessive, and no penalty is to be assessed with respect to the taxes deferred by the excessive allowances. Instead, service lives to be used subsequent to determination that retirements and replacements have not been adequately accelerated will be adjusted upward in line with the taxpayer's actual replacement pattern.

It is evident that these new rules may substantially strengthen the taxpayer's hand in negotiations with the IRS and afford him a degree of liberality and flexibility not available for many years past. But there are limits to this liberality and flexibility. In the first place, the use of the test does not permit differentiation of service lives for assets acquired in different years, except insofar as they fall into different guideline classes. Thus, to reduce the service life assigned to assets acquired in any particular year, a similar life must be assigned to all existing assets in the same guideline account. Justification of the shorter service life for the new assets by use of the reserve ratio test, therefore, requires accelerating the retirement and replacement of all of the assets in the account, even though they may be substantially different in terms of the characteristics which determine their economic longevity. A substantial price tag, in other words, may be attached to obtaining the advantages of a reduced service life for any portion of one's facilities.

Moreover, the new provisions do not really permit the taxpayer to shuck off the constraint of past experience. Instead, they move forward in time the retirement-replacement experience to be treated as relevant. For example, consider a taxpayer's situation a new replacement cycle after the effective date of the Revenue Procedure. The

TAX REFORM: DEPRECIATION PROBLEMS*

By Norman B. Ture
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During 1962, substantial changes were made by administrative action in the ground rules governing depreciation for tax purposes, and an investment tax credit was enacted. The following discussion describes the new depreciation provisions and appraises them in the light of their announced objectives, but does not presume to suggest any further policy developments in this area which might be desirable.

The objectives which have been associated with the new depreciation provisions include: (1) reduction in service lives to take account of the more rapid obsolescence alleged to characterize use of depreciable assets in the postwar era, hence permitting use of more realistic service lives for computing annual depreciation charges; (2) affording the taxpayer greater freedom and flexibility in assigning service lives to depreciable facilities in line with his own changing circumstances and greater assurance that these determinations will not be subject to challenge by the Internal Revenue Service; (3) by shortening service lives, increasing both the cash flow generated by and the rate of return realized on capital outlays, thereby providing a stimulus for expanding the rate of real capital formation; (4) providing greater incentives and capability for modernizing plant and equipment, thereby reducing production costs and improving the competitive position of American industry vis-à-vis foreign producers, hence improving prospects for resolution of the U.S. balance-of-payments difficulties; and (5) providing greater simplicity in depreciation accounting. This is surely an ambitious program even though it is estimated to cost only $1.5 billion on a full-year basis. If achieved, it will represent one of the biggest bargains in U.S. fiscal history.

Major Provisions of the New Depreciation Policy[1]

The new depreciation rules were issued on July 11, 1962, under the title of "IRS Revenue Procedure 62-21." The Revenue Procedure affords a complete revision of Bulletin F, the earlier list of suggested service lives for various kinds of depreciable facilities, and provides a

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* In the preparation of this discussion, I was ably assisted by Mrs. Julia Clones.

[1] A detailed description of these provisions is provided in U.S. Treasury Department, *Depreciation Guidelines and Rules,* Internal Revenue Service Publication No. 456(7-62), July, 1962.

My major conclusions are these. Starting from the premise that the problem of structural tax reform is one of equity and investment incentives rather than aggregate saving, there is no good case (except, perhaps, on grounds of tax politics) for reducing the weight of the individual income tax in the over-all tax system. While equity and incentive considerations coincide regarding some cut-back in top bracket rates, conflict may arise in the treatment of capital gains. A proper solution on equity grounds (taxation of gains at death with constructive realization) would be economically helpful in eliminating locking-in effects, but the eventual taxation of hitherto untaxed (unrealized) gains might be harmful.

As for the corporation tax, the equity problem would be solved with full taxation of capital gains; but short thereof, a corporation tax with dividend-paid credit is needed to include undistributed earnings in the tax base. From the incentive point of view, the most potent solution (and the one least in conflict with equity considerations) is given by the investment-credit type of tax relief. In all, the conflict between equity and incentive considerations can be held in bounds, provided that a sufficiently comprehensive and courageous view of tax reform is taken.

time, these devices introduce an element of discrimination between corporations and their owners which conflicts with the criterion of equal treatment. In order to minimize the inequality which results from reducing the over-all tax on profits (assuming that it is proper to begin with), a new inequality (relating to the relative treatment of various profit recipients) has to be taken into the bargain.

Much of the recent reasoning on the comparable effect of alternative devices on profitability implies that profitability—in the sense which is relevant for investor behavior—is in fact reduced by the tax, and hence increased by rate reduction. It disregards the fact that a tax with loss offset reduces both losses and gains, with the result that an investor who responds to the rate of return on risk (the ratio of probable gains to losses) finds his reward unchanged.[15] Having some vested interest in this type of investor, the recent return to preloss-offset thinking (and I plead guilty myself) bothers me. While the investment credit is still superior to rate reduction on these grounds,[16] the obvious remedy (if the loss-offset line of reasoning were valid) would be neither a general rate reduction nor an investment credit, but an increase in the loss-participation relative to the gain-participation rate.

6. Apart from the profitability effect, reduction in the corporation tax may raise investment because the internal flow of funds is increased.[17] This result may be solicited most effectively by introducing a retained earnings credit along the line of the German practice in the early postwar years. Such a measure, however, is diametrically opposed to what needs to be done on equity grounds. The dividend-paid credit in turn, while most desirable as a partial move towards integration on equity grounds, is undesirable (as far as the internal funds argument goes) on growth grounds. I am inclined, however, to discount the great weight frequently placed by the business community on this argument and to resolve this particular conflict on equity grounds.

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[15] Stated thus briefly, certain qualifications of the argument are overlooked; e.g., the tax is not truly general, and wages of management may be subsumed in profits. See my paper with Evsey Domar, "Proportional Income Taxation and Risk-Taking," *Q.J.E.*, pp. 387–422, May, 1944. Similar conclusions are reached if the risk is defined as the dispersion of the probability distribution of positive and negative yields. See J. E. Tobin, "Liquidity Preference as Behavior towards Risk," *Rev. of Econ. Studies*, pp. 65-87, Feb., 1958.

[16] Assume an investor who before the credit would have invested \$100. Now a credit of 10 per cent is granted. Consider two possible reactions: (1) The investor continues to invest his own \$100, but can now buy an asset worth \$110. His gain prospects are increased by 10 per cent, while his loss prospects are as before. (2) The investor now invests \$90.9 of his own funds, which with the credit of \$9.09 purchases an asset of \$100, and holds \$9.1 in cash. His gain prospect is unchanged, but his loss prospect is reduced by 9 per cent.

[17] Concern with tax effects on internal funds is quite compatible with our earlier conclusion that the supply of savings should not be dealt with through adjustments in tax structure. The flow of internal funds here enters not as a general supply of saving, but as an inducement to invest.

capital stock which did occur, it is indeed difficult to explain the constancy of the net rate of return without such an assumption.

If this is correct, and there has been a high degree of shifting in the short-run sense, the corporation tax did not have the disturbing effects on profitability and the flow of funds which it is alleged to have had. The offense against horizontal equity, similarly, was not by "double taxation" of dividends but by sales or wage taxation. Given a symmetrical assumption of "unshifting," repeal would be desirable on equity grounds, even though it would be of little significance on growth grounds.

But suppose that conditions with regard to shifting are asymmetrical, i.e., that the tax is passed on in the case of rate increase, but that there is no corresponding unshifting in the case of rate decrease. In this case (which also seems to have some support in our findings) repeal would be unjustified on equity grounds, as it would result in a transfer from other taxpayers to profits. At the same time, it would be effective as a profit subsidy and be helpful to growth. There would be much merit, however, in economizing the scope of subsidy by rendering it most effective; i.e., by using a selective rather than over-all rate-reduction approach.

5. Let me now take a less heroic view and recognize that the corporation tax is accepted as an equitable part of the tax structure. One may then inquire what type of corporate tax relief would be most helpful on growth grounds, while requiring the least total reduction in the yield of the profits tax.

Taking the view that investment responds to the net rate of return, a comparison may be drawn between the profitability-effects of rate reduction, accelerated depreciation, and the investment credit, assuming the revenue loss to the Treasury to be the same in all cases.[13] The result depends on how the condition of "equal revenue loss" is defined,[14] but there is good reason to conclude that the credit approach, especially in the form of a marginal credit, is most effective. At the same run adjustments) into higher prices or lower wages, it is objectionable as a discriminating product or wages tax. Given these considerations, it is difficult to see why there should be so much concern with horizontal equity in the individual income tax if the corporation tax is to be accepted on purely pragmatic grounds. The best solution from an equity point of view would be to assure full taxation of capital gains (including constructive realization at death) and then to forget about the corporation tax.[9]

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[13] The catalogue of measures here given is by no means complete. As an alternative to a marginal investment credit, see W. Baumol's proposal for a tax-subsidy scheme relating to growth in sales, which has the additional feature that it may be placed on a self-financing basis by penalizing declining firms. See K. Knorr and W. Baumol, ed., *What Price Economic Growth* (Prentice-Hall, 1961).

[14] Reference may be to (*a*) receipts over a finite period. Or, it may be to (*b*) receipts pertaining to the effects over infinity of a change in the tax law applicable for a finite period. If accelerated depreciation is introduced in 1962 and discontinued in 1965, the revenue effect (assuming a maximum life of twenty years) will extend to 1985. The question is whether the positive "tail" from 1965 to 1985 should be counted. Finally, reference may be to (*c*) receipts over infinity. Also, there is the question whether to define the loss of receipts in terms of (*d*) present value, or (*e*) independent of timing. Using assumptions (*a*) and (*e*), the investment credit is much superior to accelerated depreciation, especially if the period is brief (say, five years) and the economy is rapidly growing. Using (*c*) and (*d*), and assuming the Treasury's rate of discount to be the same as that of firms, the various approaches are equally effective.

Given preferential treatment of gains, some form of corporation tax need be retained. Corporate profits (whether retained or paid out) might be taxed to the shareholder as if he were a partner, a procedure which to me seems by no means as difficult as it is usually made out to be. Or, within the confines of a partial solution, the corporation may be given a dividend-paid credit while retained earnings are taxed at the corporate level.[10]

4. A comprehensive reform along these lines would be a gain in equity and, most likely, also on growth grounds. But the matter of shifting must be looked at more closely before adopting such a solution. It is usually assumed that the corporation tax, initially at least, falls on profits. But does it? Observation of the rate of return on capital invested in corporations shows that gross returns in the fifties were nearly twice those of the twenties, but that net returns (notwithstanding an increase in the corporate rate from 5 to 50 per cent) have remained about unchanged. While this observation is compatible with a hypothesis of full shifting, it is not conclusive since the effects of the corporation tax are not isolated from other factors, nor does it tell us (which is crucial for purposes of this paper) whether shifting took the form of initial price-wage adjustments, or whether it operated over the longer run through a reduction in capital input.[11] A recent attempt to isolate the tax from other effects, however, suggests that there has been a high degree of short-run shifting,[12] and given the expansion in

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[9] The net revenue loss from complete integration might be —$22 billion (corporation tax yield) +$7.7 billion (35 per cent personal rate on $22 billion gain in profits) +$3.5 billion (35 per cent on retained earnings of $10 billion) = $10.8 billion. Deducting further a service-charge-type value-added tax yielding, say, $3 billion (to be passed on to the consumer), and $3 billion due to capital gains reform, we would be left with a net loss of $5 billion, requiring an about 10 per cent average increase in income tax rates as offset.

[10] Substitution of a value-added tax for the corporation profits tax would not solve these problems. A value-added tax is more or less similar to a general proportional gross income tax without exemptions. It is neither a means of integrating retained earnings into the existing income tax, nor a substitute for a profits tax.

[11] If there is shifting of the latter type, depressing effects on growth result; if it is of the former type they do not. Similarly, if shifting operates through reduced capital formation, the double taxation argument remains valid; if shifting reflects a short-run adjustment with profits unchanged, it does not.

[12] See M. Krzyzaniak and R. A. Musgrave, *The Incidence of the Corporation Tax, An Empirical Study of Its Effect upon the Rate of Return* (to be published by Johns Hopkins Press).

offer a low-risk haven to potential risk capital, and substitution (if needed) of a more efficient technique of federal aid to state and local debt financing. The other is removal of barriers to executive mobility which now arise from various devices to cushion the impact of high marginal rates on executive compensation.

While venture-type investment largely originates in the upper brackets, subject to marginal rates of 50 per cent or more, total equity investment nevertheless derives a significant contribution from the middle and lower ranges. General rate reduction further down the line, however, is limited sharply by the much larger revenue loss involved; and rate reduction which is marked by an offsetting broadening of the base—highly desirable though it may be on grounds of horizontal equity and administrative simplicity—does not make a major contribution on growth grounds.[5]

There remains the possibility of granting preferential treatment to capital income or investment.[6] Since the distribution of capital income is much less equal than that of wage income, a vanishing capital-income credit (illustrated in crude form by the dividend deduction) would permit an investment incentive without conflict with progression and without large revenue loss. At the same time, it would leave us with a new and, considering the tradition of earned-income credit, particularly unhappy offense against horizontal equity. A less objectionable approach may be in the direction of inducements to investment, especially investment in education; e.g., provision for depreciation of education costs.

3. Turning now to the corporation profits tax and the corporate investment decision, we begin with a quite different premise. Whereas the individual income tax need be strengthened as the mainstay of an equitable structure, there is little place therein for an "absolute" corporation tax.[7] If the tax falls on profits in the first instance, it discriminates against dividend income.[8] If shifted (in the sense of short-

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[5] Suppose that we combine base broadening with rate reduction so as to leave total yield unchanged, the rate reduction taking the form of an equal percentage cut in all bracket rates. As a result, average as well as marginal rates will be reduced for each bracket of taxable net income, but the ratio of gross income to net income will be increased. On the average, the two factors will tend to wash out leaving the income as well as the substitution effect unchanged.

[6] The problem is not solved by a progressive spendings tax since the need is for investment, not saving, incentives. A comprehensive solution, therefore, would require a progressive spendings tax plus a hoardings tax.

[7] This is compatible with having the corporation (as well as unincorporated firms) pay a tax for services rendered by government. However, such a tax would be much smaller (especially at the federal level) and should take the form of a value-added rather than a profits tax.

[8] This is evident once it is recognized that the entire concept of horizontal equity relates to the individual as the bearer of tax burden. The corporation is a powerful unit from a social or economic point of view, but it does not follow that it has taxpaying ability of its own.

range of 50-80 per cent which a naïve view of statutory rates might suggest. If actual liabilities are related to adjusted gross income raised to include realized long-term gains at 100 per cent, as they should be for purposes of this analysis, effective rates for both brackets drop to about 30 per cent.

The picture is similar if marginal rates are considered. Including realized gains at full value in adjusted gross income, actual marginal rates (ratio of increment in liability to increment in income) reach 39 per cent in the $40-$75 thousand range, and thereafter decline to 30 per cent in the above $500 thousand range. These rates again stand in stark contrast to the 75-91 per cent rates which a naïve view of statutory rates would suggest.[4]

From an equity point of view this situation calls for heroic reform. The answer is to lower the high bracket rates to levels which Congress is willing to enforce, and then to assure that they apply uniformly to the full income received. In the tradition of the accretion concept this requires constructive realization at death and taxation of gains at full rates, allowing for appropriate averaging and corresponding treatment of losses.

From the growth point of view, the situation is not as clear cut. While the present system of income taxation imposes a potential disincentive effect of high rates where paid, it also offers the incentive effects of exempting large parts of capital income (i.e., income received in the form of capital gains, especially if not realized) from the progressive income tax. Since the dividend income to which the high rates apply is more in the nature of a rent income, while entrepreneurial returns are more largely reflected in capital gains, the present system—bad though it is from the equity point of view—has attractions on growth grounds. Full taxation of gains, even if combined with a substantial cut-back of marginal rates, could well leave capital income in a less favorable state. There are, however, two factors which narrow the conflict between growth and equity considerations. One is that constructive realization would largely remove the locking-in problem, the more so if it could be combined with a roll-over provision for realized gains during life; and the other is that taxation at death should be less of a disincentive than taxation during life. Depending on the weight of these factors, the investment effects of a heroic capital gains reform may not be disadvantageous on balance.

Two other upper bracket problems might be mentioned, where both equity and growth considerations point in the same direction. One is the removal of tax exemption on state and local securities, which now

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[4] For details, see my paper, "How Progressive Is the Income Tax," *Tax Revision Compendium,* Vol. 3, pp. 22-23, Committee on Ways and Means, Nov. 16, 1959.

II

I now turn to some major areas of tax reform, and their implications for equity and growth. My equity criteria are: (1) that the index of equality should be defined in terms of accretion; (2) that people in equal position should be treated equally without discrimination by uses or sources of income—horizontal equity; and (3) that people in unequal positions should be subject to moderately progressive taxation—vertical equity. My concern with growth relates to investment (rather than saving) incentives. These incentives may be given either on the income sources side (preferential treatment of profit income) or on the uses side (preferential treatment of investment outlays). In either case, horizontal equity is interfered with. Profit income, moreover, is a more important source and investment outlay a more important use at high than at low levels of income, thus suggesting a conflict with vertical equity as well. Given the importance of both equity and growth objectives, what should and what should not be done by way of tax reform?

1. To begin on a negative note, I see little merit in the argument that growth policy calls for a large-scale change in the tax structure from direct to indirect taxes, including substantial replacement of the individual income tax by a sales tax. As noted before, such a substitution is not needed, in the context of the U.S. economy, to increase the supply of saving. The objection, if any, is to income tax effects on work and investment incentives. Such effects, to the extent that they do exist, relate primarily to the higher bracket rates, my hypothesis being that the weight of the substitution effect rises relative to that of the income effect as the bracket rate increases. Since the higher bracket rates carry little significance from a revenue point of view—some 80 per cent of the total income tax yield of $45 billion is derived from the first bracket rate, while rates over 50 per cent contribute 1 per cent only—this problem can be met without changing significantly the weight of the income tax in the total tax structure and without weakening its vital role in securing a progressive tax-distribution over the lower and middle income ranges.

2. Next, there is the question whether income tax progression should in fact be reduced.

As far as the upper brackets are concerned, we are faced with an anomalous situation of very high statutory rates which apply in a very spotty fashion only. For instance, the actual average or effective rate (ratio of tax liability to adjusted gross income, both as reported) ranges from about 40 per cent in the $100 to $150 thousand bracket to about 50 per cent in the bracket above $1 million, as against a nomics to become ever more classical in its assumptions, need to be reconsidered. This is the case especially in the present setting where easy money is limited by balance-of-payment considerations, and the brunt of investment-inducing policies is thrown on the tax system.

5. There remains the basic problem of by how much capital formation must be raised to achieve a given gain in growth. Suppose tax measures are taken which raise gross fixed business investment by 20 per cent; i.e., from about 10 to 12 per cent of GNP. With a GNP of \$570 billion (hopefully estimated for 1963) this means an increase in gross investment of over \$10 billion and implies an at least one-third rise in net investment. How much would we have accomplished? According to Solow, such a gain in investment would step up the growth rate by a full percentage point; but according to Denison, the gain would be only two-tenths of a percentage point, a doubling in the rate of capital formation being needed in order to gain a full point in growth.[3]

These two conclusions spell vastly differing lessons for tax policy. In the one case, feasible changes in the tax structure may make an important contribution to growth; in the other, the pay-off is so small that there is little point in disturbing equity considerations. The divergence, which results from different treatments of technical progress, is not easily reconciled, leaving tax policy for growth in a double state of ignorance. Neither do we know by just how much various tax adjustments will raise investment, nor do we know by how much a given rise in investment will increase growth. All that is known (more or less, at least) is the sign of the growth derivative which attaches to various tax measures. This is enough to go by if the growth measures carry no opportunity cost in reduced tax equity; but where they do, policy prescription is difficult since the terms of trade between equity and growth are largely unknown.

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investment constant this can be true only with a marginal propensity to consume larger than 1. With $c_m < 1$, the expansion must generate a substantial increase in investment, such that

$$\frac{t_o}{1 - c_m(1 - t_o)}(G + I_o) = \frac{t_1}{1 - c_m(1 - t_1)}(G + I_1) \quad \text{and}$$

$$\frac{\Delta I}{I_0} = \frac{G + I_0}{I_0}\left[\frac{n(1 - c)}{[1 - c(1 - t_0)](1 - n)}\right]$$

where I_o is the initial expenditure level, G is the level of government purchases, c_m is the marginal propensity to consume, t_o is the initial tax rate and n is the proposed percentage cut in t_o. With $G = 100$, $I_o = 50$ and $t_o = .3$, a 20 per cent rise in investment is needed to neutralize the yield loss from a 10 per cent cut in tax rate.

[3] See Robert M. Solow, "Technical Progress, Capital Formation and Economic Growth," especially p. 85; and Edward F. Denison, "How to Raise the High-Employment Growth Rate by One Percentage Point," especially p. 72. Both in *A.E.R.*, May, 1962.

On economic grounds, this is not a serious conflict, since a substantial gain in yield (and public saving) may be derived from slight increases in lower bracket rates which would still remain at levels not likely to pose a serious incentive problem, without demanding excessively high marginal rates at the end of the scale. On political grounds, it may be that incentive-type reforms are hard to come by and feasible only in a context of general rate reduction. But if this is the case, it will be even more difficult to secure the much more extensive structural changes which would be needed to raise saving by a substantial amount.

Given such a deadlock, fiscal policy for growth may require a reduction in the level of government expenditures of the consumption type, possibly at the cost of resulting distortion in resource use as between private and public consumption.

4. The other side of the coin is that tax incentives to investment must be accompanied by measures to assure a sufficient level of total demand. Unless this is done, increased capacity cannot be utilized and is wasteful. This is obvious, but there is the less evident point that structural tax changes to induce investment may affect the level of deficit or surplus needed for this purpose.

Since these structural changes may involve preferential treatment of profit income, they may well reduce the marginal propensity to spend (on investment plus consumption); i.e., raise the marginal propensity to save in the private sector. If so, this will require a decreased surplus (or increased deficit) on the public side. Suppose that the marginal propensity to consume out of wage income is 80 per cent and out of profit income 20 per cent. Substitution of $1 billion of taxes on wages for $1 billion of taxes on profits curtails consumption by $600 million. Also, suppose that it raises investment by $500 million. The change in tax structure thus raises planned saving by $600 million but investment by only $500 million. Hence, a fiscal policy aimed at raising the share of capital formation in a full employment economy requires decreased government saving (or increased dissaving) of $100 million.

It follows that the association of higher growth with higher budget surplus or lesser deficit (the proposition that a fiscal policy for high employment and growth requires a larger surplus or lesser deficit than one for high employment only), which has become so widely popular in recent years, is by no means a logical necessity. It is an empirical proposition which, beyond the short-run context of a cyclical upswing, rests on what seems to me rather optimistic assumptions.[2] Its broad acceptance, reflecting the latent tendency for neoclassical eco-ing the weight of the corporation tax, extremely large changes in the tax structure towards regression would be required (short of a Kaldor-type progressive spendings tax) to raise personal saving by even a modest amount.[1] This is an unnecessary cost to incur. If all that has to be done is to secure a higher rate of saving, let the surplus be increased (or the deficit be reduced), and the funds thus gained be made available through debt retirement (or reduced borrowing) to the private capital market for investment. At the same time, let the composition of the tax structure be determined by equity considerations.

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[2] For similar reasons I am skeptical regarding the proposition that a tax cut will involve no net revenue loss since the initial loss will be recovered in the subsequent upswing. With

2. The primary and more difficult task, however, is to induce a higher level of investment, and this is a different matter. It cannot be accomplished in sufficient measure by reducing the general level of tax rates. Notwithstanding the current need for general rate reduction as a means of reaching full employment through raising the level of demand, the reduced rate level (if the cut is applied across the board) will still be sufficiently high to leave an incentive problem for growth purposes; and even though the yield from any given set of rates will rise as the economy grows, the outlook for future expenditure needs suggests that we will be left with a high tax to GNP ratio for some time to come. In this situation, structural adjustment within a given yield total offers a much wider scope for action, and the issue of structural reform must be faced.

What these adjustments should be depends on how investors behave. While the issue of structural reform exists only to the extent that capital formation is not determined rigidly by an accelerator function—otherwise, the problem is merely one of aggregate demand and may be handled by adjusting rate levels rather than tax structure—the proper remedy differs with the type of response, e.g., to changes in the net rate of return and/or changes in the internal flow of funds. Conflicts with tax equity are likely to be involved in either case, and the problem is how they may be minimized.

3. Given a need for tax reforms to provide investment incentives, must we reconsider the preceding conclusion that increased saving, when needed, should be provided through higher tax rates?

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[1] This conclusion is illustrated by the following estimates, showing the increase in saving (reduction in consumption) which would result from various tax substitutions, leaving total yield unchanged:

Substitution	*Estimated gain in saving (million dollars)*
1. Reduce the high bracket rates of individual income tax to 50 per cent, substitute 2 per cent increase in other rates	+ 300
2. Reduce individual income tax rates by 10 per cent, substitute consumer tax of $5 billion	+ 600
3. Replace individual income tax with a flat rate tax on gross income	+3,700
4. Replace individual income tax by sales tax	+5,500

For details, see my paper, "Effects of Tax Policy on Private Capital Formation," to be published by Commission on Money and Credit.

GROWTH WITH EQUITY*

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The novel issue in the current debate over tax reform lies in the interplay of equity and growth objectives. Were it not for the former, the certain way to avoid disincentive effects would be through adoption of a single-tax system, involving the use of a head tax only. The absurdity of such a "solution" should leave no doubt that both objectives must be considered. The purpose of this paper is to examine how they may be reconciled.

I

Much depends on certain premises pertaining to the economics of growth. These need to be set forth before specific tax changes can be considered.

1. The nature of growth policy is sometimes seen in these terms: If there is to be more growth, there must be more capital formation, and if there is to be more capital formation, there must be more saving. More saving may be provided for by more extensive use of taxes which fall on consumption and less extensive use of taxes which fall on saving; therefore, the way to provide for more growth is to substitute taxes on consumption for taxes on saving. I see two objections to this argument.

The nature of our economy is such that provision for saving is a permissive, not a sufficient, condition for growth. That is to say, tax policy for growth cannot be conducted on the simple assumption of *ex ante* equality between saving and investment However valid in other settings, such as India or the capital-starved postwar Europe of the fifties, this premise does not meet our present case. Policies to increase the rate of saving without assurance of corresponding gains in investment would tend to retard the growth of the economy, not to raise it.

Moreover, even after provision for increased investment has been made and increased saving becomes called for, this increase may be provided better by raising the general level of rates and yield from a given tax structure than by changing the tax structure with a given level of yield. The former approach is more flexible (which is important in view of fluctuating levels of investment) and avoids the heavy costs in tax equity inherent in the latter solution. Apart from substantial gains in business saving which may be obtained by reducvironment of gimmicks and gadgets, it would have seemed wise to modify the proposal to improve its acceptability and thereby its effectiveness.

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* Thanks are due to N. D. Baxter and T. S. Ribich for helpful comments.

The new provisions in the tax law regarding tax havens, though they seem to go too far in one respect, are not too oppressive, and anyone reading only the law itself would not be likely to consider that their adoption would have had any significant adverse effect on foreign investment. But the original proposal of the Administration to prevent tax-haven abuses proposed current taxation of all undistributed income of all foreign subsidiaries. This proposal revealed misconceptions about the reasons for foreign investments, and the nature of competition abroad, and is likely to discourage foreign investment generally. Future tax inducements for selected forms of foreign investment are not likely to offset the basic damage arising from the extreme proposal originally advanced and as yet unrepudiated.

In a paper before the National Tax Association last fall, Professor Richard Musgrave emphasized that in the real world the desire to invest was more important than the flow of savings. In agreeing with him, I wish to emphasize that the willingness to use funds depends on much more than profit margins and prospective returns of and on capital outlays. Though attitudes and the elusive element of confidence cannot be fitted neatly into our models, we cannot ignore them in formulating tax policy.

It may not be intellectually satisfying for economists to try to plan in a world with such uncertain elements as attitudes and confidence of investors and business management. But those who form and use capital live in a world full of uncertainties. Those concerned with tax policy must go beyond mechanistic analyses and learn to live with a little uncertainty themselves. Their policies, their statements, and our tax system must take account of attitudes and confidence, even when irrational, if we are to minimize the tax restraints on sustainable economic growth.

be estimated with our present knowledge. The foregoing brief descriptions of the tax factors bearing on the formation and use of capital might be regarded as a list of topics for systematic research. In many instances, however, the direction of the influence is so clearly adverse that changes in the law seem indicated without evidence on the magnitude of the bad effects.

All of the foregoing analysis covered familiar points which are assembled here as a matter of convenience. It dealt with tax factors in a rather mechanical fashion, noting the way in which they produce differences in yields, shifts in margins, and other specific consequences which can generally be taken into account in a rational manner. In my concluding paragraphs, I wish to turn to the importance of the tax law, and its administration, as a major element influencing the general atmosphere in which investors and business management make their decisions. These effects of the tax law are quite elusive and certainly not subject to measurement. They may be irrational and even unreasonable. But they are nevertheless important, perhaps much more important than the influences subject to rational appraisal.

The tax laws have, I suggest, a symbolic importance which transcends their pecuniary impact. Tax provisions which are regarded as penalizing enterprise and investment may reduce economic activity to a much greater extent than could be explained by the pecuniary importance of the taxes. Modifications of the law which indicate approval and sympathetic understanding of business and investor problems may also have unexpectedly large beneficial results.

The grounds for confidence or the lack of confidence are complex and often indeterminate. Tax policy and tax administration are among the many factors bearing on confidence and may have only minor importance. But changes in the tax law may contribute to a sense of confidence and well-being—perhaps even to a general feeling of euphoria—which will vitalize an economy. And the atmosphere in which the changes are made can be as important as the substance of the changes. Large concessions made grudgingly may be less useful than small concessions made heartily and with sincere regret that they cannot be larger.

If proposed reforms are not what the beneficiaries actually want or do not fit in with the way they think, the net effects may fall far short of what might be rationally expected. The continued insistence by the Administration on an investment credit in 1961 and 1962, instead of still faster depreciation or a special first-year depreciation deduction, may turn out to be an example. If business management regards the investment credit as a gimmick and does not like to operate in an en-the total supply of capital; they also direct investment into those areas appropriate for trustees and insurance companies. The availability of deferred compensation arrangements for executives, as an alternative to coverage by a qualified pension plan, will tend slightly to reduce the use of pension plans. Since deferred compensation contracts cannot be funded without catastrophic tax consequences, the net result may be a slight reduction in the formation of capital.

To the extent that nonvested pension plans freeze executives with their existing employers, there will be a less than optimum allocation of executive talent with a consequent less than optimum direction of capital resources. But the availability of deferred compensation contracts can help to move executives, and hence improve the use of capital.

Stock options, if properly used to give management an interest in long-term growth, can improve the utilization of capital resources; this objective is the principal justification for a differential tax treatment for them. If abused by quick sales of stock acquired under options, their influence may actually be adverse because of an undue emphasis on immediate profits. Options may also serve to keep imaginative executives from leaving established companies for new ventures where they can secure original stock with large potential for appreciation and capital gains without the use of options. In this respect, stock options may help to restore tax neutrality between large and small business.

The tax law encourages the use of trusts extending over several generations. This practice helps to maintain the supply of capital by reducing the estate and gift taxes which would otherwise deplete it, but trusts direct capital funds into trustee-type investments. Trusts may also convert the beneficiaries into passive *rentiers* instead of active entrepreneurs. A removal of the tax incentive for long-term trusts with a reduction of estate tax rates would produce the same revenue and remove these two adverse effects of the present law.

The provision of the tax law permitting tax-free exchanges of stock and securities in mergers fosters large aggregations of capital which may or may not increase the effectiveness of its use. The present tax law is not neutral, since prospective purchasers of a company who will continue it as a separate business entity must offer enough more to cover the capital gains tax arising on an outright sale. The tax-free roll over of reinvested gains, mentioned previously, would remove this tax discrimination against continued independent ownership of business.

Though the direction of all the foregoing tax influences are fairly apparent, their quantitative importance in most instances cannot even costs, that is, on value-added less profits, would put a tax premium on the efficient utilization of resources, reversing the present tax penalty. Since value-added also includes the cost of using capital equipment, as measured by depreciation, a value-added tax may be regarded as discouraging capital investment, at least in contrast to a net income tax in which depreciation is a deductible expense. (A value-added tax is, however, neutral between direct labor and depreciation, and whichever method of production gives the lower cost will still give the higher net profit. The inclusion of depreciation among the costs in a value-added base might be regarded as nullifying the tax inducements for capital investment given by investment credits and special amortization in an income tax, but these inducements could be revived in some other form under a value-added tax.)

Tax allowances for depreciation can be especially important in both the formation and use of capital. The immediate and direct effect of faster depreciation is simply a reduction in current taxes, with more funds available for general corporate purposes, including dividends. But to the extent that book depreciation is conformed to tax depreciation, as it frequently is, with faster depreciation more corporate internal funds will be deemed to arise through depreciation rather than as net income.

Funds arising through depreciation are typically considered as available for purchases of capital equipment to a greater extent than funds arising from net income. Furthermore, the fact that capital equipment is fully depreciated on a company's books often makes it more likely that it will be considered for replacement. This is true even though the extent of book depreciation should be irrelevant in decisions on replacement; it is significant at times even in companies in which the top management explicitly directs that the extent of book depreciation should be ignored. Replacement of property before it has been used for its expected life apparently suggests that it has not "paid its way" or that the recommendation for its original purchase was bad; though irrational, such attitudes must be recognized.

The Administration should be congratulated on the new depreciation guidelines, though the new reserve-ratio test will restrict the use of the faster depreciation otherwise available. No such limitation is imposed in Western European countries. The test should be dropped here, and the authorized lives further shortened, if our business is to get the benefit of tax depreciation comparable to that available to its major competitors.

Many specific provisions of the individual and corporate income taxes influence the formation and use of capital. A few of them may simply be listed here. The pension fund provisions probably increase likely to do so if the differential is great compared to the repressive effect of the tax system on the formation of capital.

But it should not be assumed that there is complete or even a high degree of fluidity in investment portfolios. Even with the very large tax advantages of individual investment in oil and real estate ventures, many investors do not seriously consider these tax-favored areas. Custom and familiarity are of great importance to each individual. Probably very few investors consider the full range of possible investments. An inherent suspicion of the unfamiliar can largely nullify a tax advantage in a specialized form of investment.

The reaction of individuals to very high income tax rates is not uniform. Some shift into investments with higher yields, seeking to maintain a previous or desired level of net income. Others, finding the absolute differential between the higher and lower yield investments reduced by the tax, shift into the lower yield securities. Others go into tax-exempt or tax-sheltered securities. The differential tax rate on capital gains, especially in conjunction with high individual rates on regular income, unquestionably shifts some capital toward investments with prospects for appreciation, including new ventures.

It must also be remembered, in any consideration of the effects of taxation on the use of capital owned by individuals, that there are virtually no nonpecuniary incentives for its employment, as there are for personal activity. Idle capital does not get bored, it has no instinct of workmanship to be satisfied and, with minor exceptions, the status which capital confers on its owners depends on its presumed amount rather than its form.

The corporation income tax is increasingly recognized as a tax penalty on the efficient use of the nation's resources rather than as a means of absorbing an economic surplus. Whether the tax is shifted forward or borne by the corporation, and ultimately by its owners, marginal firms and marginal production are unaffected or only lightly touched by the tax. The corporation income tax, if not shifted, reduces the net return and the possibilities of expansion of the more efficient producers. If shifted, the tax holds a price umbrella over the less efficient producers. In either case, the corporation income tax tends to place the control of the nation's resources in the hands of less efficient producers.

A value-added tax imposed on business, by contrast, would treat costs and profits equally and make all users of productive resources bear a share of the costs of our collective expenditures made through the government. The case for a shift from business income to value-added taxation appears to be a strong one to improve the efficiency in the use of capital and of human resources as well. A tax on business a longer period, the effect would be much more uncertain. We need to know a great deal more about the determination of dividend policies before we can appraise with much confidence the exact effects of a tax which reduces the availability of funds for redistribution and retention.

Since a substantial fraction of total corporate income is retained, it seems probable that an unshifted tax would curtail retention of income substantially. If one makes the not unreasonable assumption, in the absence of evidence to the contrary, that the effect is divided between dividends and retention in proportion to these two uses of net income, the present corporate tax which reduces current corporate income from about $50 billion to about $25 billion could be regarded as reducing retained earnings from $20 billion to the present figure of about $10 billion at the current pay-out rate of about 60 per cent. If half the tax were shifted forward, the unshifted $12.5 billion of taxes might be regarded as reducing retained earnings by about $5 billion; either amount is a substantial fraction of combined personal and corporate savings of something less than $40 billion. The foregoing very rough figures are given simply to indicate possible orders of magnitude.

The comments thus far have dealt with the direct influence of major taxes on the formation of capital. Even briefer comments will be made on the generalized effects of major taxes on the use of capital. If the individual income tax rates are uniform for all sources of income, the tax, while reducing proportionately the net return on all forms of investment income for any taxpayer, makes a greater absolute reduction in the income from the higher return. If the spread in market yields reflects a margin for greater risk, the market balance is likely to be disturbed by a proportionate reduction in yields; thus the two percentage point spread between 5 and 3 per cent securities is reduced to a 1 per cent point spread by a 50 per cent income tax which lowers net yields to 1½ and 2½ per cent. A 90 per cent tax spectacularly reduces the spread to only two-tenths of 1 per cent. Since the risk is related to the capital sum, a smaller absolute difference in net yields, even though it maintains the proportional spread, would be insufficient to offset the risk. The extent to which existing market differentials have been modified and reflect the existing tax structure is not known.

Any differential tax treatment related to the source of investment income will shift investment toward the favored source. Tax-exempt bonds, the combination of percentage depletion and the intangible drilling cost deduction, and various tax aspects of real estate investment are the conspicuous examples of differential treatment in our present laws. A differential, if it is large enough, may actually increase the absolute amount of investment in the favored area; in fact, it is portfolio and a capital gains tax on realized gains which are used for consumption. In the first case the tax is regarded as a capital levy and is likely to have the effect of a capital levy in reducing a nation's supply of capital. When large sums are involved, the capital gains tax may well exceed the amount of ordinary income left after the regular income tax. A restoration of the pretax capital out of savings would require a drastic reduction in consumption for many years—an unlikely self-denial.

By contrast, the capital gains tax on gains which are realized to secure funds for consumption is much more likely to be regarded as a form of income tax, at a favorable rate on a special form of income. If the sale of assets is made to secure a certain net sum for a particular outlay, as the purchase of a house or a child's education, the tax may require a greater disinvestment and, hence, a greater reduction of capital.

But in the more likely event that appreciated capital is regarded as a possible source of funds for consumption, the existence of a capital gains tax will be considered as a tax cost of securing disposable funds, just as a regular income tax is a tax cost of securing regular income, and the net funds will be treated as the available amount. The tax on the realization of gain shifts the margin in favor of continued retention of assets as capital and against the sale of assets for consumption.

The foregoing distinction between capital gains taxation on a shift of portfolio and on a realization of gain for consumption is the basis for the proposal for a tax-free roll over of reinvested gains and full taxation at regular rates of gains which are not reinvested. From the standpoint of economic policy, such a tax treatment would be desirable whenever it was felt important to maintain a nation's supply of capital. The tax-free roll over with full taxation of gains used for consumption also seems equitable in the sense that the tax burden is based on an individual's personal drain on a country's economy, instead of on his increased claim. A change in the law to permit tax-free roll over and full taxation of consumed gains would be a major reform to maintain a country's supply of capital and, by removing the tax penalty on changes in the form of investment, to permit a more efficient use of the available supply of capital.

The corporation income tax reduces the supply of savings, to the extent that it is not shifted, by reducing net corporate income which is either paid out as dividends or retained. The relative influence of the corporation tax on these two uses cannot be stated with assurance. In view of the policy in many corporations of maintaining a fairly steady cash dividend, a change in the tax probably in the first instance would tend to be reflected largely in the current retention of earnings. Over on those segments of income from which savings are greatest.

A highly progressive income tax is also most likely to repress incentives and divert efforts away from the creation of income toward the minimization of taxes—a form of activity in which the gain to the individual adds nothing to the national welfare, to say the least.

In the absence of a desire to restrict savings, the case for high individual income tax rates can be defended only on grounds of equity or as a means of redistributing income. Fortunately, even those who support high rates for these noneconomic reasons now seem to be in substantial agreement that the adverse economic effects make a continuation of very high rates undesirable.

Estate, gift and inheritance taxes, and capital gains taxes on shifts in investment portfolios seem especially likely to deplete a nation's supply of capital. Funds to pay these taxes typically come from the proceeds of the sale of capital assets, purchased by holders of liquid funds which might otherwise have been used to finance new investment. Estates, beneficiaries, and individuals whose capital is reduced by these taxes are not likely to reduce consumption from current income to restore the pretax level of capital.

The significant fact is not that these taxes are imposed directly on capital; so also are property taxes which presumably are regarded either as part of the personal cost of living or as another element of business cost to be reflected in prices. The significant fact is rather the attitude of those on whom the burden of tax falls; death and gift taxes and capital gains taxes on portfolio changes would seem most reasonably to be regarded as capital levies. The capital sum received or remaining to be invested is reduced by a tax which has no reference either to income or to any component part of a person's consumption. If there were only one heir who was very conscious of a pretax capital value of a family property, he might be disposed to try to restore the capital by savings, though with a large estate tax and high income tax, even a saving of all the net income from an estate for a generation might not restore the original capital sum, and the mere statement of this possible reaction to death duties indicates how improbable it is. When there are many heirs, the tax along with the division of the property means that each heir receives only a minor fraction of the total; it seems even more improbable that there would be any disposition to attempt to refrain from consumption to restore a pretax capital sum. Death taxes should be regarded as especially burdensome on a nation's supply of capital; a redistribution of wealth through estate taxation can be achieved only by a reduction in national wealth.

A distinction should be drawn, as was done in the second preceding paragraph, between a capital gains tax on a shift in an investment

TAX REFORM

CAPITAL FORMATION AND THE USE OF CAPITAL*

By DAN THROOP SMITH
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A first step in the consideration of tax reform with reference to the formation and use of capital is an appraisal of our present tax system. The ways in which the tax law influences capital formation and use are so numerous and diverse that a mere listing and very brief description would absorb all of the available time. One must accordingly be selective. The approach here is qualitative; I shall be content to indicate the nature of the tax influences without any attempt at quantification.

Too frequently, the analysis of the effects of taxation has been somewhat too mechanistic; it has looked only at the availability of funds and differential rates of return. We need also to consider the impact of tax policies and even of the philosophies which they reflect on the attitudes of those who own and use capital; that is, on the willingness to use funds and to respond to differential rates of return. This aspect of the problem is discussed in the concluding paragraphs.

The most obvious influence of taxation on the formation of capital comes from our major reliance on the individual income tax and the progression of rates. Quite apart from any discouragement of effort and initiative, income taxation, in contrast to excise or consumption taxation, discourages savings because it treats consumption and savings equally. A tax on consumption to raise an equal revenue would shift the margin in favor of savings. This is a principal reason why expenditure taxation is especially recommended for newly developed countries in critical need of capital; it would also be suitable for other countries concerned with capital formation.

The degree of progression in an individual tax influences capital formation. Studies of the relative disposition to save and consume income at different income levels provide a basis for some confidence in appraising the significance of changes in the rate structure. It seems reasonable and probable that a highly progressive individual income tax especially restricts the amount of savings by falling most heavily

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I regret the fact that Professor Schultze felt he could not add anything more to the discussion of capacity and investment. While I too regard Frank de Leeuw's contribution as a major step forward, all would agree that even that highly original study can be improved upon. However, I must confess that I find attractive and fruitful Schultze's concept of permanent sales. It helps to explain, for example, the rate of business capital spending in recent quarters, and correspondingly suggests, in the absence of a recession in 1963, a greater potential than is generally recognized. I have little to add to Schultze's continuing and imaginative work on prices. His findings on the significance of capacity and the asymmetry in the deviations from capacity leave me all the more convinced that the next big step in theory will come from someone who can interweave output and stock in a variety of ways. We have, indeed, made considerable progress since the simple Harrod-Domar models of fifteen to twenty years ago.

Capacity is one of the most significant problems affecting the future growth of the nation, and one wonders why there is relatively so little work in the field. Once upon a time, there were literally thousands at work devising all sorts of consumption functions. Today we need the same kind of intensive investigation of capacity and capital stock for a better understanding of both cyclical developments and economic growth. I hope that this panel will serve to spark many new studies that will shed more light on this all-important subject.

in constant dollars for comparison with the private economy's total stock of capital. Further, an additional and we think more meaningful measure of output is included; namely, a ratchet calculation which represents the maximum monthly output for each successive two years for each of the almost one hundred subindustries published by the Federal Reserve Board and calculated using capital weights on a so-called "two-digit" basis.

How can one use measures of capital stock and output? One approach is to determine capital requirements for the future. On this basis, the projection cannot be too far in advance, say three years, since one cannot assume a stable capital-output ratio indefinitely ahead. To arrive at judgments on capital requirements, one must decide how much capital stock will be needed for a given increase in output, and this has to be done in detail. In addition, one must determine an appropriate time period of comparison. For example, the relationship of capital stock to output in early 1955 was followed by the great capital goods boom of 1955-57, and the burst in new capital appropriations in the first quarter of 1955 was the first sign of that boom. In making decisions about the future trend of capital stock, one has to make a determination about the trend of stock and output since, say, early 1955. If a projection of output for a year ahead, together with expenditures suggested by surveys of company plans, results in a capital-output ratio that is comparable to early 1955, one could argue that another major capital goods advance would develop. The same approach holds in making judgments about downward cyclical developments, and this is on the record in the pages of *Fortune* (January, 1961).

In addition, I must emphasize my judgment that the whole question of capacity cannot be restricted just to the manufacturing sector. After all, manufacturing capital outlays represent only about one-fourth of all outlays, and their stock represents about the same proportion of total business capital stock. In appraising the past and projecting the future, it would be foolhardy indeed to ignore the other three-fourths. In taking this larger approach, the capital-output method may well be the only one possible—at least outside the energy field. Here, too, you would have to build up stocks carefully, industry by industry (see *Fortune,* October and December, 1962), and make the relevant comparisons with output. The new output series prepared by the Commerce Department and published last October will of course be helpful.

Thus, it seems to me that we are not too far away from the time when we shall have carefully worked out capital stock and output figures, industry by industry, in manufacturing, in energy, and in the nonindustrial half of the capital goods sector. However, even then, I am not sure that we can easily overcome the incomparabilities in both capital and output, and these may plague us for a long time. Still, the capital-output ratios resulting from such a body of information would then be used to determine capital requirements in a growth economy and could also afford the basis for judgments about cyclical effects. In fact, this is precisely what we at *Fortune* are trying to do in a series of four articles, ending in February, 1963. Needless to say, we do not think these estimates are the final word, and we would welcome additional resources devoted to this all-important subject.

MORRIS COHEN: An important element in the failure thus far for more advancement in capacity measures is the comparative neglect that the balance sheet of the economy has suffered at the expense of the income approach. Despite many warnings over the past decade or so and a few pioneering studies, such as those by Goldsmith and Kuznets, the profession is still neglecting the empirical balance sheet. There is widespread, and glib, talk of "excess capacity," but only a few seem interested in coming to grips with this obviously significant problem. Surveys of plant and equipment expenditure expectations may or may not be useful for insertion into a GNP model, as, for example, in the University of Michigan's set of equations, but surely such a practice adds little to our knowledge of how capital spending decisions are made and why they change. Thus, Professor Phillips has performed yeoman service in presenting in a succinct manner the major available measures of capacity.

Basically, there have thus far been two approaches to capacity measurement. One is through surveys; the other through capital stock and its relationship to output; i.e., the capital-output ratio now so familiar to students of economics. While Professor Phillips criticizes the survey method which simply asks companies what their capacity is and what their operating rate is—and I cannot quarrel with his objections—nevertheless I do want to say a kind word for what has been done so far. I refer, of course, to the McGraw-Hill annual surveys of manufacturing capacity and rate of operations. Despite its limitations, this body of information is useful and serves as a measuring rod, especially when combined with other approaches, such as the capital-output ratio. Its great virtue, and simultaneously its great defect, is the failure to define precisely what capacity is. It is a virtue because you then get answers to survey questions; the defect comes in when you try to understand what the answers mean. If the Census Bureau is to ask capacity questions in the Census of Manufactures and define terms precisely, one wonders what kind of answers they will get and how the rate of response will be. Yet I, for one, believe that such an effort by the federal government would be useful; but we must recognize that the numbers obtained may leave open more questions than are answered. Since a time trend of capacity is more likely to develop from a survey approach rather than an absolute measure of capacity, it is clear that some years will have to pass before we can begin to use such Census results.

The important advance towards a better understanding of capacity will have to come through the capital-output approach. This is just another version of the stock-flow problem, and capital-output ratios, of course, represent an important link between the income statement and the balance sheet of the economy. The capital-output method has been further advanced by the new studies of the Department of Commerce published in the *Survey of Current Business,* November, 1962.

Capital stock by itself, of course, is only half the story. The other half is output. Here I must explain that *Fortune's* measure of output is a value-added one; namely, the Federal Reserve Board index of manufacturing production for comparison with manufacturing capital stock, and private GNP

How this and other questions are answered will determine the concept and the measurement of capacity. And since the questions can be, and have been, answered differently, a variety of concepts and measurements appear even among economists with the same purpose in mind.

Perhaps the more interesting use of the idea of capacity is in assessing the efficiency of the economy; that is, comparing actual output with the output that might be—a hypothetical output. Here, too, differences arise in definition and measurement.

Hypothetical output is, of course, a forecast, and we all know how widely forecasts can differ among economists. Indeed, the same economist can, and perhaps should, present alternative forecasts. The forecast—the hypothetical output—depends, first, on what conditions are assumed; and, second, on what relation between these conditions and the volume of output is assumed. Put simply, in terms of the output supplied by an industry, for example, the supply that will be offered if price should change depends on the change in price and the shape of the supply curve.

Now, economists differ with respect to the conditions, or changes in conditions, that they believe to be worth postulating and which they in fact do postulate, when they talk about capacity output and efficiency. And because economic knowledge is limited, economists have different opinions also about the effects on output of a shift to the postulated conditions. When one economist says that the United States could be producing 10 per cent more than it is, he is saying that if certain (often unspecified) changes in policy were to be made, output would rise by 10 per cent as a result. Another economist —or the same economist—might assume different changes in policy and also assume different connections between particular policies and output. Further, the output projected upon given changes in policy will depend on the period of time allowed for the new policy to work itself out. There will be more than one forecast of output. That is, there will be more than one estimate of capacity.

Sometimes we try to skirt all these differences by agreeing to define capacity as previous peak output. But this is, in effect, an attempt to reach a consensus on the conditions assumed, without actually specifying them; and to reach a measure of agreement on the shape of the relation between conditions and hypothetical output, also without actually specifying it. Are we really avoiding the problems when we do this? That we are not, we recognize when the previous peak is left some distance behind and we must extrapolate something other than a horizontal trend in capacity.

The idea of capacity is important. People struggling with economic and business problems do not seem able to get along without the notion. It is therefore worth learning more about it. We need more work of the kind Phillips and the men he has referred to have done on the meaning and measurement of capacity. We need more work of the kind Schultze and those he has referred to have done using capacity as a variable in their equations. The work on both fronts will move ahead more rapidly if the Census adds to our statistical data. But, to repeat, the time is not yet ripe for official capacity indexes.

DISCUSSION

Solomon Fabricant: Mr. Phillips has given us a useful critique of the several capacity measurements currently available. After raising some interesting and important questions, he concludes that the time is about right for establishing a capacity series as part of the federal statistical program. After listening to his recital, however, I find it difficult to join him in his conclusion. I am not as confident that the time is right. Much more still needs to be done to prepare the way for an official capacity series. The Bureau of the Census and other government departments can do a good deal to help in this preparatory work by providing information now lacking or by improving existing data. But more and better information is one thing; an official capacity series is quite another.

As for Mr. Schultze's paper, each section raises many points that would be worth pursuing, but I must focus on the subject of this session and address my question to it. Which of the many rather different concepts and measurements of capacity would fit his functions best? Or would something other than capacity fit as well or better? Or would one concept of capacity be best for one question and a different concept of capacity be best for another function? Or would it make no difference which capacity concept were used? I would be surprised if the various capacity measurements were very good substitutes for one another, since differences among them—especially when they are expressed as percentages of capacity not utilized—are very considerable. This is not to say that it would be easy to choose among the several concepts on the basis of "goodness of fit" measured in the ordinary way. I expect all the correlation coefficients would be high. But my question is worth struggling with.

The variety of capacity concepts and measurements is troublesome, and it is natural, therefore, to ask why there should be such a variety.

One reason is that we are trying to answer more than one question. Let me mention two. Sometimes we ask: What is the "unemployment rate," so to speak, of tangible capital goods? Sometimes we ask: How does current output compare with the hypothetical output that would be produced if conditions were different? That these are different questions has been indicated by Phillips. They suggest the need for different concepts and measurements of capacity.

In dealing with the unemployment rate of tangible capital goods, we are up against all—and more than all—of the difficulties that bother us when we attempt to measure the unemployment rate of men. Conventions in the measurement of labor unemployment help us to blink our eyes to these difficulties. But we are reminded of the difficulties when we turn to the unemployment of capital goods. To illustrate: if 4 per cent unemployment of the labor force equals "full" employment, what is the analogous figure when we are concerned with the unemployment of capital goods?

service life he may assign to assets then to be acquired will depend on whether he adequately accelerated retirements and replacements during the prior period. If he failed to do so, he will find the reserve ratio test an impediment rather than an aid in assigning to his new assets a service life no longer, let alone shorter, than that he has previously been using, no matter how confident he is of its correctness. But since there is no necessary or inherent relationship between the longevity of assets acquired in the past and those about to be acquired, there does not appear to be any compelling logic in basing service lives on retirement-replacement patterns.

The fundamental difficulty in the use both of guidelines (even if regularly and frequently revised) and devices such as the reserve ratio test is that they are oriented to *ex ante* determination of service lives. In fact, an asset's period of usefulness can only be satisfactorily determined *ex post,* when it is retired. Since the importance of precise determination of service lives is by no means evident, it is unclear whether the drastic changes in depreciation policy which would be required for *ex post* determinations are worthy of serious consideration. The fact that the new provisions may fall considerably short of the claims made for them as providing for realistic determination of service lives, therefore, may well be a minor matter in their evaluation.[8]

2. *The New Provisions as a Stimulant for Private Capital Formation.* A major benefit ascribed to the new depreciation provisions, particularly in conjunction with the investment tax credit, is the stimulus they afford for increasing investment in plant and equipment. This stimulus results from the increase in the rate of return realizable on a given capital outlay and the greater cash flow generated with respect to both existing assets and additions to the stock of depreciable facilities.[9]

Cash Flow Effect. The extent of the increase in business cash flow is indicated by the Treasury's estimate of the loss in revenue when both the credit and the new depreciation provisions are applicable for a full taxable year. The investment credit is expected to generate roughly $1 billion and the depreciation revisions about $1.5 billion in additional internal funds to business income taxpayers. In the aggregate, this $2.5 billion in tax reduction is a substantial easing of business income tax burdens. Allocating about five-sixths of the total to corporations, the corporate tax savings are roughly those which would result from a 3.75 percentage point—or a 7¼ per cent reduction—in the present combined top marginal rate of 52 per cent on corporate taxable income (on the basis of first and second quarter 1962 corporate profits, seasonally adjusted at annual rates).

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[8] The reserve ratio test does objectively and correctly measure whether the taxpayer has been retaining assets no longer than the time equal to the service life he has been using. The test, therefore, might be a useful device for computing the amount of excess depreciation claimed in the past with respect to assets still in use but with no remaining depreciable basis, assuming the government intended to assess interest on the tax which was deferred thereby. If free depreciation were adopted, some such test would be essential for providing systematic recapitulation of depreciation accounts to assure their realism.

[9] In a release accompanying issuance of the Revenue Procedure, the favorable effect of the new depreciation guidelines upon investment was attributed to increased cash flow, shortening of the pay-out period, and the resulting reduction in investment risk. The shortening of the pay-out period resulting from use of the new service lives is in the same proportion as the reduction in service life; on the average, this would be about 15 per cent in manufacturing.

For corporations alone, the Treasury estimated the full-year effect of the depreciation changes, at 1962 income levels, at about $1.25 billion. On the basis of a rough and ready estimate of $55 billion for the profits of corporations with taxable profits in 1962,[10] this reduction in tax liabilities is about the same as that which would result from a 2¼ percentage point reduction in the corporation tax rate. Of the total revenue loss, the Treasury estimated that $740 million would be accounted for by manufacturing corporations. This amount is roughly that which would be lost by a 2.6 percentage point cut in the tax rate applicable to taxable profits of manufacturing corporations. Within manufacturing, tax rate cuts producing the same revenue loss as the new depreciation provisions range from a low of about a .5 percentage points in tobacco manufacturing to a high of about 5.33 percentage points in the case of paper and allied products and 6.5 percentage points in primary metals. For all nonmanufacturing corporations, the new depreciation provisions are estimated to have a revenue effect roughly equivalent to a 2.5 percentage point reduction in the corporate rate. Equivalent rate cuts range from a low of .5 percentage point in the case of finance to a high of 11.5 percentage points in the case of transportation and 21¼ percentage points in agriculture.[11]

Rate-of-return Effects. The combined effect of the new depreciation provisions and the investment credit on the rate of return realizable upon a given amount of capital outlays may be quite substantial. George Terborgh, for example, has calculated that these tax changes in combination would raise the after-tax rate of return on an asset with a service life of eight years (before the new guidelines) from 10 per cent to 12.5 per cent. For assets with service lives of ten and fifteen years, the increase in rate of return averages about 2 points—or 20 per cent. About three-fourths of this increase is attributable to the credit.[12]

Estimated Increase in Capital Outlays. How much of an increase in capital outlays (expenditures for fixed capital) are these cash flow and rate-of-return effects of the tax changes likely to generate? The answer to this question depends on the extent of the increase in the supply of investable funds resulting from the increased cash flow and upon the elasticities of investment demand and the supply of funds. The amount of the increase in investable funds depends on a number of factors, among which are the extent of the monetary expansion undertaken to finance the government's deficit and the degree of substitutability, as seen by business, between internally-generated and external funds. Thus, if there is no increase in the money supply and if there is considerable substitutability between internal and external funds, the increase in cash flow is likely to be offset at least in part by reduction in the availability of external funds. On the other hand, if the deficit is wholly financed by an increase in the money supply and/or if there is no substitutability, in the short run, between internal and external funds, the increase in the supply of investable funds is likely to equal the increase in cash flow. Given the increases in the supply of funds and investment demand, the extent of the increase in capital outlays depends on the respective elasticities of these schedules. Rough calculations, assuming alternative sets of supply and demand elasticities and a $2.5 billion increase in the supply of funds, indicate that the initial increase in capital outlays is likely to be quite modest, except when the respective elasticities are assumed to be very high. With demand elasticities of $-.25$ and $-.5$ and supply elasticities of .5 and 1.0, the calculated initial increase in capital outlays ranges from $1.3 billion to $2.2 billion. This represents an increase of between 3½ to 6 per cent in the amount of new plant and equipment outlays expected in 1962. Making a rough allowance for the amount of such outlays which represents replacements, the calculated increases in investment would represent an increase in the rate of net additions to the stock of plant and equipment of between roughly 10 to 18 per cent. The following table summarizes these estimates.[13]

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[10] This amount is significantly less than the profits of taxable corporations which would be consistent with the corporate profits estimated in the January 1962 Budget Message, upon which presumably the Treasury estimates were based.

[11] See appendix Table 2 for the details of these estimates. Regrettably, no distribution by industry of the revenue loss from the investment tax credit is available.

[12] Cf. George Terborgh, *New Investment Incentives, The Investment Credit and The New Depreciation System* (Machinery and Allied Products Institute), pp. 11, 12.

Presumably, further increases in capital outlays would be induced by the expansion of aggregate demand resulting from the initial increase in expenditures for fixed capital. Taking these feedback effects into account, the full effect of the tax changes on outlays for fixed capital, roughly estimated, ranges from $2.2 billion to $3.2 billion.

3. *Effects on Modernization of Plant and Equipment Stocks.* A further aim of the investment credit and the new depreciation rules is to encourage more rapid replacement of obsolete facilities by technically advanced plant and equipment. These tax changes will accom-

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[13] These estimates ignore the possibility that anticipations might shift favorably as a result of the tax changes. If this effect is deemed to be consequential, capital outlays might well expand substantially more than indicated here.

INCREASE IN CAPITAL OUTLAYS BY ELASTICITIES OF INVESTMENT DEMAND AND OF SUPPLY OF INVESTABLE FUNDS[14]
(Billions of Dollars)

Elasticity of Supply	Elasticity of Demand		
	−.25	−.5	−1.0
.5	1.5	2.2	2.9
1.0	1.3	2.1	3.1
∞	.9	1.8	3.8

plish this result, presumably, both by inducing a greater volume of capital outlays and reducing the period of time over which facilities are held.

The effect of an increase in capital outlays on the average age of the capital stock depends, in part, on the extent to which this increase is matched by additional retirements; i.e., the greater the increase in retirements relative to the increase in gross additions, the greater will be the reduction in the average age of the stock. Of itself, an increase in capital additions of the order of magnitude suggested above can have very little effect in the short run on the average age of the capital stock. A $3 billion increase in capital outlays would represent an addition to the existing gross stock of something less than ½ of 1 per cent.[15] Using the Office of Business Economics estimate of the mean age of the stock of fixed business capital at the end of 1961, this addition would reduce the average age by about 2½ weeks.[16]

If, however, the reduction in service lives afforded by the new guidelines results in a significant acceleration of retirements and replacements, the reduction in the average age of the capital stock might well be considerably greater. Suppose taxpayers generally and promptly retire and replace those of their present facilities which are older than the applicable new guideline lives. In this event, the average age of the stock would be reduced not only by a considerably larger increase in capital outlays than that estimated above but also by the larger volume of retirements of older assets. The extent of the initial (first-year) effect on the average age of the stock would, of course, depend on the age distribution of the present stock. Thus, the initial reduction would be relatively great among industries in which there is a preponderance of older facilities and relatively slight in industries in which there is a preponderance of new assets.[17]

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[14] See appendix for calculations. In making these estimates, an effective credit rate of 3 per cent, in lieu of the statutory 7 per cent, has been used. The 3 per cent rate conforms closely with the Treasury's estimate of the revenue loss attributable to the credit. A more generous credit would, of course, result in a larger increase in capital outlays, but this increase would be a little less than proportional to the additional revenue loss.

[15] An additional $3 billion of capital outlays (current prices) is roughly $2.5 billion in 1954 dollars. The latter figure may be compared with the Office of Business Economics estimate of the gross stock of fixed business capital at the end of 1961 of $532 billion (1954 dollars). Cf. U. S. Department of Commerce, Office of Business Economics, *Survey of Current Business*, Nov., 1962, p. 11.

[16] *Ibid.*

There is little solid information on the age distribution of the existing stock of depreciable facilities. According to recent Department of Commerce estimates the average age of total manufacturing stocks (gross) in 1961 was 8½ years, while that of equipment was 6.2 years. This is just under 50 per cent of the new average guideline life for manufacturing machinery and equipment.[18] If a relatively normal distribution around this mean is assumed, a relatively small proportion of existing assets are older than the new guideline life.

In any case, the assumption that taxpayers generally will promptly replace all existing assets older than the newly applicable guideline lives is clearly extreme. Replacements, presumably, will be determined on the basis of a comparison of the variable unit costs of production with the existing assets relative to the total unit costs with new assets. The new tax provisions do indeed reduce the total unit costs of output with new assets relative to variable unit costs with existing facilities, but whether this reduction and the consequent increase in replacements will be relatively great or relatively slight for most taxpayers cannot be confidently estimated.

4. *Simplification of Depreciation Accounting.* The effects of the new depreciation provisions on the costs of compliance may well prove to be mixed. On the one hand, the broad groupings of facilities authorized by the new guidelines materially reduces the accounting detail required for tax purposes. In addition, the use of the reserve ratio test should have the desirable effect of reducing taxpayer-revenue agent negotiations over service lives. On the other hand, the reserve ratio test may impose additional burdens on the accounting staffs of many business income taxpayers.

Presumably, the tables published by the IRS should make the application of the reserve ratio test quite simple and routine. In fact, however, there are several sets of circumstances in which the tables will not be usable, requiring the taxpayer to rely on the formula for determining the proper ratio with which his actual ratio is to be compared. For example, if there are discontinuities in the growth of the asset account over a guideline life period, the taxpayer's actual reserve ratio may fluctuate substantially above and below the range shown in the table. Or if the taxpayer's retirements and replacements bunch in one or a few years within the test period, the actual reserve ratio may well fall outside the range in the table.[19] Or the growth rate over the test period may exceed the highest rate shown in the table. In any of these cases, the taxpayer wishing to use the reserve ratio test to justify the service life he's assigned his assets will have to compute the appropriate ratio in order to demonstrate that his actual reserve ratio is not out of line. While the computation is not particularly difficult, it may be laborious.

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[17] Thereafter, during the first half of the new replacement cycle, the average age of the stock will tend to continue to fall, but more gradually, among the former industries, but will tend to rise among the latter industries. During the second half of the new replacement cycle, these patterns will tend to reverse.

[18] *Ibid.*, pp. 17-18. The OBE estimates of the age of these stocks were based on alternative assumptions concerning the longevity of the various classes of assets involved; the accuracy, therefore, depends in part on the validity of those assumptions.

Summary and Conclusions

The preceding discussion has attempted to evaluate the new depreciation provisions in the light of certain objectives which are thought to be widely endorsed. In very summary terms, this appraisal suggests that: (1) the new guideline lives conform more closely than those in Bulletin F with the actual tax practice of many taxpayers and will anticipate for some little while to come the assumed continuing shortening of useful lives resulting from the accelerating pace of obsolescence; (2) the reserve ratio test affords the taxpayer greater freedom in assigning service lives to his facilities, provided he conforms replacement practices to such assignments, but it is by no means clear that the test will materially aid in determining realistic service lives; (3) the more liberal depreciation allowances combined with the investment credit should induce a modest increase in the volume of capital outlays, at current levels of income and with current anticipations of capacity requirements; (4) the new provisions are likely to make some contribution toward modernization of the stock of depreciable facilities but there is little basis for estimating the size of this effect; and (5) so far as compliance burdens are concerned, the new provisions are a mixed blessing: the much broader groupings of assets for purposes of assigning service lives should certainly ease compliance, but the use of the reserve ratio test is likely to be burdensome for many companies in quite normal circumstances.

These accomplishments are not inconsiderable, although they may not fully justify the enthusiasm expressed in their behalf. Their failure to do so, however, reflects a widespread penchant for attributing greater weight to tax considerations as determinants of business behavior than has yet been established by careful empirical inquiry.

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[19] Indeed, it is extremely difficult to conceive the set of circumstances in which some considerable bunching of retirements and replacements will not occur, even if the constraint of a continuous growth function is met.

APPENDIX

TABLE 1

COMPARISON OF SUGGESTED SERVICE LIVES UNDER BULLETIN F, NEW GUIDELINES, AND CURRENT PRACTICE

MANUFACTURING INDUSTRY	PRODUCTION MACHINERY AND EQUIPMENT				
	Bulletin F Composite (Years)	New Guidelines (Years)	Decrease from Midpoint of Bulletin F to the New Guidelines	Actual Current Practice (Years)	Decrease from Current Practice to the New Guidelines
Aerospace	15*	8	47%	10	20%
Apparel and fabricated textile products	15–30†	9	60	15	40
Chemicals and allied products	15–22	11	40	13	15
Electrical equipment	17–20	8–12	35	15	33
Fabricated metal products	14–28	12	43	16	25
Food and kindred products	12.5–40	12–18	21–38	13–19	8–54
Lumber, wood products, and furniture	10–25	10	43	16	38
Machinery except electrical machinery, metalworking machinery and transportation equipment	10–28	12	37	15	20
Metalworking machinery	17–20	12	35	16	25
Motor vehicles and parts	15–20‡	12	31	14	14
Paper and allied products	15–28	12–16	29–35	19	21
Petroleum and natural gas	5–33†	6–16	7–29	16–19	11–16
Primary metals	17–30†	14–18	28–40	23	22
Printing and publishing	10–25	11	37	16	32
Professional, scientific and controlling instruments; photographic and optical equipment; watches and clocks	17–25	12	43	15	20
Railroad equipment	25–28	12	55	16	25
Rubber, leather, and plastic products	15–17§	11–14	18–27	15	13
Ship and boat building	20–25	12	47	19	37
Stone, clay and glass products	15–40†	14–20	7–46	19	16
Textile and mill products	15–25	9–15	40–44**	17	18
Tobacco and tobacco products	15–20	15	14	17	12
Other manufacturing	††	12	††	14	14
All manufacturing	19	13	32	15	15‡‡

* Except small tools 4–5 years.

† Items lives only.

‡ Special jigs, dies, patterns 3–4 years.

§ Except the mold account 3 years.

** Bulletin F compared with an average guideline of 14 years.

†† No Bulletin F life.

‡‡ The average life of production machinery and equipment reported in the Treasury Depreciation Survey of 1959 was 15.2 years. The effect of the new guideline lives set forth in Part I of Revenue Procedure 62-21 will be to reduce the average life of such facilities for those corporations to approximately 12 years. This average life is somewhat below the average of new guidelines because some taxpayers have already justified even shorter lives than the new guidelines and will continue to use those shorter lives. The effective percentage reduction in lives for Treasury Survey corporations is, therefore, 21 per cent.

SOURCE: U. S. Treasury Department, Office of Tax Analysis, Release, July 10, 1962.

The Reserve Ratio Test

Derivation of the Reserve Ratio. The calculation aims at determining what the ratio of accumulated depreciation reserves to gross assets should be for a depreciation account which is homogeneous with respect to assigned service life, given the method of depreciation and the rate of growth of the gross assets, if the taxpayer has on the average been retaining the assets in use (and in the account) over a period equal to the assigned service life.[1]

The first step is calculation of the appropriate reserve ratio for the assets acquired a given number of years ago. The amount of annual depreciation per dollar of asset cost depends on the depreciation method used. With a service life of N years, annual depreciation per dollar of asset is:

(1) $1/N$ in the case of straight-line depreciation;

(2) $2/N$ in the case of double declining balance depreciation in the first full year, $2/N(1-2/N)$ in the second year,

$$2/N[1-2/N-2/N(1-2/N)]$$

in the third year, etc.;

(3) $1.5/N$ in the case of 150 per cent declining balance in the first year, $1.5/N(1-1.5/N)$ in the second year,

$$1.5/N[1-1.5/N-1.5/N(1-1.5/N)]$$

in the third year, etc.;

(4) $2N[N(N+1)]$ in the case of sum of the years-digits in the first year, $2(N-1)[N(N+1)]$ in the second year, $2(N-2)[N(N+1)]$ in the third year, etc.

The accumulated depreciation per dollar of asset cost k years after the end of the year in which the asset was acquired, i.e., $R_j(k, N)$, assuming the taxpayer follows the convention of taking a half year's depreciation on assets acquired during the year, is:

$$R_1(k, N) = \frac{2k-1}{2N}, \tag{1}$$

where the straight-line method is used;

$$R_2(k, N) = 1 - (1 - 1/N)(1 - 2/N)^{k-1}, \tag{2}$$

where the double declining balance method is used;

$$R_3(k, N) = 1 - \left(1 - \frac{.75}{N}\right)\left(1 - \frac{1.5}{N}\right)^{k-1} \tag{3}$$

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[1] In general, the regulations require the taxpayer to keep an asset in the account, even if fully depreciated, until it is actually retired or disposed of.

where the 150 per cent declining balance method is used; and

$$R_4(k, N) = \frac{(2k-1)(N+1) - k^2}{N(N+1)}, \tag{4}$$

where the sum of the years-digits method is used.

The next step is to calculate the ratio of accumulated depreciation reserves to gross assets when the fact that assets of a given service life have been added in more than a single year is taken into account. Thus, the reserve ratio in any given year is the weighted average in that year of the reserve ratios with respect to each prior year's acquisitions. The weight applied to the reserve ratio for each year's acquisition is the amount of such acquisition.

For example, let A_N = the amount of assets acquired N years ago [$A_{(N-1)}$ = the amount of assets acquired $N-1$ years ago, etc.], and R_N = the ratio of accumulated depreciation reserves to the amount of assets acquired N years ago [$R_{(N-1)}$ = the reserve ratio for assets acquired $N-1$ years ago, etc.]. The weighted average of these reserve ratios, when assets have been acquired over the previous T years is:

$$W_{j,T}(N) = \frac{A_N R_N + A_{N-1} R_{N-1} + \cdots + A_{N-(N-1)} R_{N-(N-1)}}{A_N + A_{N-1} + \cdots + A_{N-(N-1)}},$$

where the subscript j designates the depreciation method used.[2]

If each year's acquisitions are a constant percent, i, more (or less) than the preceding year's acquisitions, $A_{(N-1)} = A_N(1+i)$, $A_{(N-2)} = A_N(1+i)^2$, etc., and the preceding expression becomes

$$W_{j,T}(i, N)$$

$$= \frac{A_N R_N + A_N(1+i) R_{N-1} + A_N(1+i)^2 R_{N-2} + \cdots + A_N(1+i)^{N-1} R_{N-(N-1)}}{A_N + A_N(1+i) + A_N(1+i)^2 + \cdots + A_N(1+i)^{N-1}}$$

$$= \frac{R_N + R_{N-1}(1+i) + R_{N-2}(1+i)^2 + \cdots + R_{N-(N-1)}(1+i)^{N-1}}{1 + (1+i) + (1+i)^2 + \cdots + (1+i)^{N-1}}$$

In a summary expression, this may be written as:

$$W_{j,T}(i, N) = \frac{\sum_{k=1}^{T} R_j(k, N)(1+i)^{N-k}}{\sum_{k=1}^{T} (1+i)^{N-k}}.$$

This is the expression as published in the appendix to the Revenue Procedures.

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[2] If T is less than N, i.e., if a period of years equal to the service life has not yet elapsed, then the value of those terms in both numerator and denominator for which the subscript is more than T is zero.

Constraints on the Use of the Reserve Ratio Tables. The values in the published tables of reserve ratios corresponding to given service lives and given growth rates were computed by the use of the preceding formula. One of the basic assumptions, explicit in the formula and implicit in the published values, is that growth in the asset account is continuous; i.e., that each year's acquisitions are a constant per cent more (or less) than the preceding years or that the amount of the assets in the account at the end of each year is a given percentage more (or less) than at the end of the preceding year. This is, of course, an unrealistic assumption and one which the taxpayer is not asked explicitly to make. For purposes of determining the rate of growth of an account, the taxpayer is directed to take the ratio of the amount of assets in the account at the end of the current taxable year to the amount at the end of the taxable year N years ago and to find the growth rate, i, which gives this ratio for N years in a compound interest table. The growth rate determination he is directed to make, in other words, is a base-terminal year compound interest calculation. But reference to the formula in its unsummarized form quickly shows that if in fact the rate of growth of the account has not been perfectly even in each year, which is the likeliest assumption, the value of the weighted average reserve ratio will differ from that in the published tables. The range of reserve ratio values for any given N and i in the published tables may compensate for this variance in some, but hardly in all, cases.

A collateral constraint on the use of the published tables of reserve ratios is that these are based on the unrealistic assumption of an even time distribution of retirements and replacements. If retirements and replacements in fact bunch in a particular year or years, the taxpayer's actual reserve ratio will tend to fluctuate substantially above and below the published values over the course of a replacement cycle. The bunching of retirements and replacements, of course, does not necessarily imply that the taxpayer's replacement practices do not conform with the service life he has assigned the assets. Indeed, retirement and replacement of the taxpayer's original asset acquisitions will necessarily involve relatively pronounced bunching at least in the first few replacement cycles, and this bunching will be more pronounced the more nearly constant the year-to-year growth of total facilities after the original acquisition.

In addition, the formulation has not taken into account the fact that the service life is the mean of a distribution of periods of time over which assets acquired in any year will be retained in use. Differences in the shape of the distribution from which this mean is derived may also result in deviations of actual reserve ratios from those published in the tables.

TABLE 2

CORPORATE INCOME TAX RATE REDUCTION YIELDING REVENUE LOSS EQUAL TO THAT RESULTING FROM NEW DEPRECIATION PROVISIONS, BY INDUSTRY[*]

Industry	Reduction in Corporate Tax Liability Resulting from New Depreciation Provisions (Millions of Dollars)	Corporate Rate Reduction Yielding Equal Revenue Loss[†] (Percentage Points)
Manufacturing total	745	2.6
Food and beverages	60	2.4
Tobacco	2.5	.4
Textile mill products	20	2.6
Apparel	2.5	.7
Lumber and furniture	20	3.4
Paper and allied products	60	5.4
Printing and publishing	20	2.1
Chemicals	110	3.1
Petroleum, coal products and crude petroleum	120	3.7
Rubber and leather	10	1.3
Stone, clay, and glass products	40	4.3
Metals		
Primary	120	6.5
Fabricated	40	2.9
Scientific instruments	2.5	.3
Machinery except electrical	30	1.2
Electrical machinery	30	1.5
Transportation equipment except autos	10	1.1
Automobiles	40	1.1
Miscellaneous	—	—
Agriculture	10	21.3
Mining	40	7.6
Construction	20	3.1
Wholesale and retail trade	90	1.4
Finance	30	.5
Transportation	160	11.4
Public utilities	120	3.1
Services	40	3.3
Total	1,255	2.3

[*] Revenue reductions were estimated by the Treasury as the full year effects of the new provisions at 1962 income levels; i.e., before any "feedback" effects on national income resulting from induced increases in capital outlays. The income levels used in these estimates were those in the January, 1962, Budget Message and I have, therefore, revised them downwards on the basis of later data. Thus, the corporate profits estimates used in my calculations are first and second quarter, 1962, seasonally adjusted at annual rates. The effect of this revision on the estimated revenue losses from use of the new depreciation provisions, however, is slight. On the other hand, downward revision of corporate profits increases the estimated corporate rate reduction yielding equivalent revenue losses (see footnote †). The estimated revenue loss resulting from the new depreciation provisions assumes that not all taxpayers will take full advantage of the new guideline lives. If all taxpayers were to do so, the corporate tax revenue loss would be about $1.7 billion.

[†] In calculating these reductions, estimated corporate profits (first and second quarters, 1962, seasonally adjusted at annual rates) were adjusted to obtain estimated profits of corporations with taxable profits by applying the ratios of profits on taxable returns to total profits (i.e., profits less deficits) derived from 1959 Statistics of Income data. The quotient of the estimated reduction in tax liabilities, resulting from the new depreciation provisions, divided by adjusted corporate profits is the number of percentage points reduction in the corporate tax rate applicable to taxable profits which would yield the same revenue loss.

SOURCE: U. S. Treasury Department, Office of Tax Analysis, and unpublished data provided by Office of Business Economics, U. S. Department of Commerce.

Where any of these constraints are operative or where the growth rate exceeds the highest rate shown in the published tables, the taxpayer may have to rely on the formula, modified to take into account his actual patterns of growth and retirements and replacements. If, on the average, he has been retaining assets in use for a period equal to the assigned service life, the modified formula will yield a reserve ratio very close to his actual reserve ratio.

Calculation of Combined Effect of the Investment Tax Credit and the New Depreciation Provisions on the Volume of Capital Outlays

The impact of these new provisions on capital outlays may be viewed as depending upon (1) the extent to which they increase investment demand and the supply of funds available for financing capital outlays and (2) the respective elasticities of these curves.

In this calculation, it has been assumed that the supply of funds would be increased by the full amount of the estimated revenue loss; i.e., at any stipulated interest rate, the amount of funds available before these tax changes would be increased by $2.5 billion.[3] The relevant portion of the supply of funds curve before the tax change was computed by use of the equation $Q_s = k_s r^{\eta_s}$, where alternative values of .5, 1.0 and ∞ were assigned to η_s. The starting point used in drawing the curve had ordinates of $Q_s = \$37$ billion and $r = .08$. The supply of funds curve after the tax change, therefore, was taken as $Q_s = k_s r^{\eta_s} + 2.5$.

The relevant portion of the investment demand curve, before the tax changes, was computed from the equation $r_1 = (k_d/Q_d)^1/\eta_d$, in which values of .25, .5, and 1.0 were assigned to η_d. The ordinates of the starting point of the curve were, again, $Q_d = \$37$ billion and $r = .08$. The demand curve after the tax changes was computed as follows:

First, the equation

(1) $C = A^N_{r_1}[Y(1-t) + tD_1]$ where

C = the cost of the marginal asset;

$A^N_{r_1}$ = the present value of an income stream at r_1 per cent for N years, when N is the expected period of usefulness of the asset to the taxpayer and is taken to be equal to the asset's service life for tax depreciation purposes before the new depreciation provisions;

Y = the annual proceeds, before deduction of depreciation, attributable to use of the asset;

t = the effective rate of tax; and

D_1 = the annual depreciation deduction on the asset, using straight-line depreciation[4] was solved for Y, by assuming varying values of r_1, i.e., given C, N and t, a specific value of Y was determined for each value of r_1, corresponding to various points on the demand curve before the tax changes. After the tax changes, and holding each of the Y's unchanged, the following equation was solved for r_2:

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[3] At estimated 1962 levels of income and stock of assets. This increase, therefore, ignores the effect on the supply of funds of any responses to the tax changes.

[4] Use of declining balance or sum of the years-digits depreciation instead of straight line has little effect on the extent of the increase in the investment demand schedule.

(2) $C = A^{N_1}_{r_2} Y(1-t) + A^{N_2}_{r_2} t D_2 + .03C$, where

$N_1 = N$ in equation (1);

$N_2 =$ the service life of the asset for tax purposes after the new depreciation provisions; and

$.03C$—the investment tax credit.[5]

Each of the r_2's so determined corresponds, therefore, to a specific r_1 on the demand curve before the tax changes and accordingly generates a new demand curve after the tax changes. It was found that over a considerable range of values for r_1, the corresponding values of r_2 averaged .0074 percentage points more.[6] The new demand equation, therefore, was $r_2 = (k_d/Q_d)^{1/\eta_d} + .0074$.

[5] The coefficient .03 takes into account the fact that some capital outlays are ineligible for the credit, some are eligible for the credit at a reduced rate, and in some cases the amount of the credit will be otherwise limited.

[6] The dispersion around this mean value of r_2-r_1 was very small, ± .0009.

DISCUSSION

John F. Due: These comments are confined to the Smith and Musgrave papers. Together the papers place in sharp focus the major contemporary issues on the question of appropriate fiscal policy for encouraging economic growth, particularly in developed economies, although in part the analysis applies to underdeveloped economies as well.

The first issue is that of whether the prime obstacle to more rapid economic growth is an inadequacy of savings at full employment or an inadequacy of investment necessary to allow attainment of the potential rate of capital formation as permitted by the volume of savings at full employment. In other words, should primary attention be given to increasing the potential growth rate or to ensuring that the actual growth rate approximates the potential rate? Musgrave stresses the latter, and his position appears to be the most reasonable one. Smith in a concluding paragraph accepts this point of view as well. But in most of his paper he stresses the savings considerations, as for example in his discussion of direct versus indirect taxes, the significance of estate and gift taxes, and other questions. He condemns estate taxes on the grounds that they are borne almost entirely out of savings and neglects the obviously lesser adverse effect on investment incentives.

A second and related issue is that of whether investment is restricted below levels regarded as optimum primarily by taxes, such as the corporate income tax, which, if not shifted, directly reduce the net return from investment, or primarily by inadequate demand for the products made with the investment goods. Smith of course stresses the former, when, for example, he argues for increased use of indirect taxes. This point of view, based in part on a very questionable analogy from European experience, has become very widespread in the last year; it is argued that greater use of indirect taxes, by placing more of the tax burden on consumption expenditures and less on the return from investment, would increase both the rate of savings and the incentive to undertake investment. But if the main obstacle to increased investment is lack of adequate consumer demand, any shifting of tax burden from savings to consumption would have precisely the wrong effect.

A third major issue is that of the conflict between considerations of equity and those relating to economic growth. Regardless of the merits of the two points of view on the question of the prime determinants of the level of investment, it would appear rather obvious that certain types of tax concessions, present or possible, affecting the return on capital should encourage investment, particularly by reducing the risk element. At the same time they violate usually accepted standards of equity. This conflict has been made more clear in the Musgrave paper than in any material which has previously appeared, and there is little which can be added to his excellent presentation. It must be stressed, of course, that equity considerations are in all instances value judgments; there are no "absolutes" or scientific standards in this field. But widely accepted standards are obviously violated by the various concessions, such as favorable treatment of capital gains. Thus decision about the desirability of the concessions requires another set of value judgments: the weighing of the equity considerations against the gains from greater economic growth.

Another issue is that of the best source of additional savings at full employment, should this be accepted as an objective. To Smith, increased individual propensity to save, to be attained by tax changes such as reduced reliance on direct taxes, elimination of the corporate income tax, less progression in the personal tax, and the like, represents the only acceptable approach. To Musgrave, this approach is unsatisfactory, primarily because of the magnitude of the tax changes which would be needed and their significance for equity.

But Musgrave's proposal—increased budget surpluses, and thus a sort of forced saving through governmental action—is open to dangers, particularly the effects of the higher tax rates required to produce the surplus on the problem of maintaining full employment, as Musgrave notes. The basic issue may be that we will be fortunate to hold the actual rate of growth to the potential rate with the present full employment savings ratio, let alone with a higher savings ratio.

A final issue concerns the appropriate role of the corporation income tax. Any evaluation of this tax depends, of course, upon the extent to which it may be shifted off of corporate earnings. If it is not shifted, the traditional arguments against it on both equity and investment-incentive grounds have merit; if it is, the tax can be criticized on the same grounds as other "indirect" taxes. Musgrave is inclining more and more to the shifting doctrine, primarily because of additional empirical evidence developed in another study on which he is working. Accordingly, he regards the tax as less objectionable on growth grounds, however questionable it may be on equity grounds.

Smith, assuming that in large measure the tax is not shifted, argues that it is a major obstacle in the path of efficient economic growth and advocates its replacement by a value-added tax. He claims that the present tax penalizes corporate efficiency and results in the transfer of control of resources from the more efficient firms to the less efficient ones. This statement has meaning of any sort only to the extent to which the tax is not shifted, and even then has only limited meaning. To the extent to which the sums taken in taxation would not have been used for expansion, there is no shifting of actual command of resources away from the firm. Even when there is shifting, the command of the resources goes to the government, not to the inefficient firms. Granted, the more profitable ones lose more, relative to current scale of operations, than the less profitable ones. But this effect is much less significant than might at first appear. Alternative forms of taxation placing more of the burden on the less profitable firms would, if not shifted, simply force them out of business, increase the degree of concentration, and reduce competition. The notion that high rates of profit in all instances reflect efficiency is of course invalid; in large measure, high profits are products of imperfectly competitive elements in the markets or of greater size attained by the more profitable firms. The so-called "less efficient" firms are primarily new firms which have not yet attained sufficient size for low-cost operations, and interference with their growth is obviously highly undesirable. Or they are declining firms whose ultimate fate cannot be forestalled for any length of time by lower tax payments. The notion of the tax protecting the inefficient firm is based upon a static rather than a dynamic concept of low-profit firms. Shift from a tax on net profit to some other form would greatly aggravate the problems of all firms in periods of temporary slack in the industry or general depression, and lessen the cyclical sensitivity of the federal tax yields.

Smith, as well as many others who are currently supporting the use of the value-added tax as a substitute for the corporate income tax, has apparently not given careful consideration to the nature of the value-added tax. The tax is essentially a sales tax and is so regarded in France, where it replaced the earlier turnover tax, and in the other Common Market countries, which have tentatively agreed to replace their turnover taxes, not their corporate income taxes, by this form of levy to bring uniformity in the indirect tax field. A value-added tax extending through the retail level is identical to a retail sales tax, except that impact is spread through production and distribution levels instead of being concentrated on the retailers. For the same reasons that sales taxes shift, so would the value-added tax. Thus Smith's argument that the tax would make "all users of productive resources bear a share of the costs of our collective expenditures made through the government" has no merit whatever. The tax will not place a penalty on the inefficient firm unless the tax is not shifted, any more than a sales tax does. The case for a value-added tax must rest on the same grounds as the case for a federal sales tax. In no sense does the tax represent some new and modern form of levy which will solve the admitted evils of the corporate income tax without the disadvantage of a sales tax.

To the extent to which the corporate income tax is shifted, the difference between it and the value-added tax is lessened, as well as the objection to the change and the need for it. Even if the corporate tax were primarily shifted, the replacement of it by the value-added tax would greatly reduce the relative tax burden on the differentially high incomes of firms in particularly advantageous positions and would increase the incentive to incorporate small closely held businesses to reduce tax liability.

EARL R. ROLPH: Mr. Ture's instructive account of the Treasury's new guidelines for the service life of equipment is generally critical of the official claims for the changes but leaves unanswered an important question. Will these changes result in more or less accurate reporting of depreciation and hence of business profits? According to my understanding, new equipment continues to be constructed out of metal rather than cardboard and glue. Hence it seems a bit odd that the life expectancy of equipment should be reduced in every category, and rather substantially. Mr. Ture states that the new guidelines are more in accord with experience than those laid down by Bulletin F, but the latter have not been those in actual use. Perhaps, in view of Mr. Ture's special knowledge of the topic, he can be persuaded to shed some light on the accuracy-versus-acceleration interpretation of the Treasury's new guidelines.

Professor Smith's proposals call in general for yet more lenient tax treatment of property incomes and especially of gains in the form of transfers at death and of appreciation of corporate securities. By implication, he must be favoring heavier taxation for people with small incomes, although his statement is not explicit on this point. One detects little concern with equity issues even in the minimum sense of equal treatment of equals. It would be instructive to learn if Professor Smith favors even proportional taxation, meaning by this a system that treats all personal gains as income subject to a uniform rate of tax.

Turning to Professor Musgrave's paper, we are told that some alternative federal tax structures will increase saving but by only small amounts, suggesting that such modifications are probably not worth while. Hence he adopts the common-sense approach that the total of private saving depends upon the distribution of after-tax income. But of course this conclusion is merely impossible. If saving is defined in the usual manner, the amount saved by a person or a group is the difference between two simultaneous flows, income and consumption, and if the group considered is a closed economy, the amount saved equals the amount invested. This equality holds for any distribution of income as among persons and as between the public taken as one group and the government taken as another. If the government's revenue and expenditure are held constant, private saving will remain the same whether the tax systems being compared are distributional neutral, progressive, regressive, or merely erratic. Saving in this sense is a pure residual of no economic importance.

The question of substance concerns the effect of alternative tax structures, or alternative fiscal systems, upon the amount of consumption, public and private. In a full employment setting, to obtain more investment, assuming that greater investment is a necessary condition for more rapid growth, resources must be released from other uses, mainly government noninvestment expenditures and private consumption. Alternative fiscal systems may be compared and appraised in terms of such an objective. There are many fiscal systems compatible with a forced-draft program of private and public investment. But if the tax structure is to satisfy equity goals as well, the topic becomes of some interest. This is Musgrave's problem.

Let us suppose that on equity grounds it is agreed that the tax structure should be radically progressive with respect to income distribution and in addition must satisfy the equal-treatment-of-equals test. Suppose that a forced-draft investment program is to be inaugurated. If there is a conflict between equity and growth, it must arise either from incentives or from lack of investment finance or both. We owe to Professor Musgrave (with Domar, *Q.J.E.*, 1944), the theory that the higher the rate of tax with full loss offsets, the greater the incentive to invest. Unfortunately, he seems to have forgotten this theory until late in his paper. The various compromises he is prepared to make, albeit reluctantly, need not be made on investment-incentive grounds. As concerns financial aspects, private consumption must be reduced and funds made available for the new and higher rate of investment. To reduce consumption, the tax structure should ideally avoid touching consumption that has a positive productivity in terms of working capacity and in terms of improving the health and education of children. For this purpose, progressive taxation appears to be an appropriate tool. The wealthy, because they would be heavily taxed, will curtail superfluous consumption, and with generous exemptions, people of modest incomes will be left untouched. There remains the problem of assuring that investment funds will be sufficiently abundant. This objective can be achieved by operating the federal budget at a sizable surplus and reducing the government debt—a policy to which Musgrave briefly alludes. Such a program is, however, imperative in a full employment setting if a forced-draft investment program is to be adequately financed without violation of equity goals.

There is no inherent conflict between the objectives of equity and growth. Growth from a full employment setting has slight relevance in the actual setting in which we find ourselves. If the Council's estimate of a deficiency in GNP of $35 billion (annual basis) is taken as reasonably accurate, marginal tinkering with the tax structure in the terms Musgrave discusses is grossly inadequate for the purpose of achieving full employment. Massive tax reduction is another question. To explore the adequacy of such a measure, one would need to take into account, among other considerations, monetary and debt policies as constrained by the reverence our financial leadership has now taken toward the gold stock.

CHALLIS A. HALL, JR: The three papers presented here on tax reform have been conspicuous for their emphasis on capital formation and economic growth. Yet each contribution is distinctive in its approach. Professor Smith has summarized the structure of traditional thought on this problem, and then has deftly pulled the rug from under the edifice. In balancing the generally conflicting claims of equity and growth, Professor Musgrave has succeeded in traversing a very difficult tightrope. By applying traditional concepts in an imaginative way, Mr. Ture has neatly laid out plausible ranges of the investment impacts from the new guideline lives and the investment credit. Faced with such a rich menu and limited time, a discussant must resolve an unenviable problem of choice. I have chosen to range widely and at random.

A major point advanced by Musgrave is that the desire to invest will not be effective unless a high degree of economic activity is obtained. It seems to me that this assertion is not generally accurate. If overcapacity exists at 5.5 per cent unemployment, investment incentives for expansion will evidently be low until the excess capacity is eliminated. But the excess will be eliminated in time as growth catches up with capacity, even if unemployment remains at this 5.5 per cent level. At this point, incentives for expansion investment should be quite buoyant. If it is true that there is some capital stock appropriate to a given level of economic activity and consistent with a vigorous investment demand, it follows that the problem of promoting a given level of activity is something different from the problem of promoting a certain rate of economic growth. (These problems are not, of course, unrelated, since a capital stock that is high relative to a labor force that is employed at 94.5 per cent may be low relative to a labor force that is employed at 97 per cent.) Thus there is no essential reason, in my view, why tax reform involving the promotion of saving and the desire to invest cannot be considered when the level of economic activity is relatively low. This view does not, of course, imply advocacy of a low employment policy.

Contemporary thought about the effects of taxation is strongly influenced by economists' conceptions of rational, profit maximizing behavior. Musgrave's and Ture's analyses and comments on the investment credit and accelerated depreciation are good examples. I have no objection to such analyses: they reveal the intricacies of tax effects in a consistent framework. However, as economists we would be wise to heed Smith's admonition about mechanistic doctrines. In particular, there are relatively few companies that calculate expected profit rate on new investment with discounted cash-flow techniques. Many companies do not even take into account accelerated depreciation or other features of the income tax in reckoning expected profit on new ventures. Under prevailing methods of estimating profit rates, the consequences of the investment credit or accelerated depreciation for the inducement to invest will be different from those suggested by the application of present value techniques, and the powers of alternative structural devices in stimulating investment will also be different from those suggested by orthodox analysis. Without detailed information on these critical practices in industry, the tax economist can only grind out his results and prescriptions with the tools at hand and hope that discussion before the Congressional committees will clarify the issues.

An important and related problem, again emphasized by Smith, concerns the climate of opinion in which tax measures are formulated and administered. Investor confidence does have a significant relationship to the economic response generated by a tax structure. Lack of confidence raises the evaluation of risks made by investors, reduces their commitments, especially in venture situations, and tends to curtail investment. It follows that tax reform to promote investment and growth should be well conceived and delicately negotiated if the tax reforms are to create their desired effects.

FINANCIAL INSTITUTIONS AND MONETARY POLICY: A RE-EXAMINATION OF THEIR INTERRELATIONSHIP

FINANCIAL INTERMEDIARIES AND THE GOALS OF MONETARY POLICY*

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In recent years, the interrelationship between financial institutions and monetary policy has come to be frequently considered in terms of the institutional scope of central banking. Specifically, the large size and wide dispersion of federal debt, on the one hand, and the proliferation of nonbank financial institutions, on the other, have given rise to the questioning of the confinement of central bank controls to commercial banks. With reference to the proximate objectives of the monetary authority, it has been questioned whether regulating the quantity of money and influencing the level and structure of interest rates are an adequate set of objectives for monetary policy in an economy with a changing pattern of financial institutions and practices.

Whatever the outcome of this questioning, cognizance of the changing pattern of financial institutions in a growing economy suggests that the conventional proximate objectives of monetary policy can be probed from another direction as well. In view of the financial institutions' adaptive capacity as manifested in their diversification and proliferation, the question arises whether the widely accepted, conventional operating goals of monetary policy are necessary or desirable for the future enhancement of the effectiveness of such policy. This question is the focus of the present paper.

"Defensive" and "Dynamic" Goals

In particular, the present paper is addressed to a widely noted, definitive exposition of the conceptual basis for the actual conduct of monetary policy in the contemporary United States. In the most detailed authorative account available of Federal Reserve open market operations, R. V. Roosa provides an informative picture of the nature of the activities performed by the officials on the front lines, so to speak, of monetary policy; namely, the personnel of the Trading Desk at the Federal Reserve Bank of New York [2]. A theme that permeates this

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entire vivid account is an analytic distinction by Roosa that may in time turn out to be a contribution of no less import than the "credit availability" doctrine, with which his name is so widely identified. The analytic distinction he makes is between the "defensive" responsibility and the "dynamic" responsibility of the central bank.

The defensive side of central banking means "defending against those seasonal, regional, or perhaps accidental causes of sudden stringency that arise in the process of issuing currency, or clearing checks, or meeting net flows of funds among regions, (or a vis-à-vis other countries), for example, and which might by unhappy coincidence aggravate, or even ignite, a financial and economic crisis" [2, page 8]. The dynamic side of central banking is the "use of the latent potential of a fractional reserve banking system in resisting inflation and deflation and facilitating economic growth" [2, page 8]. Thus, the monetary authority has a defensive goal and a dynamic goal. The defensive goal is "to help keep the machinery of the money market working smoothly in distributing and allocating the market's stock in trade within any given period" [2, page 18]; the dynamic goal is "to exert through the money market whatever degree of pressure upon bank reserves, liquidity, and the general availability of credit is required for stability without inhibiting sustainable economic growth" [2, page 18].

Historically, points out Roosa, the "shift-over from a purely *defensive* to what might be called a *dynamic* conception of Federal Reserve responsibility" [2, pages 8-9] is marked by the passage of the Banking Act of 1935. "The emergence of open market operations as a major instrument of monetary policy was the symptom, and in many ways represented the substance, of this change" [2, page 9]. Yet he emphatically cautions throughout his volume that "it would be misleading indeed to identify open market operations in any exclusive way with the *dynamic* side of Federal Reserve System operations. In practice, the *defensive* and the *dynamic* responsibilities of the System are carried out side by side, day by day, and most operating arms of the System are involved to some degree in each" [2, page 9].

Moreover, a major point that Roosa's volume conveys is that the pursuit of the System's defensive aim constitutes the greater part of the System's open market operations. At the same time, however, Roosa also points out that, "at least with respect to open market operations, there has in fact been a fusion of both types of responsibility" [2, page 105], and that it has "become second nature to the operating personnel to handle each problem with its *defensive* and *dynamic* aspects joined together" [2, page 105].

In consequence of the simultaneous pursuit of the defensive goal and the dynamic goal, it may be misleading to identify specific changes in the System's holdings of government securities, from, say, one week to another, with variations of general credit policy. Specifically, it would be an oversimplification to regard, as do the public and the textbook writers, an increase in the System's security holdings as an indication that money is going to become easier; analogously, it would also be an oversimplification to regard a reduction in the System's holdings as an indication that money is going to become tighter. In Roosa's words, "such observations are not necessarily wrong, but they frequently are" [2, page 100]. He gives as an illustration a period of six months which consisted almost entirely of System purchases of government securities at a time when the actual dynamic policy was one of restraint. This paradox, he notes, is resolved by recognizing the nature of the defensive policy pursued. The defensive policy was one of "keeping close watch on the absorption of reserves by other factors, and then taking action day by day, to offset only enough of the reserve absorption to prevent the creation of even tighter conditions in the money market than the policy of the Federal Open Market Committee intended" [2, page 103]. Therefore, a major explicit conclusion from Roosa's exposition is that "System Account purchases or sales, while always made with a view to effecting the general degree of reserve pressure intended by the Committee, can never be taken alone as a signal of the actual credit policy being pursued" [2, page 103].

Defensive Operations

Thus, it is their role as seasonal-offsetting operations that is of the essence of the System's defensive activities. In particular, three factors of reserve variation are important as spurs for seasonal-offsetting operations. Of these, the most important one by far is float, next in importance is currency in circulation, and third is the Treasury's balance in Federal Reserve banks.[1] All three of these variables are subject to seasonal fluctuations. The System's defensive activities are essentially open market operations intended to offset seasonal fluctuations in these three variables.

In terms of their magnitude, the defensive operations are much more important than the dynamic ones, insofar as separation of the two is quantitatively possible. For example, the range of intramonthly variation in float is of the order of $½-$1 billion. The consequence of this variation is, as Roosa observes, that "in a framework of overall policy considerations that may envisage changes of 300 or 400 million dollars in bank reserves within a month as the signal of a significant change in fundamental policy, it is necessary to offset through other means a considerable part of the much larger variation in reserves produced by float alone, if the reserve conditions expressive of general credit policy are to prevail" [2, page 69].

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[1] For the sake of completeness, it should be noted that Roosa also mentions variations in gold and foreign account and in required reserves as objects of offsetting operations. We do not refer to either of the two in the text for the following reasons. The matter of offsetting variations in gold and foreign account is a question of the secular, or pro-growth, objectives of the System, rather than a question of counterseasonal offsetting. And as for changes in required reserves, "they are much more markedly influenced by cyclical changes in business activity and the reflection of these movements in the employment of bank credit" [2, p. 76].

The day-by-day variation of currency in circulation is considerably less than the changes in float. Yet, even at that, "currency flows during the course of any year may account for a greater swing in total bank reserves outstanding, and give rise to a larger volume of compensating System action, than might be required for all of the System's *dynamic* responsibilities, if there were no mechanical fluctuations to face" [2, page 71].

Variations in the Treasury's balance in Federal Reserve banks are dealt with in a significantly different manner from variations in the other two variables. As Roosa explains it, "the problem in the case of float and currency is to decide whether or not a projected deviation is occurring by judging the immediate evidence visible in the market, and then if necessary to take offsetting action of an entirely different kind, usually through buying or selling government securities to release or absorb additional reserves. In the case of the Treasury's balance, the means exist for taking direct action immediately upon the item itself, in order to hold to a minimum its distorting influence upon the money market . . ." [2, pages 73-74]. Yet, even in the case of the Treasury's balance, although fluctuations "no longer play the major role among the disturbing influences in the money market that they once did, variations in the Treasury's balance still occur in magnitudes sufficiently large, at times, to require some offsetting action in the money market by the Account Management" [2, page 74].

The complexity of the System's offsetting open market operations can readily be inferred from Roosa's detailed account. Thus, in the case of float, he states:

> In calculating probable float patterns for each month, the starting point, of course, is the daily pattern of float for the same month over the past several years. Ordinarily, five preceding years are studied closely, although others may also be examined. Adjustments must be made for the shift of dates from year to year from one day of the week to another. Thus, for example, June 1, 1956, fell on a Friday and the behavior on that day and date would normally be somewhat different from 1955, when June 1 fell on a Wednesday. Moreover, there is always a float backwash following a holiday, and in this case the holiday was only a "partial" one—the Memorial Day holiday on May 30 being observed only in the northern and western parts of the country. That holiday backwash will also vary depending upon the day of the week in which the holiday falls. Detailed aspects such as these have to be studied on the basis of past experience and of recent ex-

perience over the past few days, before a projection can be prepared to show whether float may rise on June 1, 1956 by 100 million dollars or more, or possibly drop off by that amount. As a general rule, on some date near or just following the middle of the month, float will reach a monthly peak, ½ billion dollars or more above the low point usually occurring around the beginning of the month, but the day-by-day incidence of the changes will also be influenced by such factors as tax dates, falling on the fifteenth of several months [2, pages 69-70].

The Feel of the Market

The very complexity of the factors which the System's Trading Desk attempts to project and to offset requires, as Roosa emphasizes, considerable reliance on what the Trading Desk personnel develop in the way of "the 'feel' of the market." Thus, again with reference to float, it is noteworthy that:

> No matter how great the efforts of the Federal Reserve's operating arms . . . it is never possible to know what . . . [the actual float changes] are for a given day until some time after 3 p.m. on the following day. Thus there normally is no possibility of taking action on the basis of known behavior of the float until the second business day following that on which a given change in float occurs. Since the same is true for all of the other factors . . . [that call for offsetting operations], it should be clear that there is no substitute for action based upon the "feel" of the market (though always conditioned and alerted by the projections) if the Federal Reserve System's efforts are to be most nearly effective in preventing mechanical influences such as these from obscuring the pattern of bank reserves indicated by general credit policy [2, page 70].

Likewise, with reference to projections of daily currency changes, Roosa notes:

> Much of the time, the precision of the projections is very close. But there are generally three or four periods in any year when something happens to enlarge or spread out the currency outflow, or its return, and at these times there is again no substitute for the "feel" of the market in interpreting the immediate repercussions of such developments upon the existing state of bank reserves [2, page 71].

In essence, then, the daily activities of the Trading Desk are based on the combination of projections and the "feel" of the market.

> Alerted by these projections to the most probable pattern of developments, the Account Management then devotes itself, in effect, to putting together everything than can be observed in the market itself, in order to reach a judgment as to whether today's projection is actually coming true. There is no better description for this process than "getting the feel" of the market. It is the combination of both, the projections as modified or confirmed by the "feel," which provides the basis for deciding what action, if any, is to be taken in carrying out the instructions of the Federal Open Market Committee [2, page 65].

Some Questions

Now, in light of the conceptual distinction between dynamic and defensive operations, let us consider the use to which the feel of the market is put. On the one hand, it is suggested that, due to the actual fusion of dynamic and defensive operations, it is frequently wrong to identify an expansion of bank reserves with a policy of easy money and a contraction of bank reserves with a policy of tight money. On the other hand, it is indicated that the System's daily operations based upon the feel of the market, as conditioned and alerted by projections, are for the purpose of making sure that the System's dynamic policy "shall emerge from the day's confusion as a dominating force" [2, page 105]. Is there not a basic contradiction between these two propositions? To repeat, on the one hand, there is fusion of defensive with dynamic operations with the result that the behavior of the volume of bank reserves does not reflect dynamic policy alone. On the other hand, there is activity based on the feel of the market in the endeavor to make it clear to the private economy what the course of the System's dynamic policy is. Are not these two ideas incompatible?

The possibility of internal contradiction becomes even more bothersome on further reflection about the nature of activity based on the feel of the market. For as Roosa states the rationale for defensive operations:

> Much of the time . . . there is something happening mechanically, in ways that no one bank or investor could be expected to see, either to keep the money market swinging dizzily from side to side or to keep it for a significant time deceptively easier (or tighter) than general credit policy would intend. The fact that some of that gyrating or deviating occurs even after the Federal Reserve System has taken offsetting action only underlines the need for the Trading Desk to make thorough appraisals every day of the bank reserve situation, and of the credit conditions and psychology related to it, in order to limit what it cannot avoid [2, page 65].

Now, since there is complete fusion of defensive with dynamic operations as reflected in the actual behavior of the volume of bank reserves, how do we know whether the Account Management's feel of the market is—in particular instances or in general—good, bad, or indifferent? Indeed, how do we know that the "gyrating" or "deviating" that occurs in the money market and that may becloud the public's comprehension of the pattern of dynamic policy is not brought about rather than minimized by the System's seasonal offsetting operations?

We do not know, so long as dynamic operations remain fused with daily defensive operations, that are partly based on the Account Management's feel of the market. As one possible way to overcome this agnosticism, we venture to propose exploration of the possible desirability of the following experiment. The present paper takes but a small step towards such exploration.

The Experiment

This would be the experiment: Let the monetary authority refrain from seasonal offsetting in open market operations and engage in only those open market operations that would be in pursuit of the dynamic goal alone. At least two problems are immediately raised by this proposal. One relates to the duration of the experiment and the other to its announcement.

First, the experiment would, at the very least, require one year, for the obvious reason that the defensive offsetting operations are contraseasonal. But the time span of one year would probably be quite inadequate as a guide for future policy. Not only is a sample of one unconvincing evidence, but the passage of a single year is apt to be too brief an interval for the process of institutional adaptation that financial intermediaries might undergo if future monetary policy were confined to the pursuit of the dynamic goal alone. Thus, two or three years appear to be the more plausible provision for the duration of the experiment.

Of no less importance is the announcement aspect. At least three alternative possibilities are available. One possibility is that the monetary authority announce that beginning on, say, January 1 of the coming year, the Account Management will refrain from seasonal offsetting operations on an experimental basis. The point in explicitly announcing the experimental character of the venture would be to assure the financial system that in case of serious monetary or credit disturbances, the central bank would be prepared to abandon the experiment and revert to its previous pattern of operations, without further ado.

A second possibility would be to announce that beginning on a certain date, the Account Management will no longer engage in seasonal offsetting operations. Here the deliberate avoidance of any reference to experimentation would aim at expediting the process of adjustment by financial intermediaries to the central bank's novel approach.

A third possibility would be to make no announcement whatever and simply to institute the experiment. In the setting of contemporary American monetary policy, this last alternative is likely to be excluded on public relations grounds regardless of the merits it may have. It may also be deemed psychologically unwise to announce that as important a departure from past practice as the venture here proposed should be on experimental basis. The idea of the monetary authority conducting nationwide experiments of multi-year duration may be tolerable when carried in the back of the minds of central bankers but not when disseminated to a financial community whose immediate reaction to any significant departure from long-standing routine would be dismay if not hostility.

Therefore, if it were to be considered at all, the experiment might have to be considered and announced as a permanent departure from past practice. Only in this way might it be feasible to put the private financial sector on notice that a basic change in the nature of System open market operations is to take place, and that this change may, within the network of financal intermediaries and spending units, induce activities and arrangements that previously did not exist.

Possible Results

Now to suggest exploration of the possible desirability of so radical a departure from past practice would be altogether too presumptuous without some conjecture about the consequences that might ensue from this departure. Even if only at the conjectural level the social costs of the suggested change appear to exceed its social benefits, the change is clearly for the worse. At the same time, however, it is noteworthy that it is not easy to conjecture with confidence about a change whose consequences depend on the adaptability and inventiveness of thousands of private institutions over a period of a few years. The following thoughts are, therefore, put forward as no more than highly provisional speculation regarding the adjustment of financial intermediaries to the confinement of open market operations to the dynamic goal alone.

System abstention from counterseasonal offsetting operations can be expected to bring about seasonal fluctuations in a number of variables. In the first place, seasonal fluctuations in banks' total reserves and required reserves are likely to be accentuated. In consequence, there are likely to come about seasonal fluctuations in the federal funds rate along with a permanent expansion of the federal funds market itself.

Now so long as the Federal Reserve System's rediscounting facility is continued to be operated at a nonpenalty rate, the upward movement of the federal funds rate will be stemmed by the rediscount rate. Alternatively stated, seasonal fluctuations in the volume of rediscounting will occur. The implication of season fluctuations in rediscounting is that the monetary authority will release to the commercial banking system reserves through the rediscount window that previously were released by means of defensive open market operations.

It would appear, therefore, that in the absence of fundamental reform of the rediscounting facility, the termination of seasonal offsetting operations at the Trading Desk in New York might simply be followed by the institution of counterseasonal offsetting operations at the rediscount windows of the Federal Reserve System throughout the country. Yet this new division of labor within the System would still enable the Account Management to focus on its dynamic goal, and the discount-window managers could continue to exercise the discretionary authority they already have in regard to the magnitude and frequency of reserve accommodation they choose to extend at the window.

Moreover, the very likelihood of seasonal fluctuations in the volume of rediscounting serves to highlight the character of the nonpenalty-rate rediscounting facility as a mechanism of escape for member banks from the free-market conditions that would obtain in the federal funds market if the rediscount rate were always kept above the federal funds rate. In other words, the proposal of Trading Desk confinement to dynamic open market operations serves to underline the importance of a reconsideration of the System's current rediscounting instrument. Here it can only be mentioned in passing that this writer has elsewhere suggested the adoption of a floating rediscount rate that would be linked to the federal funds rate and that would always be maintained above the latter; thereby rendering the rediscount rate an effective penalty rate at all times.

Thus, the outcome of Trading Desk confinement to the dynamic goal largely depends upon whether we assume the present rediscounting facility to be retained or to be reconstituted into a penalty-rate mechanism that would render the central bank truly a lender of last resort. If the present facility is retained, then seasonal fluctuations in the money market appear most likely to occur in the rate and volume of federal funds and in the volume of rediscounting. By contrast, if penalty-rate rediscounting of the aforementioned type is instituted, the rediscount rate along with the federal funds rate and with the volume of rediscounting are likely to exhibit seasonal fluctuations. And in correspondence with the fluctuations in both the federal funds rate and the rediscount rate, other interest rates pertinent to the short-term reserve adjustment of commercial banks might also exhibit seasonal fluctuations. In particular, the Treasury bill rate, under the impact of seasonal variations in commercial bank holdings of Treasury bills, and commercial loan rates might be affected noticeably.

What the magnitude of effect upon interest rates would be is a question that only the actual performance of the experiment might settle definitively. Nevertheless, an indication of a very rough order of magnitude will here be hazarded on the basis of the following estimate of the seasonal variation in the money supply that might come about in the absence of any seasonal offsetting by the System.

The System's total holdings of government securities exhibit seasonal fluctuations, reflecting the Account Management's contraseasonal open market operations. Specifically, on a monthly basis, the seasonal adjustment factors for the System's holdings of government securities during 1954-61 varied from the March trough of 98.2 per cent to the December peak of 103.0 per cent; viz., a variation of 4.8 percentage points.[3] In terms of the current size of the System's holdings, which are about $30.0 billion, such seasonal variation in the System's holdings amounts to $1.5 billion. Now assuming a money multitions would yield at least two distinct though related benefits. First, it would permit open market operations to be concentrated entirely on the dynamic goal of monetary policy, thereby facilitating the task of the Account Management in pursuing this goal. Second, it would enhance public comprehension and appraisal of central bank activity. Some seasonal variation of interest rates appears to be a modest cost in relation to these benefits. Moreover, even this cost could be avoided by the use of rediscounting for seasonal offsetting, if any seasonal variation of interest rates were considered intolerable.

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[3] These seasonal adjustment factors are arithmetic means of the ratios of the original monthly series (total U.S. government security holdings of Federal Reserve banks) to centered twelve-month moving averages of the original series. The ratios were kindly computed by the staff of the Board of Governors of the Federal Reserve System on the Board's computer.

In sum, if experiments have any place in macroeconomics, here is a suggested experiment whose possible desirability might warrant further exploration. For such exploration, Roosa's contribution provides rich stimulus.

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THE PLACE OF FINANCIAL INTERMEDIARIES IN THE TRANSMISSION OF MONETARY POLICY*

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The influence of evolving financial institutions on the mechanism linking monetary policy with the pace of economic activity has recently attracted widespread attention. An active discussion contributed to the output of learned publications and even spilled over to Congressional hearings. Conferences met and commissions worked to evaluate the role of financial arrangements. But a systematic set of hypotheses describing financial markets and their interaction with the markets for real assets and output has not been provided. It is the purpose of this paper to convey in broad outline a theoretical frame which appears to explicate the role of financial intermediaries in the monetary processes.[1]

The Wealth Adjustment Process as a Framework for Monetary Analysis

Standard Keynesian theory exhibited the interest mechanism as the centerpiece in the transmission of monetary impulses to the pace of economic activity. This emphasis on the crucial role of interest rates remains to be explored. The context in which interest rates operate will differ radically if the system is analyzed in terms of a set of interrelated flow magnitudes or a stock dominated system of stock-flow relations. In the stock-flow analysis, the interrelation of money with current activity appears as part of a general wealth adjustment process.

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[1] Among the publications should be mentioned papers by Warren Smith, Hyman Minsky, David Alhadeff, and, of course, the works of John Gurley and Edward Shaw. Our frame of analysis is partly based on Karl Brunner's inquiries into the patterns of stock-flow interactions. A programmatic paper bearing on this problem was published in *Econometrica,* 1950. A detailed analysis of different patterns of stock-flow interaction was developed in a lengthy (unpublished) paper presented at the meetings of the Econometric Society in December, 1953. The present paper summarizes the formal structure of an explicit frame in the context of a stock-flow analysis based on our studies of demand patterns for financial assets, of the money supply mechanism, and on some preliminary explorations into investment and consumption behavior. Lack of space prohibits an adequate discussion of the formal structure. Mimeographed copies of a compact outline are available on request.

The public adjusts the composition of its balance sheet in response to relative prices—including interest rates—to achieve a desired balance-sheet position. Variations in output emerge from this process, particularly in response to the public's decision to adjust its real capital.

The unavoidably aggregative character of our analysis involves a grouping of economic units into four sectors with several subsectors and a classification of magnitudes occurring in balance sheets and income statements. The economy is partitioned into a (1) government sector including the central bank, (2) commercial banks, (3) financial intermediaries, and (4) the public composed of households and business firms. The government sector absorbs a portion of the economy's current output and issues interest and noninterest-bearing debt. The latter magnitude forms the monetary base. The commercial banks supply checking or time deposits and allocate their assets among reserves, investments in securities with a fixed coupon issued either by the government sector or the corporate subsector of the public, and loans to the public. Furthermore, banks borrow from the Federal Reserve banks according to a demand function, which explains the desired borrowing ratio in terms of market rates of interest, the rediscount rate, and the excess reserve ratio.

The nonbank financial intermediaries are subdivided into two subsectors. The first subsector consists of mutual savings banks, credit unions, savings and loan associations, and the postal savings system, whereas the second subsector is composed of the insurance business, pension funds, and similar institutions. This grouping is expected to reflect the order of substitutability between the liabilities issued by financial institutions.[3] Both groups of financial intermediaries allocate their assets among reserves, dominantly held in the form of checking deposits at commercial banks, investment in securities, and loans to the public. The portfolio allocations of financial institutions are significantly shaped by the interest rates prevailing for the various types of financial assets.

The public faces a considerably vaster allocation problem. Households and business distribute financial assets among base-money, the liabilities issued by financial institutions, and securities issued by the government or the corporate sector. The public also supplies liabilities to banks and other intermediaries. Furthermore, (nonhuman) wealth is allocated between financial assets and real (i.e., nonfinancial) assets in response to the yield rates on the various types of assets. This allocation pattern determines a crucial link in the transmission mechanism. An adequate explication requires a careful description of the public's stock-flow behavior bearing on its real capital position. According to the relation among asset price and output price and the associated patterns of stock-flow interaction we distinguish among three types of real capital:

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[3] Variations in interest rates significantly affect the distribution of the public's financial assets among checking deposits, time deposits at commercial banks, and liabilities of the first subsector of intermediaries. We suspect such variations in interest rates to exert a minor effect on the public's decision to distribute financial assets between liabilities of the second subsector of intermediaries and the liabilities of the remaining financial institutions.

Type I (e.g., machinery and equipment) is characterized by the simultaneous determination of an asset price and an output price by independent behavior relations. The asset price is (proximately) formed by a stock relation exhibiting an asset allocation process operating in a market for securities whereas the output price is (proximately) determined by a flow relation describing the interaction of producers' supply response with the firms' desired rate of accumulation. The latter rate depends essentially on the relation between asset price and output price, modified by interest rates on financial assets.

Type II is characterized by an output price identical with the asset price formed on an asset market. Sufficiently low transaction costs encourage the development of an asset market, not for real claims against real capital, but for real capital items themselves, where capital positions are adjusted by suitable unloading and acquisition. The asset price is (proximately) determined by the interaction of desired and actual stock and the actual (gross) rate of accumulation by the producers' supply response to this asset price. The (short-run) asset price (= output price) is thus dominated by a stock relation exhibiting a portion of the public's asset allocation process.[8] Houses, automobiles, and a variety of inventory items may be cited as examples.

Type III (e.g., many consumer durables) possesses only an output price. Sufficiently large transaction costs prohibit the emergence of an asset market on which positions are directly modified in either direction. A reduction in the desired stock is achieved not by unloading on a market but by natural depletion (i.e., real consumption). Output price and actual rate of accumulation are determined by the interaction of producers' supply behavior with the desired rate of accumulation. The latter rate depends on the relation between desired and actual stock. A flow relation incorporating pertinent stock magnitudes thus shapes the short-run behavior of output price.

The growth rate of the public's real wealth is the sum of the actual rates of accumulation for the three types of real capital. As a rule, this growth rate will not exhaust the current yield (= current output) of total wealth. A portion of this yield is allocated to real consumption (i.e., destruction of wealth). Real consumption is seen to depend on the prices of the categories of output distinguished,[4] yield rates on the types of assets specified,[5] the public's (nonhuman) wealth, the income expected from human wealth and the government's absorption rate.[6]

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[8] The difference between Type I and Type II capital may be further clarified: In case of Type II the real capital items are actually traded on the market, whereas in the case of Type I capital real claims against real capital are traded. The price formed on real claims imputes implicitly an asset price to the real capital.

Before we terminate our general description of the wealth adjustment process, some comments are appropriate about the speed of adjustment of the actual to the desired stock of particular assets. We sketched the mechanism linking money with output as a process continuously adjusting desired and actual balance-sheet positions, varying over the spectrum of assets from base-money to the component of real capital that has no asset market (Type III). The available evidence does not suggest a uniform adjustment rate of all asset markets or sector portfolios. The actual lag structure of the wealth adjustment process is most likely of extreme complexity and well-nigh impossible to trace with any measure of fidelity. Some crucial assumption about relative orders of magnitude must be introduced for the general frame to yield cognitively meaningful results. Such an assumption is easily conceived as a first approximation justified by the observation statements obtained with its aid. Our basic approximation formalizes the sharp differences in the adjustment rate of financial and nonfinancial assets. We stipulate, therefore, that, relative to the time units usually applied to our empirical data, financial institutions and the public completely adjust their actual to their desired financial assets, whereas the adjustment of the stocks of real capital involves a production process distributed beyond the time unit implicit in our analysis. This postulate implies that the stock supply and stock demand of financial assets are related by equilibrium conditions, while an explicit adjustment process linking desired stock and new production flows needs to be formulated for the real assets. The comparatively negligible lags are thus eliminated and the dominant ones emphasized to construct a model which enables us to investigate the nature of the channels connecting money with output and the role of financial institutions in the transmission of monetary impulses.

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[4] Total output is divided into four classes: output of Type I capital, output of Type II capital, output of Type III capital, and output for real consumption.

[5] These yield rates include the real rates of interest on real capital I and II. These real rates are implicitly determined by the appropriate asset prices in conjunction with the yield expected from real capital (per unit).

[6] The government sector absorbs in general portions of all four types of output distinguished. The three accumulation rates thus contain a government component. Taxes affect the expected yield from real capital, the income expected from human wealth, and the possible separation of prices into buyers' and sellers' price differentiated by a tax parameter. The wealth-adjustment process characteristically operates through modification of interest rates; i.e., through relative prices. Therefore, some comments on the credit markets which proximately shape financial rates seem appropriate. The allocation pattern of financial institutions and the public together with the government sector's supply of debt completely specify the demand and supply relations composing the credit markets. These markets jointly determine the two loan rates, the securities' yield rate, and associated rates on time deposits and on liabilities of nonbank intermediaries.

Aspects and Implications of the Wealth Adjustment Process

Monetary policy operates directly on the monetary base, the reserve requirements, the rediscount rate, and the distribution of government debt between interest-bearing and noninterest-bearing debt. The wealth adjustment process enables us to trace the general nature of the monetary mechanism converting policy impulses into an accelerated (or decelerated) pace of economic activity. An increase in base-money disrupts the prevailing allocation pattern of financial assets. The commercial banks' actual and desired reserve ratios diverge and, depending on the nature of the transaction which generates the increase in the base, the public's allocation pattern may also be disrupted. Both banks and public endeavor to adjust their actual position to achieve the pattern most desired in response to newly prevailing market conditions. The adjustment in portfolios affects supply and demand in the credit markets. The yield rate on securities and the interest rate on bank loans fall. The response of financial intermediaries to the reduction of security yields transmits the downward pressure to rates on loans supplied by nonbank intermediaries.

The operation of this interest mechanism, closely associated with the process of asset reallocation, modifies the response of money stock and the total volume of financial assets.[7] The modification is the result of a feedback from the initial responses of the economy to the injection of base-money. It has the effect of slightly altering the magnitude of the responses but has no particular significance for the direction of the adjustment. The decisive core of the transmission is the distribution of adjustments in yield rates over the spectrum of assets and not the variations in money stock or stock of financial assets. The latter occurrences are "symptomatic" by-products of the adjustments in the interest structure.

The reduction in financial rates achieved by the injection of base-money disrupts the public's portfolio balance between financial and real assets. The resulting desire to adjust portfolio compositions lowers the real rate of interest and consequently raises the asset prices of real capital. The increase in the asset price of capital I relative to the output price accelerates the actual rate of accumulation depending on the price and cost elasticities of the producers' supply behavior. The increase in the asset price of capital II stimulates the production of new capital II according to the producers' supply elasticities. Furthermore, the resulting rise in the market value of the public's (nonhuman) wealth raises the desired stock of capital III and the desired rate of real consumption. The increase in output initiated by the monetary impulse appears thus as the sum of the indicated accelerations in the rates at which real assets are accumulated and the accompanying increase in real consumption.[8]

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[7] For financial assets, the interest mechanism reduces the magnitude of the portfolio adjustment. For the money stock the outcome depends crucially on the comparative size of the public's and the banks' interest elasticities. If these interest elasticities are of approximately equal order, the money stock responds with unit elasticity to variations in the base. If the public's interest elasticities outweigh the banks' elasticities, then the interest mechanism magnifies the response of the money stock to a change in the base and the elasticity of money supply with respect to the base becomes greater than unity.

A close inspection of the above transmission chain reveals the pervasive play of relative prices, including interest rates, in the process by which the public adjusts its portfolio to a change in monetary policy. The response of the interest rate structure to policy actions modifies the proportion of financial and nonfinancial wealth in the public's desired portfolio. This modification operates via changes in total wealth in response to variations in relative prices—yield rates on financial assets and the asset prices of real capital—and changes in the income expected from human wealth. Wealth and substitution processes thus distribute the impact of a given change in policy over the production sectors. Adjustments in the prevailing stock of real capital and the rate of real consumption follow. The crosscurrents generated by relative output prices and the consequent revision in the income expected from human wealth amplify the initial wealth and substitution effects. The feedback from the credit market tends to dampen the subsequent reaction.

Wealth and substitution processes thus distribute the initial monetary impulse over the complete spectrum of interest rates, ranging from yield rates on close substitutes for base-money to the more remote yields on real capital. The response of asset prices can thus be used as a proximate measure for the magnitude of the transmission impulse, to be subsequently distributed between changes in real output and price adjustments according to the existing price elasticities.

The role of financial institutions is of paramount concern in the present context. In particular, we note that the wealth elasticity of the public's demand for financial assets and the proportion of financial assets in total wealth contribute to shape the response of the asset prices to variations in the base or other policy variables. A larger proportion of financial assets, or a larger wealth elasticity, magnifies the transmission impulse. This implication bears on the interpretation of the role of nonbank financial intermediaries. Our preliminary investigations of the demand functions for financial assets suggest that commercial bank time deposits and the liabilities of Type I intermediaries possess a wealth elasticity above unity and above the wealth elasticity of the public's demand for checking deposits. Growth in wealth has thus been accompanied by a growing proportion of financial assets and a shift within the financial system toward nonbank intermediaries. The analysis of the wealth adjustment process thus reveals that the response of the financial system to expanding wealth has amplified the transmission impulse emitted by monetary policy.[9]

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[8] The wealth effect on the desired stock of capital III and real consumption is amplified by the operation of relative price effects, which also work to distribute monetary impulses. Impulses generated by variations in reserve requirements, the discount rate, or the stock of interest-bearing government debt are converted into accelerated flow rates of real consumption and capital adjustment by the same transmission mechanism sketched above. It is noteworthy that our estimates of the transmission impulse, measured as an elasticity, is largest for the base, medium for the reserve requirements, and smallest for interest-bearing government debt.

The explicit formulation of the monetary process yields an interpretation of the financial institutions asset allocation patterns. Highly interest-elastic cash-asset portfolios raise the interest elasticity of the stock of financial assets and produce relatively large portfolio changes between reserve (or cash) positions and holdings of financial assets. Such large changes dampen the reaction in interest rates and thus the response of pertinent asset prices. A corresponding sensitivity of the financial institutions' allocation of earning assets between loans and investments, on the other hand, seems essentially irrelevant since the quantity of financial assets supplied is unaffected by such changes. Uniformly high or low direct or cross-elasticities of loan and investment portfolios with respect to loan rates and securities yield rates do not affect the transmission of monetary impulses. Institutional evolutions which apparently render the financial intermediaries' partition of earning assets more sensitive to market conditions are thus, per se, no evidence of lower effectiveness in our policy mechanisms. The comparative magnitudes of the direct and cross-elasticity of loan portfolios with respect to loan rates and securities yield rates, on the other hand, exert a substantial influence on the magnitude of the transmitted impulse. If both banks and nonbank intermediaries exhibit a large and/or growing direct elasticity with respect to loan rates, compared to the cross-elasticity of loans with respect to securities yield rates, a stronger reaction in pertinent asset prices must emerge before equilibrium is established between desired and actual stocks of loans and securities.[10]

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[9] The significance of a relatively declining banking system in the context of an expanding financial system requires additional comment. Variations in the relative importance of banks, measured by the proportion of bank deposits in total financial assets, affect the transmission impulse via the interest elasticities of the stock of financial assets. A declining proportion of bank deposits lowers this interest elasticity and consequently raises the response of the pertinent asset prices.

[10] Numerous additional features of security markets and the public's wealth position shape the nature of the transmission mechanism. A small proportion of real capital (at

The analytic frame effectively shows the two-way connection between the financial and the nonfinancial processes. Monetary impulses not only affect the real processes but real impulses feedback to financial processes. Variations in the stock of real capital, of income expected from human wealth, or the yield expected from real capital affect the allocation pattern of financial assets, trigger the interest mechanism, and generate a feedback to the asset prices of real capital. In particular, an increase in the expected yield on real capital influences the credit markets and induces a reallocation of financial assets accompanied by a rise in interest rates on financial assets. This rise spills over to the real rate, thus attenuating the initial rise in the price of real assets attributable to the original increase in expected yield.

The nature of the feedback mechanism between the markets for financial and real assets determines whether our financial system cushions the shocks operating on the real processes or works to magnify their effects on output and price behavior. If the repercussions through the financial system mobilize resources by generating large variations in allocation patterns and small changes in interest rates (including the real rate), the absorptive capacity of the financial system is small and may even be replaced by a magnifying effect. We tentatively conclude that the financial system is a shock absorber. The net effect of repercussions within the financial system, arising from variations in the expected yield on real capital, attenuates the initial impact on the asset prices of real capital.[11] Among the features which contribute to a large feedback impulse are (1) a low wealth elasticity of the public's desired portfolio of financial assets and (2) the low interest elasticities of this portfolio. On the other hand, large direct and cross-elasticities of the banks' loan portfolio with respect to loan rates and securities' yield rate dampen the feedback impulse by inducing large changes in desired holdings of financial assets under the conditions posited. Furthermore, sensitivity of the intermediaries' loan portfolio and of the public's allocation to intermediary liabilities with respect to securities' yield rate also contribute to absorb a portion of the initial "shock" in the reallocation of financial portfolios and thus lower the feedback inputs.

The history of our intellectual endeavors suggests the importance

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market value) in nonhuman wealth and a large elasticity of the corporate sector's stock supply of securities with respect to the real rate on real capital converts monetary action into comparatively large impulses on asset prices. The same result applies in case of a comparatively small absorption of securities by the financial system.

[11] The response of the desired currency ratio to serious deflationary experiences should be mentioned in this context as a factor which contributed on occasion in the past to magnify the feedback impulse generated by a contraction in the expected yield from real capital.

of a methodological rule requiring immediate exposure of a working hypothesis to some accumulated observations. The broad contours of three distinct phenomena can be appraised. First, we note that under the hypothesis outlined expansions in base-money are typically associated with accelerations in the pace of economic activity or increasing prices, whereas a rising stock of real wealth, per se, decelerates this pace and lowers prices. A regression of net national product (at current prices) on the money stock and a proxy measure of real wealth confirms this expected pattern. More significant is the reasonable performance of an (approximative) macrotheory explaining income behavior in terms of money supply and money demand, the latter being determined by an index of interest rates, real wealth, and an expression depending on current income.[12]

The major contours of the movements in financial rates yield a second measure of support. Variations in transaction costs, risks, preferences, and relative supply quantities of different debt intruments determine the relative movements of financial rates. But the broad, common sweep of all the rates is determined, under the hypothesis outlined, by the joint operation of policy variables on the one side, and the stock of real capital, income expected from human wealth, and the yield expected from real capital on the other. In particular, a sufficiently small growth rate of the monetary base generates a pattern of rising financial rates and a sufficiently large growth rate of the base a pattern of declining financial rates. The essential contours of the movements in base and financial rates during the thirties and fifties confirm this expectation.[13]

The behavior and the relative importance of commercial banks in the financial system is the third phenomenon to be considered. We

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[12] The regression was computed in the form of logarithmic first differences, based on annual data covering the sample period 1898–1960. The results of the estimation are as follows:

$$\log \frac{Y}{Y_{-1}} = .0006 + \underset{(.141)}{1.352} \log \frac{M}{M_{-1}} - \underset{(.291)}{.859} \log \frac{Y_{-1}{}^{p}}{Y_{-2}{}^{p}}; \quad R = .784$$

Y = net national product at current prices, M = money supply (currently held by public and demand deposits adjusted and time deposits of commercial banks), Y^p = Friedman's real permanent income as a proxy for real wealth. The subscripts indicate the lagged values. Standard errors of regression coefficients appear in parentheses below the coefficient. The macrotheory mentioned in the text can be justified on the basis of the wealth adjustment process. It has been developed and appraised in our paper on "Predicting Velocity: Implications for Theory and Policy," presented at a joint meeting of the American Finance Association and Econometric Society, December, 1962.

[13] The comparative behavior of base and interest rates during the fifties is particularly intriguing. Until the middle fifties the growth rate of the base substantially exceeded its average growth rate in the second half of the decade. We also note that around the middle fifties the increasing trend of interest rates accelerated. The reversal in the growth rate of the (adjusted) base at the beginning of the sixties has also been accompanied with a reversal in the trend of financial rates.

submit in this context that the pertinent considerations apply to the variations in the proportion of total financial liabilities which are obligations of commercial banks. Under the hypothesis considered, changes in this proportion are a by-product of the working of the interest mechanism. In particular, periods with a growth rate of the base persistently large relative to the growth of real capital and its expected yield tend to exhibit an increasing importance of commercial banks. On the other hand, periods with persistently low (relative) growth rate in the base tend to show decreasing importance for commercial banks. The banks' relative weight was computed as the ratio of bank deposits (demand and time) to the liabilities of Type I nonbank intermediaries. Annual data for the period 1896-1961 were used. Inspection of this time series reveals a complete absence of any trend, but a clearly marked oscillation with two peaks and a trough. The ratio begins at 1.615 in 1896 and moves without interruption to a peak of 4.204 in 1920. The subsequent decline leads to a trough of 1.634 in 1933. The new expansion phase for banks terminates with the peak ratio 4.092 in 1945. The postwar decline proceeded to a value of 1.632 in 1961. Both periods following the two World Wars experienced a comparatively small growth in the base, whereas the intermediate period was definitely characterized by a very substantial growth in the base.

Concluding Remarks

The analytic frame characterizing the wealth adjustment process yields explications and interpretations for a variety of issues in addition to those considered above. It explains the negative multiplier estimates for government expenditures in the context of standard Keynesian theories, the relative instability of multiplier estimates for an inclusive concept of autonomous expenditures in a similar context, and the relative stability of a "monetary multiplier" in periods exhibiting large variations in the base. The model refutes, on the other hand, the widespread interpretation of "net inflows" to time and savings accounts as a savings flow. Such flows are shown as the by-product of an asset allocation process only tenuously related to saving (i.e., growth in wealth). The working hypothesis implies, furthermore, the significant operation of fiscal policy, with some modifications, though. It emphasizes the deflationary feedback via the stock magnitudes of the financial system of a sustained deficit financed by issuing interest-bearing debt. It also indicates the irrelevance of the concept of "debt monetization" by commercial banks and asserts the decisive importance of the manner in which a deficit is financed as against existence and magnitude of the deficit. We also infer from the

FINANCIAL INTERMEDIARIES AND THE EFFECTIVENESS OF MONETARY CONTROLS

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Does the existence of uncontrolled financial intermediaries vitiate monetary control? What would be the consequences of subjecting these intermediaries to reserve requirements or to interest rate ceilings?

This paper is addressed to these questions, but it treats them theoretically and at a high level of abstraction. The method is to set up models of general equilibrium in financial and capital markets and to trace in these models the effects of monetary controls and of structural changes. Equilibrium in these models is an equilibrium of stocks and balance sheets—a situation in which both the public and the financial institutions are content with their portfolios of assets and debts, and the demand to hold each asset is just equal to the stock supply. This approach has obvious limitations, among which the most important is probably that it has nothing to say about speeds of adjustment and other dynamic effects of crucial practical importance. On the other hand, monetary economics has long suffered from trying to discuss these effects without solid foundation in any theory of general financial equilibrium. We feel that we can advance the discussion by outlining a systematic scheme for comparative static analysis of some of the questions at issue.[1]

The models discussed in the text are simple ones, designed to bring out the main points with few enough assets and interest rates so that graphical and verbal exposition can be used.[2] The exposition in the text takes advantage of the fact that introducing nonbank financial intermediaries, uncontrolled or controlled, into a system in which banks are under effective monetary control presents essentially the same problems as introducing commercial banks as an intermediary, uncontrolled or controlled, into a system in which the government's essential control is the supply of its own currency. The analysis therefore centers on the more primitive question: the effects of financial intermediation by banks, the consequences of leaving their operation unregulated, and the effects of regulating them in various ways. The conclusions have some interest in themselves, in clarifying the functions of reserve and rate controls on commercial banks. By analogy they also bear on questions concerning the extension of such controls to other financial intermediaries.

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[1] This paper is based on work by both authors. Some of its topics were treated in a preliminary way in a Cowles Foundation Discussion Paper (No. 63, January 1958, mimeographed) of the same title, by James Tobin. The general approach of that paper was elaborated and extended in a systematic way by William Brainard in his Yale doctoral dissertation, "Financial Intermediaries and a Theory of Monetary Control," submitted in 1962. Some of the results obtained in the dissertation are used in this paper.

[2] An Appendix giving both the mathematical analysis of these simple models and their extension to models containing many financial intermediaries, assets, markets, and rates is available upon request.

The main conclusions can be briefly stated. The presence of banks, even if they were uncontrolled, does not mean that monetary control through the supply of currency has no effect on the economy. Nor does the presence of nonbank intermediaries mean that monetary control through commercial banks is an empty gesture. Even if increases in the assets and liabilities of uncontrolled intermediaries wholly offset enforced reductions in the supplies of controlled monetary assets, even if monetary expansion means equivalent contraction by uncontrolled intermediaries, monetary controls can still be effective. However, substitutions of this kind do diminish the effectiveness of these controls; for example, a billion dollar change in the supply of currency and bank reserves would have more effect on the economy if such substitutions were prevented.

Whether it is important that monetary controls be more effective in this sense is another question, to which this paper is not addressed. When a given remedial effect can be achieved either by a small dose of strong medicine or a large dose of weak medicine, it is not obvious that the small dose is preferable. Increasing the responsiveness of the system to instruments of control may also increase its sensitivity to random exogenous shocks.[8] Furthermore, extension of controls over financial intermediaries and markets involves considerations beyond those of economic stabilization; it raises also questions of equity, allocative efficiency, and the scope of governmental authority.

The Nature of Financial Intermediaries

The essential function of banks and other financial intermediaries is to satisfy simultaneously the portfolio preferences of two types of individuals or firms. On one side are borrowers, who wish to expand their holdings of real assets—inventories, residential real estate, productive plant and equipment, etc.—beyond the limits of their own net worth. On the other side are lenders, who wish to hold part or all of their net worth in assets of stable money value with negligible risk of default. The assets of financial intermediaries are obligations of the

[8] The balancing of these considerations and the desirability of finding structural changes which increase the first kind of responsiveness without increasing the second are discussed in the Brainard dissertation cited above.

borrowers—promissory notes, bonds, mortgages. The liabilities of financial intermediaries are the assets of the lenders—bank deposits, savings and loan shares, insurance policies, pension rights.

Financial intermediaries assume liabilities of smaller default risk and greater predictability of value than their assets. The principal kinds of institutions take on liabilities of greater liquidity, too; thus bank depositors can require payment on demand, while bank loans become due only on specified dates. The reasons that the intermediation of financial institutions can accomplish these transformations between the nature of the obligation of the borrower and the nature of the asset of the ultimate lender are these: (1) administrative economy and expertise in negotiating, accounting, appraising, and collecting; (2) reduction of risk per dollar of lending by the pooling of independent risks, with respect both to loan default and to deposit withdrawal; (3) governmental guarantees of the liabilities of the institutions and other provisions (bank examination, investment regulations, supervision of insurance companies, last-resort lending) designed to assure the solvency and liquidity of the institution. For these reasons, intermediation permits borrowers who wish to expand their investments in real assets to be accommodated at lower rates and easier terms than if they had to borrow directly from the lenders. If the creditors of financial intermediaries had to hold instead the kinds of obligations that private borrowers are capable of providing, they would certainly insist on higher rates and stricter terms. Therefore, any autonomous increase—for example, improvements in the efficiency of financial institutions or the creation of new types of intermediaries—in the amount of financial intermediation in the economy can be expected to be, *ceteris paribus*, an expansionary influence. This is true whether the growth occurs in intermediaries with monetary liabilities—i.e., commercial banks—or in other intermediaries.

In the interests of concise terminology, "banks" will refer to commercial banks and "nonbanks" to other financial institutions, including savings banks. Moreover, "intermediary" will refer to an entire species, or industry, of financial institutions. Thus all commercial banks constitute one intermediary, all life insurance companies another, and so on. An "institution" will mean an individual member of the species, an individual firm in the industry—a bank, or a life insurance company, or a retirement program.

Financial institutions fall fairly easily into distinct categories, each industry offering a differentiated product to its customers, both lenders and borrowers. From the point of view of lenders, the obligations of the various intermediaries are more or less close, but not perfect, substitutes. For example, savings deposits share most of the attributes of demand deposits; but they are not means of payment, and the institution has the right, seldom exercised, to require notice of withdrawal. Similarly, there is differentiation in the kinds of credit offered borrowers. Each intermediary has its specialty; e.g., the commercial loan for banks, the real estate mortgage for the savings and loan association. But the borrowers' market is not completely compartmentalized. The same credit instruments are handled by more than one intermediary, and many borrowers have flexibility in the type of debt they incur. Thus there is some substitutability, in the demand for credit by borrowers, among the assets of the various intermediaries.

There is also product differentiation within intermediaries, between institutions, arising from location, advertising, and the other sources of monopolistic competition. But this is of a smaller order of importance than the differentiation between intermediaries. For present purposes, the products offered by the institutions within a given intermediary can be regarded as homogeneous.

The Substitution Assumption. These observations about the nature of financial intermediaries and the imperfect competition among them lead to a basic assumption of the following analysis. The liabilities of each financial intermediary are considered homogeneous, and their appeal to owners of wealth is described by a single market rate of interest. The portfolios of wealth-owners are made up of currency, real capital, and the liabilities of the various intermediaries. These assets are assumed to be imperfect substitutes for each other in wealth-owners' portfolios. That is, an increase in the rate of return on any one asset will lead to an increase in the fraction of wealth held in that asset, and to a decrease or at most no change in the fraction held in every other asset. Similarly, borrowers are assumed to regard loans from various intermediaries as imperfect substitutes. That is—given the profitability of the real investment for which borrowing is undertaken—an increase in one intermediary lending rate will reduce borrowing from that intermediary and increase, or at least leave unchanged, borrowing from every other source.

The Criterion of Effectiveness of Monetary Control

A monetary control can be considered expansionary if it lowers the rate of return on ownership of real capital that the community requires to induce it to hold a given stock of capital, and deflationary if it raises that rate of return. (The words expansionary and deflationary are used merely to indicate the direction of influence; the manner in which the influence is divided between price change and output change depends on aspects of the economic situation that are not relevant here.) The value of the rate of return referred to is a hypothetical one: the level at which owners of wealth are content to absorb the given stock of capital into their portfolios or balance sheets along with other assets and debts. In full equilibrium, this critical rate of return must equal the expected marginal productivity of the capital stock, which depends technologically on the size of the stock relative to expected levels of output and employment. If a monetary action lowers the rate of return on capital that owners of wealth will accept, it becomes easier for the economy to accumulate capital. If a monetary action increases the rate of return on equity investments demanded by owners of wealth, then it discourages capital accumulation.

This paper concerns the financial sector alone, and we make no attempt here to describe the repercussions of a discrepancy between the rate of return on capital required for portfolio balance and the marginal productivity of capital. These repercussions occur in the market for goods and services and labor, and through them feed back to the financial sector itself. Let it suffice here to say that they are qualitatively of the same nature as the consequences of discrepancy between Wicksell's natural and market rates of interest.

We assume the value of the stock of capital to be given by its replacement cost, which depends not on events in the financial sphere but on prices prevailing for newly produced goods. We make this assumption because the strength of new real investment in the economy depends on the terms on which the community will hold capital goods valued at the prices of current production. Any discrepancy between these terms and the actual marginal productivity of capital can be expressed alternatively as a discrepancy between market valuation of old capital and its replacement cost. But the discrepancy has the same implications for new investment whichever way it is expressed.

This required rate of return on capital is the basic criterion of the effectiveness of a monetary action. To alter the terms on which the community will accumulate real capital—that is what monetary policy is all about. The other criteria commonly discussed—this or that interest rate, this or that concept of the money supply, this or that volume of lending—are at best only instrumental and intermediate and at worst misleading goals.

Summary of Regimes to Be Discussed

The argument proceeds by analysis of a sequence of regimes. A regime is characterized by listing the assets, debts, financial intermediaries, and interest rates which play a part in it. In all the regimes to be discussed, net private wealth is the sum of two components: the fixed capital stock, valued at current replacement cost; and the noninterest-bearing debt of the government, taking the form either of currency publicly held or of the reserves of banks and other intermediaries. In the models of this paper, there is no government interest-bearing debt.[4] Consequently there are no open market operations proper. Instead the standard monetary action analyzed is a change in the supply of noninterest-bearing debt relative to the value of the stock of capital. (Only the proportions between the two components of wealth matter, because it is assumed that all asset and debt demands are, at given interest rates, homogeneous of degree one with respect to the scale of wealth.)

TABLE 1

SUMMARY OF FINANCIAL REGIMES DISCUSSED IN TEXT

REGIME	STRUCTURE OF ASSETS AND DEBTS		YIELDS TO BE DETERMINED ON
	Holder	Assets (+), Debts (−)	
I	Private wealth-owners	+ Currency + Capital	Capital
II	Private wealth-owners	+ Currency, + Capital, + Intermediary Liabilities	Capital, Intermediary Liabilities and Loans
	Private borrowers	− Loans, + Capital	
	Intermediary	− Liabilities, + Loans, (+ Reserves)	
III	Private wealth-owners	+ Currency, + Capital + Deposits	A) Capital, Loans, Deposits
	Private borrowers	− Loans, + Capital	B) Capital, Loans (Deposit rate fixed)
	Intermediary (Banks)	− Deposits, + Loans, + Reserves (Currency)	

The public is divided, somewhat artificially, into two parts: wealth-owners and borrowers. Wealth-owners command the total private wealth of the economy and dispose it among the available assets, ranging from currency to direct ownership of capital. Borrowers use the loans they obtain from financial intermediaries to hold capital. This split should not be taken literally. A borrower may be, and usually is, a wealth-owner—one who desires to hold more capital than his net worth permits.

A final simplification is to ignore the capital and nonfinancial accounts of intermediaries, on the ground that these are inessential to the purposes of the paper. Table 1 provides a summary of the regimes to be discussed.

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[4] This complication has been discussed in other works of the authors; in Brainard, *op. cit.*, and in Tobin, *An Essay in the Pure Theory of Debt Management*, to be published in the research papers of the Commission on Money and Credit.

Regime I: A Currency-Capital World

It is instructive to begin with a rudimentary financial world in which the only stores of value available are currency and real capital. There are no intermediaries, not even banks, and no credit markets. Private wealth is the sum of the stock of currency and the value of the stock of capital. The stock of currency is, in effect, the government debt, all in noninterest-bearing form. The required rate of return on capital R_o is simply the rate at which wealth-owners are content to hold the existing currency supply, neither more nor less, along with the existing capital stock, valued at replacement cost. The determination of R_o is shown in Figure 1. In Figure 1, the return on capital R_o is measured vertically. Total private wealth is measured by the horizontal length of the box *OW*, divided between the supply of currency *OC* and the replacement value of capital *CW*. Curve *DD′* is a portfolio choice curve, showing how wealth-owners wish to divide their wealth between currency and capital at various rates R_o. It is a kind of "liquidity preference" curve. The rate which equates currency supply and demand—or, what amounts to the same thing, capital supply and demand—is $\bar{R}_o$.

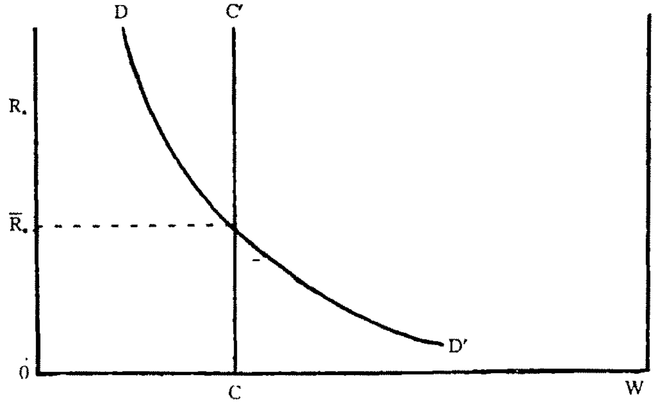

FIGURE 1

In this rudimentary world, the sole monetary instrument is a change in the supply of currency relative to the supply of capital. An increase in currency supply relative to the capital stock can be shown in Figure 1 simply by moving the vertical line *CC′* to the right. Clearly this will lower the required rate of return $\bar{R}_o$. Similarly, the monetary effect of a contraction of the currency supply can be represented by a leftward shift of the same vertical line.

Regime II: An Uncontrolled Intermediary

Now imagine that a financial intermediary arrives on the scene. The liabilities of the intermediary are a close but imperfect substitute for currency. Its assets are loans which enable private borrowers to hold capital in excess of their own net worth. How does the existence of this intermediary alter the effectiveness of monetary policy? That is, how does the intermediary affect the degree to which the government can change $\bar{R}_o$ by a given change in the supply of currency?

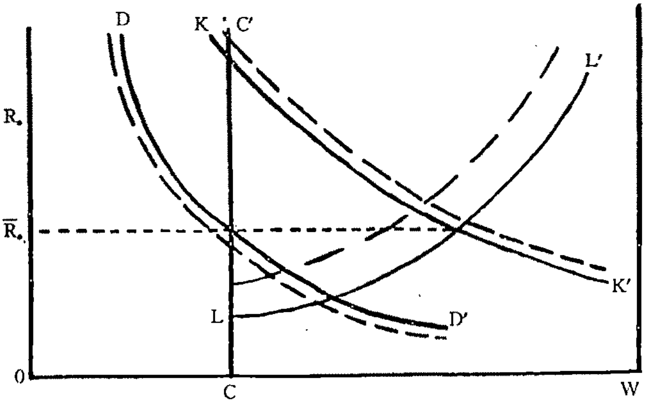

FIGURE 2

We will assume first that the intermediary is not required to hold reserves and does not hold any. Its sole assets are loans. To any institution the value of acquiring an additional dollar liability to the public is the interest at which it can be re-lent after allowance for administrative costs, default risk, and the like. Consequently, in unrestricted competition this rate will be paid to the intermediary's creditors. In equilibrium, the borrowers' demand for loans at the prevailing interest rate on loans will be the same as the public's supply of credits to the intermediary at the corresponding rate.

This regime is depicted in Figure 2. The axes represent the same variables as in Figure 1, and the supplies of currency and capital are shown in the same manner. But the demand for capital is now shown in two parts. The first, measured leftward from the right vertical axis to curve *KK'*, is the direct demand of wealth-owners. The second part, measured rightward from line *CC'* to curve *LL'*, is the demand for capital by borrowers. This distance also measures the demand of borrowers for loan accommodation by the intermediary. Curve *DD′* represents, as in Figure 1, the demand of wealth-owners for currency. The horizontal difference between *DD′* and *KK′* is their demand for the liabilities of the intermediary.

In this regime there is a second interest rate to be determined, the rate R_2 on intermediary liabilities. The rate on intermediary loans, r_2, is uniquely determined by R_2; competition among institutions keeps the margin between these rates equal to the cost of intermediation. The position of the three curves in Figure 2 and the demands which they depict depend on this rate as well as on R_o. The three dashed curves represent a higher intermediary rate R_2 than the solid curves. However, only the solid curves represent an equilibrium combination of the two rates, at which (1) the demands for capital absorb the entire capital stock, (2) the loan assets of the intermediary equal its liabilities, and (3) the demand for currency is equal to the supply.

We may presume, of course, that the introduction of the intermediary lowers the required rate of return on capital $\overline{R}_o$. For wealth-owners, the intermediary's liabilities satisfy some of the same needs which would be met in Regime I by an increase in the supply of currency. At the same time, some of the capital which wealth-owners formerly held can now be lodged with borrowers, at a lower rate of return. These borrowers were unable to obtain finance to hold capital in the Regime I.

An autonomous growth of the intermediary can be formally represented by a reduction in the margin between the intermediary's lending and borrowing rates. As the intermediary becomes more efficient in administration, risk pooling, and in tailoring its liabilities and assets to the preferences of its customers on both sides, this margin will decline under the force of competition. It can be shown that a reduction in the margin always lowers the required rate of return on capital and increases the intermediary's assets and liabilities.

A reduction in the supply of currency will, in this regime, as in the first regime, raise the required rate of return on capital. It will also raise the intermediary's rates. The existence of the intermediary does not, therefore, mean that monetary control is ineffective. However, it normally means that the control is less effective, in the sense that a dollar reduction in the supply of currency brings about a smaller increase in $\overline{R}_o$ when it can be counteracted by expansion of the intermediary. The possibility of substituting the intermediary's liabilities for currency offers a partial escape from the monetary restriction. But so long as the intermediary's liabilities are an imperfect substitute for currency, the escape is only partial.

Regime II: A Controlled Intermediary

The proposition that the intermediary weakens monetary control can be demonstrated by imagining that we can impose some quantitative restriction on the expansion of the intermediary. We can then compare the strength of monetary restriction in Regime II, with and without this quantitative control.

Assume, therefore, that the government's noninterest-bearing debt is divided into two segregated parts: currency held by the public and reserves held by the intermediary pursuant to a legal fractional reserve requirement. Assume further that this requirement is effective; i.e., that the aggregate size of intermediary liabilities permitted by the reserve requirement is smaller than the size which would result from unrestricted competition. When the reserve requirement is effective, the margin between the intermediary's lending and borrowing rates is greater than is needed to compensate for risk and administrative costs. Let the supply of currency to the public be reduced. In an uncontrolled Regime II, this will in certain circumstances lead to an expansion of intermediary assets and liabilities. In those circumstances, preventing such expansion by a reserve requirement will increase the effectiveness of monetary control. That is, a dollar reduction in the currency supply will raise $\overline{R}_0$ more if an expansionary response by the intermediary is prevented. There are also circumstances—probably less plausible—where monetary restriction would, in an uncontrolled regime, result in a contraction of the intermediary. In these cases, of course, control of the intermediary does not strengthen monetary control.

The example just discussed is a simple and artificial one. But the point it makes is of quite general applicability. In the more complex real world, currency and commerical bank liabilities are together subject to control via monetary policy, while the scales of operations of other financial intermediaries are not. The freedom of these intermediaries offers an escape from monetary controls over commercial banks, but only a partial escape. Likewise, the effectiveness of monetary controls would be enhanced if each nonbank intermediary was subject to a specific reserve requirement which would keep it from expanding counter to policies which contract commercial banks.

Regime III: Commercial Banking

The reserve requirement introduced in Regime II was expressed in terms of a specific government debt instrument, available only for this purpose and only in amounts determined by the government. The more familiar situation is that the reserve asset is, for all practical purposes, currency itself. Currency, that is, can serve either as a means of payment in the hands of the public, or as reserves for the intermediary. The government determines the total size of its noninterest-bearing debt, but its allocation between currency and reserves is a matter of public choice. It is, of course, this kind of reserve and reserve requirement that we associate with commercial banks—the most prominent intermediary. Indeed, the traditional business of banks is to accept deposit liabilities payable in currency on demand, and this obligation is the historical reason for banks' holding reserves in currency or its equivalent in "high-powered" money.

Let us consider, therefore, a third regime in which the one intermediary is a commercial banking system required to hold as reserves in currency a certain fraction of its deposit liabilities. Total private wealth is, as in the first two regimes, the sum of currency supply and the capital stock. Wealth-owners divide their holdings among currency, bank deposits, and direct ownership of capital. Banks dispose their deposits between reserves in currency and loans to borrowers, in proportions dictated by the legal reserve requirement. Borrowers hold that part of the capital stock not directly held by wealth-owners. So far as interest rates are concerned, there are two important variants:

A. Interest on bank deposits is competitively determined, and stands in competitive relation to the interest rate on bank loans. This relationship will depend on, among other things, the reserve requirement, which compels a bank to place a fraction of any additional deposit in noninterest-bearing reserves.

B. Interest on deposits is subject to an effective legal ceiling, and at the same time the reserve requirement normally restricts the banking industry to a scale at which the loan rate exceeds this fixed deposit interest rate by more than the competitive margin.

In this regime, there are two sources of demand for currency. One is the direct demand of the public. The other is the banks' reserve requirement; in effect, the public demand for deposits creates a fractional indirect demand for currency. This creates an interesting complication, as follows: The basic assumption about the portfolio behavior of wealth-owners is that assets are all substitutes for each other. Essentially the same assumption is applied to the behavior of borrowers. That is, a rise in the interest rate on any asset A induces, *ceteris paribus*, an increase in desired holdings of A and a decrease, or at most no change, in the desired holdings of every other asset. It is this assumption which enabled us to state unambiguously the direction of the effects of monetary actions in the regimes previously discussed. In the present regime the same substitution assumption still applies to the portfolio behavior of wealth-owners and borrowers. This means, among other things, that the public's direct demand for currency is assumed to decline, or at most not to rise, in response to an increase in the rate offered on bank deposits. But, of course, an increase in this rate also increases the demand for bank deposits. Thus it indirectly increases the demand for currency to serve as bank reserves.

It is certainly conceivable, especially if the required reserve ratio is high, that the indirect effect of an increase in the deposit rate outweighs the direct effect. In that event, currency and deposits are, taking account of public and banks together, complements rather than substitutes. This possibility is the simplest example of a very general phenomenon. Even though the substitution assumption applies to the portfolio choices of the public, and of every intermediary, taken separately, it is possible that assets will be complements in the system as a whole. This can happen whenever the public and intermediaries hold the same assets (currency, or government bonds, or other securities) or whenever one intermediary holds as assets the liabilities of another intermediary. Some of the implications of complementarity for both the stability of the system and its responses to various changes in parameters and in structure can be illustrated in the present regime.

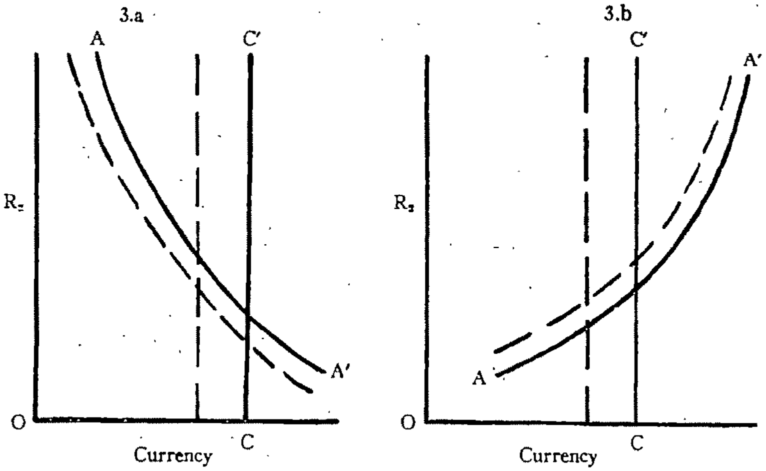

FIGURE 3

Equilibrium in Régime III may be depicted by considering separately the conditions of equilibrium in the market for currency and in the market for loans. In Figure 3 the supply of currency is shown as the vertical line CC'. The demand for currency, in relation to the deposit rate R_2, is the curve AA'. This includes both the direct and the indirect demand for currency. As noted above, this relationship may be either downward-sloping, as in Figure 3.*a* or upward-sloping, as in Figure 3.*b*. The upward-sloping case is that of complementarity. In each case the position of the demand curve depends on the level of R_o; the dashed curve represents a higher R_o, which tends to reduce the demand for currency. From the relationships involved in Figure 3 can be derived a locus of pairs of rates R_o and R_2 which equate demand and supply for currency. Such a relationship is shown in Figure 5 by the curve E_c. In the "substitutes case," corresponding to Figure 3.*a*, it is downward-sloping, as shown in Figure 5.*a*. In the "complements case," corresponding to Figure 3.*b*, it is upward-sloping, as shown in Figures 5.*b* and 5.*c*.

In Figure 4 the loan market is shown, the volume of loans on the horizontal axis and the deposit rate on the vertical axis. The supply of loans, *BB′*, is essentially the public demand for deposits, after allowance for the fractional reserve requirement. The demand for loans, *LL′*, is the amount of loan accommodation borrowers wish at various deposit rates, taking account of the fact that the loan rate systematically exceeds the deposit rate for the reasons already mentioned. The positions of the loan supply and demand curves depend on R_o, the rate of return on capital. A higher R_o shifts both curves upward, as indicated by the dashed curves. From these relationships a second locus of the two rates R_o and R_2 can be derived, the pairs of rates which equilibrate the loan market. This is the upward-sloping relationship E_L, also shown in Figure 5.

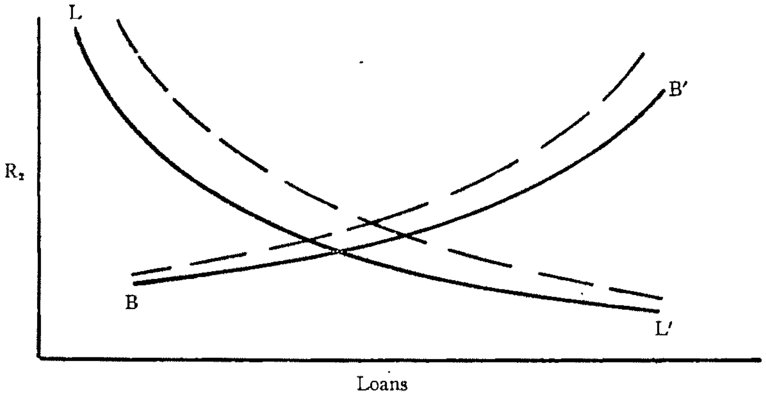

FIGURE 4

Three possible cases for the system as a whole are shown in the three parts of Figure 5: the first, or substitutes case, in Figure 5.*a*, a moderate complements case in Figure 5.*b*, and an extreme com-

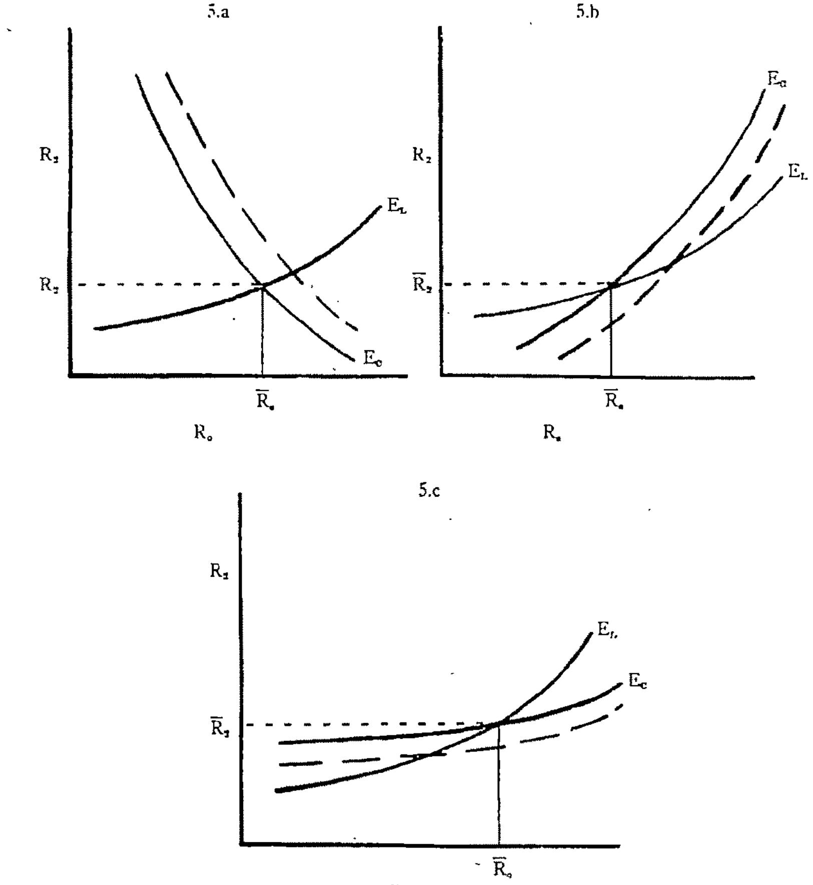

FIGURE 5

plements case in Figure 5.*c*. Now if there is a reduction in the supply of currency, equilibrium in the currency market can be maintained only by an increase in the rate on capital R_0 associated with any given deposit rate R_2. This can be seen by a shift left in the currency supply in Figure 3. Consequently the effect of a reduction in the currency supply can be shown in Figure 5 by a rightward shift in the curve E_c. In the first two cases, Figures 5.*a* and 5.*b*, this means an increase in both rates, as would be expected. However, in the extreme complements case, Figure 5.*c*, it indicates a decrease in both rates!

This implausible result arouses the suspicion that the solution indicated in Figure 5.*c* is an unstable equilibrium. We have examined the question of stability under the assumption that excess demand for capital leads to a fall in the rate of return on capital R_0, while excess borrowers' demand for loans, relative to the supply permitted by reserve requirements and depositor preferences, leads to a rise in loan—deposit rate R_2. The case exhibited in Figure 5.*c* is indeed unstable, while the other two cases are stable.

Consider now alternative (B), in which the interest rate on deposits is subject to an effective legal ceiling. The competitive link between the deposit and loan rates is broken by this regulation. In Figure 3, in other words, there is only one applicable level of the deposit rate. Consequently, there is only one rate on capital which is consistent with equilibrium in the "market" for currency. Figure 6 exhibits this situation. The currency equilibrium curve of Figure 6 is simply a vertical line; although the loan rate r_2 measured on the vertical axis of Figure 6 can vary, its variation does not affect either the deposit rate or equilibrium in the currency market.

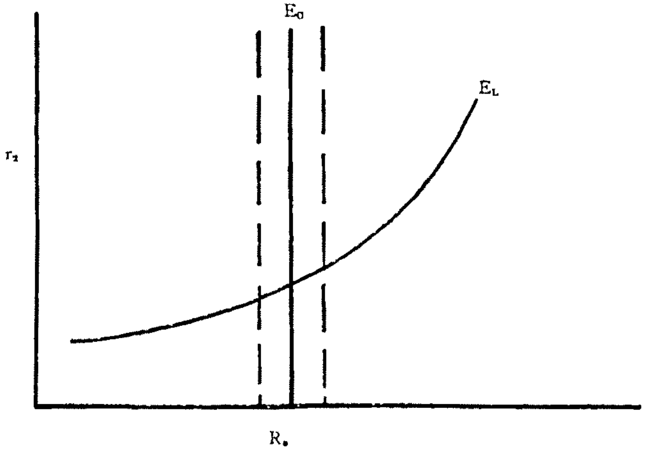

FIGURE 6

The effect of an increase in the controlled deposit rate depends on whether currency and deposits are, when both indirect and direct demands are taken into account, substitutes or complements. If they are substitutes, an increase in the controlled deposit rate will reduce the net demand for currency. Therefore, the rate on capital R_0 which balances the supply and demand for currency will be lower. In Figure 6, this means a movement to the left in the vertical line. In the new equilibrium both the rate on capital and the loan rate will be lower. An increase in the deposit rate is an expansionary monetary action.[5]

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[5] It is perhaps not too fanciful to refer, as an example of this kind of effect, to the consequences of the increases in Regulation Q ceiling rates on time and savings deposits in commercial banks permitted in 1961. Contrary to many predictions, these increases led to

If, on the other hand, currency and deposits are complements, the result of an increase in the controlled deposit rate is the reverse. Both the rate on capital and the loan rate are also increased. An increase in the deposit rate is a deflationary monetary action.

With a fixed deposit rate, a reduction in currency supply is always deflationary. This is true, of course, whether currency and deposits are substitutes or complements. But the question of real interest is whether monetary restriction is more deflationary—i.e., produces a bigger increase in the return on capital—when the deposit rate is flexible or when it is fixed. Monetary restriction will, in the flexible case, increase the deposit rate; in the other case the legal ceiling prevents this reaction. Now if currency and deposits are substitutes, an increase in the deposit rate is expansionary; it opposes and weakens the monetary contraction. But if they are complements, the reverse is true: flexibility in the deposit rate reinforces and strengthens quantitative monetary control.

Once there is a reserve requirement, variation in the required ratio is another instrument of monetary control. There are two questions to ask about such variation. The first concerns its effect upon the required rate of return on capital. The second concerns its effect on the strength of quantitative monetary control.

With a fixed deposit rate, an increase in the reserve requirement is always deflationary. With a given currency supply, the higher reserve requirement necessarily means that the public must curtail its holdings of currency or deposits on both. The only way they can be induced to do so is by an increase in R_o.

With a flexible deposit rate the same conclusion applies when currency and deposits are substitutes. However, it is conceivable, when they are complements, that an increase in the reserve requirement will be expansionary.[6]

We may now ask what is the effect of introducing or in general of increasing the reserve requirement on the strength of quantitative monetary control. The answer may depend on whether the deposit rate

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lower, rather than higher, mortgage lending rates; they were expansionary. Given the low reserve requirement against these deposits, especially when compared to demand deposits, it is to be expected that time and savings deposits are strong substitutes for currency and reserves.

[6] This may be seen in the following way: The initial effect of an increase in the reserve requirement may be divided into two parts: (*a*) the increase in the demand for currency and decrease in the supply of loans which result from banks' attempts to meet the higher reserve requirement; and (*b*) the increased margin banks will require between their deposit and loan rates when they have to place a higher proportion of deposits in non-interest-bearing reserves. The first of these effects is always contractionary. The second effect will also be contractionary in the substitutes case. In the complements case, however, the effect of increasing the margin between the rates is expansionary, and may even outweigh the first effect. At the same loan rate the deposit rate will be lower; as we have already noticed, lowering the deposit rate is expansionary in the complements case.

is fixed or flexible. With a fixed deposit rate an increase in the reserve requirement will decrease the response to changes in currency supply. This can be seen by imagining that uncontrolled Regime II is modified, first, by fixing the deposit rate and, second, by imposing a reserve requirement. With a fixed deposit rate and no reserve requirement, any reduction in the supply of currency is at the expense of the public's direct holdings of currency. The increase in the rate of return on capital necessary to reconcile the public to these reduced holdings of currency will also diminish their demand for bank deposits. But when banks hold no reserves, this cannot release any currency.

On the other hand, once banks are required to hold reserves, a contraction of bank deposits releases currency. Therefore, direct holdings of currency do not have to absorb the full reduction in the currency supply. Consequently, the necessary increase in the rate of return on capital is smaller.

When the deposit rate is free to rise, we would expect an increase in the rate in the wake of currency contraction to increase the volume of bank deposits. This expansion tends to offset the reduction in the public's currency holdings. Substitution of deposits for currency moderates the increase in R_o necessary to reconcile the public to smaller holdings of currency. When banks hold no reserves, this substitution can proceed without any brake. But when banks are subject to a reserve requirement, it can proceed only to the extent that the public is induced to give up additional currency to serve as bank reserves. Therefore, a reserve requirement means that a larger increase in R_o, the rate of return on capital, is needed to make the public content with a larger reduction in its direct holdings of currency.

This is the essential reason why regulations preventing or limiting expansion of the intermediary strengthen monetary control. It may be observed that such regulations are of two kinds: either a rate ceiling which prevents the intermediary from bidding for funds, or a reserve requirement, or both. Once there is an effective rate ceiling, however, increasing the required reserve ratio—though itself an effective instrument of control—reduces the effectiveness of a given change in the supply of currency. It is possible, even when the deposit rate is flexible, that bank deposits decline in response to a contraction of the currency supply. Then, just as in the case of a fixed deposit rate, increasing the reserve requirement diminishes the response of the system to such contraction. A reserve requirement is not necessary to prevent expansion of the intermediary from offering an escape from monetary control, because the intermediary would not expand anyway.[7]

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[7] This statement needs to be somewhat qualified to allow for the fact that a higher re-

TABLE 2

SUMMARY OF RESULTS FOR REGIME III

Currency and Deposits Are:	Substitutes	Complements	Extreme Complements
System is....................	Stable	Stable	Unstable
Increase in deposit rate is........	Expansionary	Deflationary	
Variation of currency supply more effective when deposit rate is...	Fixed	Flexible	

The principal results for Regime III are summarized in Table 2. These conclusions have been reached by adding one intermediary, banks, to a currency-capital model and then imposing rate and reserve regulations on banks. But they are illustrative of more general conclusions. In a many-intermediary world, similar propositions apply to the extension to nonbank intermediaries of the rate and reserve regulations to which banks are subject.

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serve ratio enlarges the competitively required margin between deposit and loan rates. This strengthens the contribution of a higher reserve requirement to the effectiveness of reduction in the currency supply, and makes it possible that this contribution is positive even when deposits do not expand.

DISCUSSION

Abba P. Lerner: Recent discussion of nonbank financial intermediaries has for the most part been concerned with the question whether their development and growth are hampering the efficacy of monetary policy. The pure economics of this would seem to be very simple if one may be permitted the use of a word that seems to have been studiously avoided in the submitted papers; namely, liquidity.

Monetary policy consists of using changes in the stock of money to bring about changes in the flow of spending in the economy. An increase in the stock of money provides an increase in the flow of liquidity that it yields and that is enjoyed by its holders. To the extent that liquidity and other economic goods are substitutes, the holders of the augmented money stock will endeavor to give up some of their liquidity for such other goods. They will spend or lend some of the extra money for this purpose. Since that does not decrease the quantity of money in the economy—it only changes hands—the process continues until the flow of spending has risen to an equilibrium level at which the augmented stock of money and the correspondingly augmented flow of liquidity no longer seem excessive (and, conversely, for a decrease in the money stock).

Defining a unit of liquidity as the liquidity flow provided by the possession of one unit of money stock (one dollar), we may say that a money substitute is any item the possession of which provides some liquidity, but less than one unit of liquidity per dollar's worth of the money substitute. Although a dollar's worth of the money substitute provides less than one unit of liquidity, it is still worth a dollar because it also provides some money income or some other benefit to its possessor.

Money substitutes constitute cushions that diminish the effects of a given change in the money stock if their supply increases in response to a tightening of liquidity (an increase in demand relative to the supply) and decreases in response to an easing of liquidity. A decrease in the money stock (with the corresponding decrease in the flow of liquidity it yields) has its effects dampened if it results in an offsetting increase in liquidity provided by an induced increase in money substitutes; and an increase in the money stock has its effects dampened if it results in an offsetting decrease in money substitutes.

The degree to which money substitutes lessen the effectiveness of changes in the money stock depends on their elasticity of substitution for money and on their elasticity of supply; i.e., on the limits to their acceptability as substitutes for money and on their availability. The greater the number and variety of money substitutes the greater are likely to be their over-all elasticities of substitution and supply. (If there is a growth over time in the number and quantity of money substitutes, there is likely to be a corresponding growth over time of their elasticities of substitution and supply and a corresponding weakening over time of the effectiveness of monetary policy.) But as long as these elasticities are not both infinite—that is, as long as it takes a finite change in the money prices of the money substitutes (i.e., in the appropriate interest rates) to induce the substitution or to induce a change in their supply—some of the effects of the change in money stock will remain. Unless the money substitutes are perfect substitutes for money (that is, say $2.00 of a money substitute is perfectly substitutable in unlimited degree for $1.00 of money plus $1.00 of other assets) and are in perfectly elastic supply (so that any absorption in substitution for money is completely filled on the same terms by additional supplies of the money substitute, and any release of the money substitute by an increase in the money stock is completely withdrawn from the economy), any desired effect on the rate of spending in the economy can still be brought about by monetary policy, although it will require a larger change in the money stock to bring it about the greater the elasticities of substitution and supply.

This remains true in spite of the possibility that the induced change in the quantity of money substitutes may be greater (in dollar value) than the opposite change in the money stock that induces it. The quantity of liquidity added or substracted by the induced change in the quantity of money substitutes must be less than that subtracted or added by the initiating change in the money stock, but since each dollar's worth of money substitute provides less liquidity than does a dollar of money, the larger change in (the money value of) money substitutes can constitute a smaller change in the quantity of liquidity.

The money substitutes are, of course, deposits with or the liabilities of financial intermediaries, while money frequently stands for the demand deposits with commercial banks, currency being abstracted from. It is, however, perfectly possible to define money more narrowly, calling only currency "money" and bank deposits "money substitutes" (as is done by Tobin and Brainard), or to define money more widely, including time deposits with commercial banks together with currency and demand deposits and counting only the liabilities of nonbank financial intermediaries as money substitutes (as is done, e.g., by Milton Friedman).

To summarize, the effects on the flow of spending of a given change in the stock of money is stronger (1) the less the degree to which the direct change in money liquidity is offset by induced opposite changes in money substitute liquidity (provided by financial intermediaries) which reduce the net change in liquidity, and (2) the greater the degree to which a change in net liquidity changes the flow of spendng. This in turn depends on: (*a*) the responsiveness of interest rates to changes in liquidity (i.e., the state of liquidity in the economy—how far the economy is from saturation with liquidity, the "Liquidity Trap," where the response is zero); (*b*) the responsiveness of spending (essentially investment spending) to changes in interest rates and loan availability (i.e., the state of confidence—how far the economy is from "Collapse of Confidence," when the response is zero).

It follows that any effective restriction on the ability of financial institutions to expand the supply of money substitutes in response to a tightening in liquidity (which would entail a reduction in their ability to contract the supply of money substitutes in response to an easing of liquidity) diminishes the force of the offsetting or cushioning factor in (1) above and thereby tends to strengthen the effect of a given change in the money stock.

With this sketch of the elementary economics of financial intermediaries as background I now turn to these three papers.

Professor Aschheim's paper seems at first to deal with a completely different problem; namely, whether the Federal Reserve's "defensive" concern for offsetting seasonal or other temporary fluctuations in the need for liquidity may not be preventing the system from dealing effectively with the "dynamic" responsibility for protecting the economy from inflation, depression, and stagnation. He asks whether there might not be a basic conflict involved in the use of the same instruments for dealing with both objectives, and suggests in effect that the defensive objective be abandoned.

It does not seem to me that any necessary conflict is involved on account of the objectives outnumbering the instruments. The use of the same steering wheel to avoid collisions does not prevent the driver from getting to his appointment. The maneuvering of a ship to avoid rocks or storms does not prevent it from reaching port. But there is a good reason for separating the responsibilities. Otherwise, the failure to achieve the dynamic objectives can be disguised or defended by claims that this was necessary for the defensive objective. And Aschheim makes a very good case for supposing that no very great harm could be done by abandoning the defensive objective altogether.

Aschheim points out that financial intermediaries seek their profits in doing precisely what is done by the Federal Reserve's defensive desk in trying to cushion the economy against fluctuations in the need for money. In taking advantage of differences between longer term and shorter term interest rates, the financial intermediaries moderate them, and thereby moderate the temporal fluctuations in the supply and demand for liquidity that give rise to the interest rate differences. This is the connection between Aschheim's paper and the nonbank financial intermediaries. In effect, he welcomes the proliferation of financial institutions. He is not concerned with their liabilities being substitutes for money, but he welcomes the financial institutions themselves as substitutes for defensive monetary policy which make it easier for the monetary authorities to free themselves for the dynamic responsibility.

Since I am neither anticapitalist nor antisocialist, but see much merit in competition, I see no objection either in defensive monetary policy being carried on by financial intermediaries for private profit or in speculation on the money market being undertaken by the Federal Reserve, stipulating only that the Federal Reserve do its speculation (or defensive monetary policy) at a desk other than the one concerned with the dynamic problem, so that there is no danger of the two objectives being confused. I would happily watch them compete and say, "May the better man win." (Or perhaps I should rather say, "May the worse method perish!")

There is, however, a presumption that the Federal Reserve would not be more efficient than the private intermediaries. For that the Fed would have to employ people who are better able to outguess the market than the private speculators. Such people could earn too much outside the Fed to stay in it very long. There is also the danger that the defensive or speculative desk, if it should indeed be more efficient than its private competitors, might come to measure its achievement by the profits it piled up on the books of the Fed. But the making of profits would constitute evidence that they were not carrying the speculation to the socially desirable point of eliminating the differences on which the profits rested—the point that would be reached by perfectly competitive private speculation—but were being ensnared by the temptations of monopoly profits.

In any case, the dynamic desk, or monetary policy proper, should be freed from all responsibility for offsetting such temporary changes in the need for money as could be known in advance—such as seasonal influences of weather or social customs. It should be free to concentrate on satisfying (1) secular changes in the need for money such as are due to growth of population or productivity or to developments in economic organization (including the growth of new financial institutions) and (2) unanticipated changes in the need for money that were not foreseen (or perhaps could not even have been foreseen) by the financial intermediaries or by the defensive desk.

The paper by Professors Tobin and Brainard is directed at the effects of various kinds of regulation of nonbank financial intermediaries on the effectiveness of monetary policy. Here I am tempted to reverse the common complaint of commentators that they got the paper too late. I got it too early, in the form of a very early draft in which I found a number of items I wanted to criticize. Unfortunately, the authors also found time to read it and to revise it ahead of me. I shall therefore limit myself to four comments on the final version.

1. It seems to me that a reaction to the historical neglect of nonbank financial intermediaries takes the form of an overemphasis on the similarities between them and commercial banks. The famous proposition that banks differ from other financial institutions in that they can "create money" and that this is why they have to be regulated while the others need not, is turned around because of this. The statement made that "it is more accurate to attribute the special place of banks among intermediaries to the quantitative restrictions to which banks alone are subjected than to attribute the quantitative restrictions to the special character of bank liabilities," amounts to saying that banks are not regulated because they create money; they create money because they are regulated.

It is true, of course, that if there were no restrictions on banks, one could no longer say that a reduction in restrictions would enable the banks to expand their activities, including the volume of their liabilities which constitute "money." But that would be only because they would already have expanded to the point where they no longer wanted to expand any further. It is precisely the special quality of the banks, or rather of the banking system, that in such an expansion it is quite peculiarly favored by being able largely to count on retaining possession of what it sells and thus to have it available for still further expansion. The explanation of this peculiar power lies, of course, in the banking system's covering such a large part of the financial transactions of the economy that, unlike a relatively small nonbank financial intermediary, it can count on its loans coming back to it somewhere as a deposit. But this "widow's cruse" aspect of the banking system is central to the issue. This is the early macroeconomics of the banking theory of money or credit creation that used to be so vehemently denied by outraged microbankers. And without the restrictions, the credit creation or money creation by the banking system would have been much greater than could have been absorbed without severe inflation. That is why even Milton Friedman recognizes the desirability of social control over the creation of money, and why it seems to me more appropriate to say that reserve requirements and other restrictions are imposed on the banking system because it would otherwise have created too much money. The restrictions are responsible for the power of the banking system to create money only in the sense that by preventing the actual creation of money it keeps the potential, the power, of creating money from being used up.

2. An apparent conflict with the elementary economics of financial intermediaries is provided by the interesting concept of complementarity between the demand for bank deposits (and its analogues) and the demand for currency, if one includes the indirect demand for currency as reserves for the deposits. This transposes all the conclusions of the elementary economics. A decrease in the supply of currency causes holders to turn to deposits as a substitute. As the supply of deposits increases in reponse to the increase in demand (for loans to be held as deposits), additional currency is demanded as reserves for the additional deposits. This accentuates the scarcity of currency and thus the existence of the money substitute and its elasticity of response to the decrease in currency apparently strengthens rather than weakens the power of monetary policy.

But there will be such an aggravated effect on spending only if the liquidity provided by the increased volume of deposits is less than the liquidity lost by the withdrawal of currency into additional reserves for the additional deposits. This would mean that the financial intermediary uses up more liquidity in its reserves than it provides by its money-substitute liabilities; i.e., that instead of being a creator of liquidity it is a destroyer of liquidity. That is why its response has the perverse effect. Since the only way a financial intermediary can survive is by its liquidity creation covering its operating expenses, we need hardly be concerned with this special case or with some other conundrums with which it presents us, such as the apparent implication of figure 5.*b* that a decrease in the volume of currency would call for a reduction in the deposit rate if R_0 (the "required return on capital") is to stay constant.

3. Much more important—and calling for separate treatment—is the completely different case of complementarity between currency and deposits (and its analogues) on the supply side. Where an increase in the supply of currency makes possible an increase in the supply of deposits by providing reserves, and where the liquidity provided by the volume of deposits made possible by a dollar of currency used as reserve is greater than the liquidity provided directly by a dollar of currency, then the deposits and the currency are truly complementary. The deposits may even be called amplifiers of changes in the supply of currency, changing in the same direction as the currency in response to changes in the supply of reserves rather than as dampeners of the effects of changes in the supply of currency, which change in the opposite direction in response to

changes in the demand for them as substitutes for currency. These seem to be the issues dealt with near the end of the paper, where they are not clearly distinguished from the case of complementarity in demand. If these cases are not covered by the elementary economics of financial intermediaries, it is only because they are already covered by the old elementary economics of money creation by banks.

4. My final point is purely heuristic. I refer to the device of relating changes in the money stock not to the rate of spending in the economy but to R_0, the required rate of return on the given capital stock. I can appreciate the aesthetic attractions of having a theory that runs entirely in terms of asset or stock analysis, but this only leaves for later the analysis of how dissatisfaction with stocks of assets held leads to the changes in flows of expenditure on consumption and investment that are the real concern of monetary policy.

It may be useful to recount the difficulties that I had myself on account of the use of this device. First, I was puzzled by how changes in the rate of interest could leave the quantity of capital constant—until I realized that some physical measure of capital was implied, and I found it much easier to follow the argument after I had turned it round, and instead of asking how much the required rate of return on capital must rise to make the economy content with a given reduction in the money stock, I asked how much the money stock must be reduced to prevent an increase in the return on capital from bringing about an increase in investment. I then was puzzled by the apparent assumption of a stationary society in which the marginal productivity or capital was equal to the rate of interest, so that there was an equilibrium stock of capital and no tendency to change it by net investment or disinvestment. But I realized that this was not really intended and that for "rate of return on the ownership of real capital" I could read "marginal efficiency of investment at that rate of investment that provided full employment or price stability or whatever is the objective of monetary policy." The next hurdle arose when I considered that the rate of return on old capital (at replacement cost) can be very different from the marginal efficiency of investment in newly invented types of capital until I remembered the custom of assuming capital to be homogenous in models of the kind in hand—which did not prevent me from beginning to lose interest in the model. Finally, I wondered what gymnastics would be required to permit an increase in the propensity to consume to be translated into an increase in the rate of return on the capital stock for the purpose of figuring the money stock change called for to restore equilibrium.

I am thus left with a very dim view of the benefits to be derived from attempting to squeeze the problems of monetary policy into a purely stock analysis. Sooner or later the transition has to be made from a stock to a flow if one is ever to get to deal with the effects of changes in the stock of money to changes in the flow of spending. To multiply the series of stocks affected beyond what is necessary is merely a pussyfooting postponement of the inevitable. It would seem to be much less painful to take the plunge right at the beginning and consider directly the effects of changes in the stock of money on the flow of spending.

The fascination with asset analysis also leads to other extravagant statements such as that "the essential function of banks and other financial intermediaries is to satisfy simultaneously the portfolio preferences of two types of individuals or firms." This is surely an extremely narrow definition of "function." The quotation correctly describes the mechanism, but the function surely is rather to facilitate saving and lending and borrowing and investing, even though these unfortunately are flows and not assets. And with all due respect to the current fashion of growthmanship, it is surely surprising to hear that "this required rate of return on capital is the basic criterion of the effectiveness of a monetary action." Does this so clearly make relatively insignificant the increase in spending that would absorb the $30-$50 billion slack in the U.S. economy? Nor are we permitted to allow common sense to creep in by redefining "the required rate of return on capital" to mean "the required rate of spending," for the sentence following the one just quoted drags us sharply back to the Keynesian classical full employment model, telling us that "to alter the terms on which the community will accumulate real capital—that is what monetary policy is all about."

The paper by Brunner and Meltzer promises to avoid Tobin and Brainard's concentration on assets and to consider the stock-flow relationships involved in considerable detail. This is a very difficult and very valuable enterprise. I was especially intrigued by this paper, as it seems to come to conclusions that are often quite contrary to those yielded by my elementary economics of financial intermediaries. Unfortunately, I shall have to wait until somebody translates the mathematics into English. Meanwhile, I cannot avoid the suspicion that the mathematics has failed to distinguish between complementarity in demand and complementarity in supply, and that this is the source of such results as that the growth of financial intermediaries increases the power of monetary policy—if that is what is meant by "the response of the financial system to expanding wealth (a shift . . . toward nonbank intermediaries) contributes to amplify the transmission impulse emitted by monetary policy."

It is perhaps too much to hope that the translator will also turn into less annoying English such expressions as "supplying liabilities" (lending money), "issuing liabilities" (accepting deposits, borrowing money), "incurring nonmarketable debt" (borrowing money), "transmission impulse" (impulse, transmission, the transmission of an impulse?) and "noninterest-bearing debt" (money). Or perhaps I shall have to accept the last as established terminology and retaliate by referring to national debt, or perhaps to debt in general, as "interest-bearing money." It might even turn out to be much easier for many people to consider the possibility that a growing economy might need some growth in the stock of "interest-bearing money" than to contemplate a deliberate government deficit.

Arthur Benavie: I shall confine the bulk of my remarks to the Brunner-Meltzer paper. Their model incorporates financial institutions into a wealth adjustment process which links the monetary impulse and aggregate demand. The role of intermediaries is broken down into two categories of portfolio responses to monetary policy: asset reallocations by the public involving intermediary claims and portfolio adjustments by the intermediaries themselves. The portfolio balance of the public is seen as disrupted by changes in total wealth produced by the impact of the monetary impulse on the structure of prices including interest rates. Changes in wealth are assumed to produce changes in the same direction in the ratio of financial to total assets and in the ratio of nonbank to bank assets. The larger the proportion of financial to total assets and the larger the wealth elasticity of the public's demand for financial assets the greater is argued to be the impact of monetary policy. Thus, the financial system helps monetary policy as far as this public behavior is concerned. With regard to portfolio adjustments by the intermediaries themselves, the major point made is that their impact on monetary policy depends upon the interest elasticity of their cash to asset ratio and upon the magnitude of the elasticity of the loan portfolio with respect to the loan rate compared to its elasticity with respect to security yield rates.

There are two points I wish to make with regard to this model, the second of which briefly touches on conceptual distinctions and statistical evidence which I submit are of general applicability to the problem of intermediaries and monetary policy. First, there are places in the exposition where behavioral relationships are not given a rationale, where logical relationships are not clearly spelled out, and where empirical connections are asserted but not supported. For example:

1. It is argued that the larger the ratio of financial to total assets, the greater the transmission impulse emitted by monetary policy. It is not explained whether there is simply a logical relationship between this ratio and the size of the impulse or whether there is some behavioral relationship involved.

2. It is argued that "variations in the relative importance of banks . . . affect the transmission impulse via the interest elasticities of the stock of financial assets. A declining proportion of bank deposits lowers this elasticity. . . ." It is not explained—and it does not seem self-evident—why the relative position of the banking system should be expected to have any particular effect on the specified interest elasticity. And what evidence or reasoning is there to support the assertion that the weight of the banking system is positively related to this elasticity?

3. It is asserted that the public's demand equation for financial assets is homogeneous of a degree greater than unity with respect to total wealth and that the demand for intermediary claims has a larger wealth elasticity than that of the demand for checking deposits. From this it is deduced that a "growth in wealth would thus be accompanied by a growing proportion of financial assets and a shift within the financial system towards nonbank intermediaries." These responses are presumed to strengthen monetary policy. Any increase in the wealth elasticity of intermediary claims relative to checking accounts clearly would strengthen monetary policy. But an increase in the wealth elasticity of demand for financial assets would not seem to, for it would induce a larger portfolio reallocation from nonfinancial to financial assets in response to an easy money policy, thus apparently weakening the transmission impulse.

4. No evidence is presented to support the behavioral postulates regarding the wealth elasticity of the public's demand for financial assets or the relative wealth elasticities of checking deposits and intermediary claims. There is also no discussion of the theoretical underpinning for these hypotheses. And if one is concerned with contracyclical monetary policy, it would seem of concern to probe the question of lags as well as of causation.

My second point regarding the Brunner-Meltzer paper deals with the scope of monetary policy's impact on the public. They argue that "the response of the rate structure to policy actions modifies the public's allocation pattern bearing on financial and nonfinancial wealth. This modification operates via changes in total wealth. . . ." I question why other responses of the public vis-à-vis intermediary claims which seem plausible and which appear to have potential implications for the strength of monetary controls are not given the same a priori seat of honor in their model as this wealth effect. My argument for a broader scope than is found in their model is as follows: An implicit assumption of this model, as of the Tobin-Brainard model, is that portfolio adjustments which have implications for the strength of monetary policy are those adjustments which occur in response to monetary policy. This feedback assumption constitutes the seed, I submit, of conceptual distinctions which are essential to the structure of any model designed to analyze the various channels through which intermediaries impinge on the effectiveness of monetary controls. This assumption implies that it is important to distinguish between portfolio reallocations which are a response to the variation in variables induced by monetary policy and portfolio reactions motivated by the fluctuation in variables which is not induced by monetary policy. This latter category of asset shifts has no necessary implication for the impact of a given dose of monetary tightness or ease since these portfolio responses are by definition a *ceteris paribus* with respect to the impact of monetary policy. The former category, i.e., asset switches motivated by monetary policy, would appear to have necessary effects on monetary policy to the extent that they exert an influence on aggregate demand through affecting the total supply and cost of loanable funds. This distinction implies that cyclical fluctuations in the flow of funds to intermediaries (which are largely portfolio reallocations by the public) have implications for monetary policy which cannot accurately be interpreted except by breaking this flow of funds down into its basic components; i.e., into the various portfolio adjustments. These asset adjustments can then be diagnosed as to their meaning for monetary policy by assessing how the variables motivating them are linked to monetary policy and by calculating the effect of the asset shift on the total supply of credit.

In pursuing this kind of investigation, this discussant has constructed a statistical equation explaining the flow of funds to intermediary claims. The predictors in this equation are—with the exception of personal saving—variables which appear to motivate portfolio adjustments by the public. Using quarterly data, the flow into intermediary claims appears to be significantly correlated with the following eight independent variables: the interest rate on intermediary claims (positive), the interest rate on corporate bonds (negative), the interest rate on Treasury bills (negative), the rate of change in stock prices (negative), the growth rate of GNP (negative), the change in the money supply (positive), personal saving (positive), and the stock of individual indebtedness (negative).

The most important portfolio adjustments by the public suggested by these results appear to be the following ones:

1. Shifts between money and intermediary claims in response to changes in the interest rate paid on these claims. This behavior pattern weakens monetary policy. A tight monetary policy, for example, would increase the intermediary interest rate (after a lag of two quarters or more) which would pull funds into intermediary claims thus increasing the flow of credit from the financial system.

2. Shifts between intermediary claims and money in response to a change in the availability and cost of credit. This behavior strengthens monetary policy. A tight monetary policy, for example, by reducing the availability and increasing the cost of credit, would motivate an asset switch from intermediary claims into money and thus reduce the credit flow from the financial system. The positive regression coefficient on the money supply and the negative coefficient on the GNP (which affects the transactions demand) appear to support this hypothesis.

3. Shifts between intermediary claims and interest-yielding marketable securities in response to changes in the differential between these rates and the interest rates on intermediary claims. This response would appear to strengthen monetary policy to the extent that funds move between commercial bank time deposits and money. A tight money policy, for example, would raise primary security rates relative to the intermediary claims rate which would draw funds from time deposits into primary securities (thus into demand deposits) which would increase the credit flow from the public by more than it would reduce the credit flow from the banking system. The negative regression coefficients on both the bond and the bill rates are consistent with this hypothesis.

These results allow rough calculations to be made as to the size of the effect of each portfolio adjustment on the strength of monetary controls. This requires a measure of the variation in each predictor variable which is generated by contracyclical monetary policy. From the statistical equation the effect on the flow of funds to intermediaries produced by this variation in the motivating variable can be estimated. Finally, the effect of these flows to intermediaries can be translated into changes in the total supply of credit by incorporating assumptions about the reserve ratios behind demand deposits and the intermediary claims.

As a final point, the two models presented here focus on the effects of a given dose of monetary policy, as do my own investigations. The over-all effectiveness of monetary policy has perhaps another dimension as well: the size of the dose that is felt can be administered within a given time period. The monetary authorities appear to feel that during a period requiring a tightening monetary policy there is a constraint on the scale of their operations due to the possibility of congestion and panic in the bond market. Scarcely any discussion can be found in the literature of the impact which intermediaries exert, if any, on this dimension of the effectiveness of monetary policy. A complete analysis would appear to require at least a consideration of this question.

HYMAN P. MINSKY: The Aschheim and the Brunner-Meltzer papers are at the opposite ends of the spectrum covered by monetary analysis. Aschheim's paper deals with the techniques and objectives of Federal Reserve action whereas the Brunner-Meltzer paper sets out a formal framework for the analysis of interrelations among real and financial stocks and flows. In addition the two papers differ in that (1) Aschheim emphasizes money markets intermediation (by government bond houses, consumer credit houses, etc.) whereas Brunner-Meltzer emphasizes savings intermediation (by savings banks, insurance companies, etc.) and (2) the evolutionary and adaptive properties of financial institutions are basic for Aschheim's argument but are not essential for Brunner-Meltzer.

Aschheim's view is that the Federal Reserve's use of open market operations in the simultaneous pursuit of defensive and dynamic objectives is a mistake, for the operations dictated by defensive objectives get in the way of the successful achievement of dynamic objectives. He suggests that open market operations should be reserved for those changes in the reserve base dictated by the growth and stability objectives of monetary policy and the adaptive properties of the money and capital markets be relied upon to achieve a self-adjusting money market with respect to those disturbances that now lead to defensive operations.

I am sympathetic to this view. The very active, nay busy, intervention by the Federal Reserve in the money market has not resulted in any noticeable improvement in the efficacy of monetary controls: it cannot be demonstrated that the fuss and feathers is really necessary. Complex financial markets have functioned very well without such daily activist intervention by the central bank.

The high-level busywork of the Trading Desk can be rationalized only by a belief that the money market is inherently unstable so that if the operators did not "lean against the wind" a classical financial crisis would result. The ability of the money market financial intermediaries to vary the efficiency of a reserve base by exploiting pockets of liquidity is evidence that this belief is not valid. Assuming that the Federal Reserve System would behave "like a central bank should" if a financial crisis did develop, the unnecessary defensive operations are seen as premature lender of last-resort operations.

To achieve a self-adjusting money market with respect to transitory changes it is not enough to rely upon the adaptive powers of financial institutions. The adaptive changes must be backed up with appropriate modifications of Federal Reserve practices. Once open market operations are reserved for dyamic and lender of last-resort purposes, major banks can be expected to use both the money market intermediaries and the discount window to adjust their reserve position. Under these circumstances financial stability requires that the money market intermediaries have a guaranteed source of refinancing. To achieve this it is necessary for the Reserve System to open the discount window to the money market intermediaries (repurchase agreements being ruled out as a form of defensive open market operations). Once this step is taken the Federal Reserve System can create a market in which the discount rate is a penal rate by closing the window to the money market banks. "Going British" may be a corollary of the reforms advocated by Aschheim.

Brunner-Meltzer emphasize balance-sheet adjustments as generating stock-flow price differentials which in turn generate investment and consumption demand: flow magnitudes "emerge as adjustment flows in a wealth allocation process."

The wealth adjustment process is stated in a model that has some four sectors and thirty-seven equations. When confronted with a large model it is easy to ask why not one more sector or equation. In the present case this is not a form of academic gamesmanship; rather, it centers around a fundamental issue of research strategy. It seems particularly inappropriate to consolidate households and nonfinancial firms into a public sector in a model that is designed to examine the significance of financial factors in determining system behavior. After all, households are the basic surplus sector and nonfinancial firms are the basic private deficit sector in our economy.

Brunner-Meltzer ignore the flow implications of the stock of financial instruments. Every financial contract specifies that money flows will take place at particular dates or under defined circumstances. The stock of private liabilities determines a money-flow constraint upon private decisions: each liability emitter attempts to arrange his receipts and spending so as to be able to meet his commitments. In particular, the greater the payment commitments relative to expected receipts the greater the reluctance and the smaller the ability of a private unit to finance its operations by deficits. During a protracted expansion dominated by household and business deficits the ratio of household and business financial commitments to income rises, whereas in an expansion dominated by government deficits the ratio of private commitments to income decreases. Hence the conclusion that there is a "deflationary feedback via stock magnitudes of a sustained [government] deficit financed by issuing interest-bearing debt" is not true if the effects of accumulated government and private deficits in generating demand are compared.

Brunner-Meltzer argue that the relative price of equities and investment goods is the major determinant of business investment. The recent stock market boom was associated with a plateau rather than a rise in business investment. The recent past seems to support a flow interpretation of investment demand, that investment demand is related to excess capacity, rather than an explanation centering around the price of equities and investment goods, and may indicate that equity prices are more closely related to consumption than to investment demand.

Brunner-Meltzer find that whether financial intermediaries cushion or amplify the repercussions from a change in base-money depends on the value of various income and wealth elasticities. Any proposition that depends upon the value of elasticities or other parameters naturally leads to a deeper question: What economic phenomena determines the value of these parameters? With respect to financial variables, the answer is clear: The parameters are determined by the structure of financial institutions. As financial institutions change, the value of the parameters change. Therefore the efficacy of monetary policy at any time depends upon whether changes in financial institutions and practices will occur in response to money market developments and whether they will tend to offset or reinforce central bank operations.

DEFENSE AND DISARMAMENT

ECONOMIC ANALYSIS IN THE DEPARTMENT OF DEFENSE*

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Introduction

Traditionally, the economics of national defense has meant the study of economic war potential and the problems of mobilizing it to produce armaments. During the fifties, the range of national security problems treated by economists was broadened to include many topics quite unrelated to fiscal policy and monetary theory. The economics of national defense now includes studies of the organization and management of the Defense Establishment and the armaments industries, and it includes economic analysis of the requirements for weapon systems and forces; that is, the central issues of defense policy and programming. Although much of the research on these problems has been classified, enough outstanding unclassified examples have appeared to provide the economics profession in general with a good survey of the range of topics being studied.[1]

In 1961, a group of economists who had been specializing in this field joined the Department of Defense in positions which gave them the opportunity to take the initiative in applying the results of their research to the practical problems of policy planning and financial management in the Department. This paper is a progress report on some of their work.

Of course, let me emphasize, the principal military advisers to the Secretary of Defense are the Joint Chiefs of Staff. Military planning is the responsibility of the Joint Staff and the planning organizations of the military departments. However, the Secretary of Defense has the responsibility to see to it that the plans and programs of the Department of Defense are selected with proper attention to considerations of economy and efficiency, and in this he is assisted by, among others, the economists on his staff. Although this paper is about

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[1] See, for example, C. J. Hitch and R. N. McKean, *The Economics of Defense in the Nuclear Age* (Cambridge, 1960). Henry S. Rowen, *National Security and the American Economy in the 1960's* (G.P.O., Washington, 1960). M. J. Peck and F. M. Scherer, *The Weapons Acquisition Process: An Economic Analysis* (Boston, 1962). J. A. Stockfisch, ed., *Planning and Forecasting in the Defense Industries* (Belmont, Calif., 1962).

the work of economists, I do not want to suggest that we think we are doing the whole job ourselves.

I believe that it is fair to say that the economists in the Defense Department have been successful in making economic analysis an effective contributor to the efficiency of the Department's program. Such basic concepts as marginal products and marginal costs have made it possible for the economists to summarize and integrate a great deal of critical program data and other diverse kinds of information for the few key officials who must make the major decisions on resource allocation. Moreover, although we have learned much from the experience, I believe that our earlier research has stood up well under the practical tests of realism and relevance.

Economic analysis within the Department of Defense is now as diverse as economics itself. It covers many areas. We have, for example, problems of industrial organization in administering the $10 billion weapons industry which the Department of Defense has created. Determining rates of military compensation raises all the traditional problems of the labor market. The Defense Department must consider the substantial impact of its expenditures on the U.S. balance of payments. Rather than cataloguing all of the activities in which economics, or more accurately economists, have proved useful, I have chosen to focus in this paper upon the problems of determining requirements for weapon systems and forces; that is, the allocation of the defense budget. While this application of economics is relatively novel, I hope to make clear that economic analysis here is simply traditional marginal analysis, compounded, however, by real life uncertainties, data problems, problems of complex interactions between many diverse elements, and the continuing dialogue with the politically and legally responsible decision-makers.

The Programming System

Before economic analysis could be applied systematically to the problem of allocating the defense budget, it was necessary to install a financial management system that provided the right kind of information for top-level decision making. The system we found did not do this. It had several important defects, perhaps the most important of which was the almost complete separation between planning and decision making on weapon systems and forces, on the one hand, and budgeting on the other. What Arthur Smithies found in 1954 was still true six years later. To use his words: "Planning and programming precede budgeting, and programs provide the basis on which budgets are prepared. Programs, however, are prepared in terms of military concepts and not in terms of dollars. When a program is completed, the cost in dollars is not known."[3] In other words, the long-range plans for weapon systems, forces, and all of their supporting elements were made by the services on the basis of their estimates of the forces required to assure our national security. Generally speaking, costs were not introduced systematically, either to test the feasibility of the whole program or for purposes of evaluating the efficiency of allocation.

Budgeting, on the other hand, had as its point of departure the guideline dollar totals laid down by the Administration and based on estimates of the burden the economy could or should bear. The result was a gap. The "required forces" always cost much more than the Administration and the Congress were willing to pay. The process by which the conflicting interests were resolved was unsystematic and wasteful because it led to unbalanced programs.

Furthermore, the Secretary of Defense did not receive adequate cost data. The budgetary system identified costs by object classes—Procurement, Military Personnel, Installations, etc.—the inputs to the Defense Department, rather than by weapon systems and forces, such as B-52 wings and Army divisions which are the tangible outputs of the Department. The identification by object classes is suitable for purposes of appropriations and for management of a program already decided upon, and we have retained it for those purposes, but it is not suitable for top-level decision making on such questions as the proper mix between B-52 wings and Army divisions. Moreover, cost data were presented and financial management was conducted at the Defense Department level on a year-at-a-time basis. The full time-phased costs of the proposed forces were not presented to the Secretary of Defense. Because the costs of most programs are small in their first years, this led to the starting of many programs that could not be completed at anything like existing budget levels. Although a certain amount of this is a desirable hedge against uncertainty, it is clear that there were a great many wasteful stretch-outs and cancellations of programs that would not have been started if the costs of all of the approved programs had been anticipated.

Another problem was that the cost data that were presented when a program was being "sold" to the Secretary of Defense were not related in any systematic way to the costs used for budgeting. This meant that the services had a very powerful incentive to understate the costs, without the offsetting incentive of knowing that they would be held accountable for the same estimates when it came to budget preparation. Still another problem with the cost data was that the estimates that were made of the marginal costs of the various programs were incomplete and generally very low. For example, in 1961 the Congress appropriated a half billion dollars to buy another wing of B-52's, apparently in the belief that that appropriation would "pay for" the wing. As the Secretary of Defense pointed out in his announcement of his decision not to procure the extra bombers, the half billion represented only procurement costs. The total cost to buy and operate the bombers and their associated equipment for a period of five years would be three times that much. The understatement of marginal costs has a sort of "Duesenberry effect"; it makes increases appear to be relatively attractive and it makes program reductions appear to be unrewarding in terms of savings.

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[3] *The Budgetary Process in the United States* (New York, 1955), p. 241.

There were other problems also. Program decisions were made largely unilaterally on a single-service basis. For example, the Navy and the Air Force decided on their long-range ballistic missile programs largely independently of each other. To the extent that there was interaction, it was more competitive than co-operative.

Finally, the system did not provide data on marginal utilities or marginal products. The traditional military requirements study was typically a calculation of the forces required to achieve a single hypothesized objective. To give an oversimplified example, suppose that the objective were to achieve an expectation of destroying 97 per cent of 100 targets, using missiles having a 50 per cent single-shot kill probability. The requirements study would conclude that 500 missiles were required, without pointing out that the last 100 missiles only added an expectation of killing about 3 extra targets. We are now finding that it is just the latter kind of information that is required to close the gap I referred to earlier.

To correct these deficiencies, we established the Programming System. Reduced to simplest terms, the Programming System consists of a five-year projection of all forces, weapon systems, and other activities described in physical (or nonfinancial) terms, together with their costs, all as approved by the Secretary of Defense, plus a set of regular procedures for modifying the plan.

The key to the Programming System is decision making by program elements and major programs; that is, by the outputs of the Department rather than by the inputs. A program element is an integrated activity combining men, equipment, and installations whose effectiveness can be related to our national security policy objectives. The list includes B-52 wings, infantry battalions, and combatant ships, taken together with all the equipment, men, installations, and supplies required to make them effective military forces. The program elements are assembled into major programs which contain interrelated elements which closely complement each other or are close substitutes for each other and which must therefore be considered together in arriving at top-level decisions. The entire defense program is now divided into major programs, as follows: (1) Strategic Retaliatory Forces, (2) Continental Air and Missile Defense Forces, (3) General Purpose Forces (primarily intended for the nonnuclear defense of overseas theaters), (4) Airlift and Sealift Forces, (5) Reserve and Guard Forces, (6) Research and Development, (7) General Support, (8) Civil Defense, and (9) Military Assistance.

The key point about a program element is that it has both costs and benefits associated with it. The benefits are the ways in which it helps us to achieve broad national security objectives. The costs are primarily the appropriations in all categories that will be required for its execution, not only in the current year, but throughout the lifetime of the program.

The Programming System has made it possible to unify program decisions and budget decisions. Budgets are still prepared annually in much the same way as before. However, the Five-Year Force Structure and Financial Plan is used as guidance to the military departments for the preparation of their budgets. During the annual budget review, the approved five-year plan is translated into requests for appropriations, by object classes, and there is a detailed review of the cost estimates and the manner in which the approved program is being carried out.

The programming approach is based on an over-all Department of Defense look at national security requirements, not a single service approach. Decisions about Polaris and Minuteman, Army divisions and tactical fighters, and other closely related programs supported by different services are made by the Secretary of Defense. This has made possible better balanced forces for the Department as a whole.

The Programming System frees the Department from year-at-a-time decision making, and forces us to look systematically at our plans and programs over a longer period of time. For defense planning, a year is arbitrary and short. The leadtime for most weapon systems is at least several years. The decisions that were made in 1961 about the fiscal 1963 budget will not have their full impact on our military posture until the period 1965 to 1967. We need an economically realistic plan for the future, so that long-lead decisions on program components will have a reasonable chance of turning out to be right.

Finally, and perhaps most important, the Programming System has formed the foundation for a systematic approach to the problem of reconciling "military requirements" and "reasonable budget levels." In the Programming System, costs and benefits are looked at together at the same time and by the same people rather than at different times and by different people. What is required militarily is looked at in the light of its costs, and estimates of what the country can or should afford to pay are considered in the light of the requirements of national security. Estimates of marginal costs and marginal products are provided for the Secretary of Defense, so that he and his principal advisers can concentrate their attention not so much on such questions as whether we need a capability to destroy 97 per cent of the 100 targets, with the alternatives unspecified, as on such questions as whether the capability to raise expected target destruction from 94 to 97 per cent is worth the cost of 100 extra missiles. Of course, data on marginal costs and marginal products do not imply mathematically what the number should be. We have no equivalent to the point at which marginal cost equals marginal revenue. A complex judgment still has to be made. But data of this kind contribute a great deal to making that judgment an informed one.

The Economics of Our Posture for Thermonuclear War

The U.S. military posture for thermonuclear war includes our Strategic Retaliatory Forces, Continental Air and Missile Defense Forces, our Civil Defense Program, parts of our theater-based forces, plus research and development and weapon production programs to support them. An economic theory is required to allocate resources among these components.

By 1961, a great deal of progress had been made in the development of an economic theory for our posture for thermonuclear war. Many possible objectives had been explored, the important criteria had been clarified, and a reasonably good understanding had been developed of how the various elements of the program are related to the objectives.[3] It should be emphasized, however, that although the analytical methods were developed to a point of high sophistication, the theory rests on very little empirical data, and hopefully will remain so. There is no lack of awareness of this point among top-level policy-makers in our government.

Briefly stated, the objectives sought for our posture for thermonuclear war are twofold: First, we seek to make war unlikely; and, second, if despite our best efforts a war should occur, we seek to limit the damage caused to U.S. and Allied population and industry and to put ourselves in a position to achieve a speedy and favorable termination of the war. For the most part, these objectives can be translated into the quantitative criteria of damage to U.S., Allied, and Sino-Soviet population and industry and the balance of surviving military forces occurring under various different circumstances of outbreak and conduct of the war.

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[3] See, for example, A. J. Wohlstetter, "The Delicate Balance of Terror," *Foreign Affairs*, Jan., 1959; H. S. Rowen, *op. cit.;* and Herman Kahn, *On Thermonuclear War* (Princeton, 1960).

Although there is obviously much more to the problem of thermonuclear war than economic analysis of efficient and inefficient postures, I will comment here only on two lines of advance that we have made in the past two years in the economics of the problem. First, we have made a great deal of progress in the translation of our broad objectives into specific quantitative criteria that can be applied in a systematic and practical way to the evaluation of alternative proposed forces and postures. This has been a matter of translating research results into practical operating procedures.

Second, prior to 1961, very little was done on the questions of budget level and total requirements. Economic research was limited almost entirely to questions of allocation within existing budgets and force levels. Although the problem of determining the budget level cannot be separated altogether from the efficiency of allocation, it was easier for economists to assume a given budget level and address exclusively the question of efficiency of allocation. However, when the Secretary of Defense made it clear that he was not going to begin with a predetermined budget level, but rather was going to address the problem of requirements directly, it became necessary to change somewhat the focus of the analysis. Instead of working in terms of marginal rates of transformation and substitution, in effect we switched to marginal products and marginal costs. This allowed the Secretary to make his own judgments as to the point at which the various marginal products were no longer large enough to justify the incurring of extra costs.

Conventional Force Requirements

Although the determination of conventional (i.e., nonnuclear) force requirements, in the sense of forces required to do a particular well-defined task, has traditionally received thorough study by the military profession, the broader problems of relating alternative conventional force levels to national security objectives and of determining the over-all U.S. conventional force levels had not received much systematic treatment. Of course, as in the case of our nuclear forces, nonmilitary considerations such as those of foreign policy and domestic economic policy and political factors must enter into the decision.

There are several factors that make the economic analysis of conventional force requirements a very different kind of problem from the economics of our posture for thermonuclear war. First, the uncertainties appear to be much greater and intangibles play a larger part in the case of conventional forces. I say "appear" to be "much greater" because it is altogether possible that our ability to put quantitative limits on the effects of nuclear weapons is based on ignorance of some crucial aspects of the problem. On the other hand, the United States has fought many nonnuclear wars. From the study of these and many other wars, the military profession has developed emprical generalization about the effects of various force ratios, weapons, terrain, and other factors. But, as they are quick to point out, the averages are surrounded by very wide variances. The uncertainties are large because of the critical importance of such factors as morale, leadership, surprise, etc., and the fact that the outcomes of conventional land-air battles are the product of very large numbers of complex interacting elements.

Another important difference is that the specific objectives sought by conventional forces lend themselves much less easily to quantitative treatment than do the objectives of our posture for nuclear war. The economic theory of our posture for nuclear war can be described in terms very similar to the economic theory of a multiproduct firm. There are smooth continuous rates of transformation and substitution and almost ubiquitous diminishing marginal returns. The objectives for conventional forces must generally be defined in terms of seizing or holding specific pieces of terrain in or for specified periods of time against specified enemy forces.

The approach we have adopted has been to formulate, in collaboration with the military planners, a range of carefully defined limited war problems in each of the various theaters in which our security requires a capability for major military operations. The questions are of the form, "What is the minimum amount of forces that would be required to hold, with high probability, for X weeks (perhaps indefinitely) at the Y line against an attack by Z forces?" After examining various limited wars in each theater, mixes of wars in two or more theaters are examined. Then alternative forces, together with their required logistic support, are assembled to deal with the various assumed limited wars, their costs estimated, and the results presented to the Secretary of Defense. Although much remains to be done, these military-economic studies have given the civilian leadership a much clearer picture than they had before of over-all force requirements to implement the desired strategic objectives.

Practical Ways of Dealing with Uncertainty

I have referred several times to the numerous and major uncertainties present in this kind of work. There are good ways and bad ways of dealing with the problems of decision making under uncertainty. Economists have the advantage of familiarity with an extensive and sophisticated literature on the subject. I would like to outline here some of the practical quantitative methods we have adopted to deal with uncertainties in our work.

One encounters many different kinds of uncertainties in empirical studies of weapon systems and defense policy. Most of them fall into the following categories. There are uncertainties about operational factors such as the accuracy and reliability of missile systems, the hardness of targets, or the range of future aircraft. These are closely related to uncertainties about the time and cost to develop weapon systems. There are uncertainties about enemy behavior, his weapon systems and forces, and the behavior of other countries. How many ICBM's will the Soviet Union have in 1968? Will country X continue to grant us base rights in 1967? There are also conceptual uncertainties. How will the weapon systems we are buying now for the late sixties be related to our national security objectives then? If past experience is a reliable guide, it is almost certain that our strategy will change between now and then. If we fail to anticipate the change and allow for it now, our weapon systems may be quite inappropriately designed for the strategy of 1968, and the deficiency may have to be remedied at great cost later on.

Without attempting to give a complete catalogue of the tricks of this trade, let me indicate some of the methods and rules we follow.

First, there seems to be a widespread belief that the safe thing to do, in cases of doubt, is to overestimate one's opponent and underestimate one's own capabilities. If uncertain about the reliability of the enemy's missiles and ours, so this belief would have it, one should pick from the high end of the range of uncertainty for the opponent's and from the low end for our missiles. In fact, we have learned that it is just as dangerous to overestimate the enemy's capabilities relative to our own as it is to underestimate them. Overestimates do not necessarily lead to insurance and safety. They are just as likely to lead to despair, to pricing of important policy objectives out of the market, and to strategies of desperation.

Next, we have found that in cases of uncertainty, it is often useful to carry three sets of factors through the calculations: an "Optimistic" and a "Pessimistic" estimate that bracket the range of uncertainty and a "Best Estimate" that has the highest likelihood. These terms are not very rigorous. A subjective judgment is required. But it is surprising how often reasonable men studying the same evidence can agree on three numbers where they cannot agree on one. In fact, one of the great benefits of this approach has been to eliminate much senseless quibbling over minor variations in numerical estimates of very uncertain magnitudes.

I pointed out earlier that the relationship of marginal products to marginal costs is usually of central importance in determining force levels. Interestingly enough, we have found that it is frequently the case that although there are major uncertainties about total products and costs, the marginal products and costs will be much less sensitive to variations in the factors.

In the case of uncertainties about enemy behavior, we have often found it useful to play a simple two-person game in which the enemy is permitted to adjust to variations in our posture, in order to evaluate our own alternatives. Of course, thinking through the enemy's countermove is a time-honored part of military procedure. The trick, in the long-range planning business, is to apply realistic technical, budgetary, lead-time, bureaucratic, and other constraints to the enemy's hypothesized freedom to react. A frequent error here is to allow him too much freedom to adjust; the consequences are those of overestimating the opponent, described earlier.

With respect to conceptual uncertainties and uncertainties about strategic context, another error to be avoided is becoming locked in tightly by current strategic concepts and assumptions. We therefore make a conscious effort to evaluate alternative postures under a wide range of different hypothetical future circumstances and policies. Although this procedure doubtless is not nearly as good as actually knowing the future, it does help our decision-makers to build into our posture a certain amount of flexibility that might not otherwise be included.

Conclusions

I would like to conclude with three general observations.

First, the tools of analysis that we use are the simplest, most fundamental concepts of economic theory, combined with the simplest quantitative methods. The requirements for success in this line of work are a thorough understanding of and, if you like, belief in the relevance of such concepts as marginal products and marginal costs, and an ability to discover the marginal products and costs in complex situations, combined with a good quantitative sense. The advanced mathematical techniques of econometrics and operations research have not proved to be particularly useful in dealing with the problems I have described. Although a good grasp of this kind of mathematics is very valuable as intellectual formation, we are not applying linear programming, formal game theory, queuing theory, multiple regression theory, nonlinear programming under uncertainty, or anything like it. The economic theory we are using is the theory most of us learned as sophomores. The reason Ph.D.'s are required is that many economists do not believe what they have learned until they have gone through graduate school and acquired a vested interest in marginal analysis.

Second, I observed earlier that economic analysis cannot determine the optimum allocation or size of the defense budget. It stops with informing the legally constituted officials who have to make the critical judgments. Because the range of possible alternatives is so great, economic analysis in the Office of the Secretary of Defense has become a continuing dialogue between policy-makers and economists, in which the policy-makers ask for alternative solutions to their problems, make decisions to exclude some in order to focus attention on the most interesting candidates, and make value judgments and policy decisions, while the economist attempts to clarify the conceptual framework in which decisions must be made, to define alternative possible objectives and criteria, and to explore in as clear terms as possible (and quantitatively) the cost and effectiveness of alternative courses of action.

Finally, the range of interesting and important problems urgently requiring the attention of economists is very great. As well as the many unsolved problems remaining in the area of weapon systems and force requirements, we have problems of personnel compensation, requirements and utilization, problems of internal organization and pricing, inventory management, contracting for research and development and procurement, and many others. For economists interested in the field of national security, the Defense Department offers excellent and exciting opportunities for public service.

TOWARDS A PURE THEORY OF THREAT SYSTEMS

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The world of the economist is organized fundamentally by exchange. This relationship, by which each of two parties gives up something to the other and receives something in return, is indeed a powerful social organizer. It is capable of organizing the division of labor and the allocation of resources, and it guides specialized production, as every elementary student of economics knows. In its general form it starts off as a conditional promise: "You do something nice to me and I will do something nice to you." If the other party to whom the communication is addressed accepts the invitation, the promises are fulfilled and the exchange is consummated. In this consummation we have a positive-sum game, provided that the exchange has been a free one in which both parties benefit, at least in their own opinion at the time. There may be later regrets which could turn the operation as it actually takes place into a negative-sum game, but the economist has generally assumed that these are small, and that such disappointments as may occur in exchange constitute a learning process by which everybody eventually learns to make wise choices among the opportunities which are open to him in the market. Exchange, furthermore, is the key relationship in what we might call unconscious or ecological economic development. It encourages the division of labor and this in turn encourages the increase in "skill and dexterity," and the growth, ultimately, of a specialized class of "philosophers," as Adam Smith called them, who practice research and development—all of which leads to increase in the "productive power of labor."

Exchange, however, is not the only system by means of which social organization is built up. Economists have always recognized that exchange can develop a process of social organization and growth only if certain prerequisites are fulfilled in the way of institutions of property, law and order, and so on. The economist, however, has tended to identify these other organizers as essentially static or given in nature and providing merely the preconditions or the framework within which exchange does the real work. Milton Friedman and the "Chicago School" (if there is one) represent the extreme of this point of view. This, however, would seem to be an unrealistic appraisal of the way in which the dynamics of most societies operate. Exchange is by no means the only human relationship which is capable of producing differentiation, division of labor, role structure, communication patterns, and all the other marks of organizational development. I distinguish at least two other types of human relationship which have the property of organizing social systems. The first of these is the threat, which is the main subject of this paper; the second may be called the integrative relationship, which involves a "meeting of minds" (that is, a convergence in the images and the utility functions of the parties towards each other). There is, of course, a negative of this relationship: the disintegrative relationship, in which the images move further apart. The integrative relationship is itself a complex of many different types of relationship, such as, for example, the teaching-learning relationship between the teacher and the student, the persuasive relationship between an orator and his audience or between an advertiser and his addressees, and a great variety of relationships, such as respect, affection, love, and so on, which lead towards similarity in value or utility functions over the domain of various states of the world in the minds of the parties concerned. Economists pay very little attention to these integrative systems, and indeed have generally tended to assume, what in the dynamic sense is sheer nonsense, that utility functions are given —whereas, as Veblen pointed out, they are the product of human interaction. I would add that they are especially the product of the integrative relationships in society.

It must be emphasized that threat systems, exchange systems, and integrative systems are practically never found in a pure form. All actual social systems are likely to contain all three elements. Even under slavery, for instance, which has a high proportion of threat, there are elements of exchange and even of integration. At the other extreme, in the family, the monastery, or the utopian community, where the integrative system is dominant, there are also elements of exchange and threat. Even in the most loving family or community there is a point below which the terms of trade of an individual member cannot deteriorate without causing serious trouble, and the threat of expulsion always remains an ultimate sanction. Just as economics, however, has prospered by abstracting from the complexity of the social system as a whole a single relationship and element, that of exchange, and has built an elaborate theoretical and empirical structure on this foundation, which can then throw light on the more complex processes of the real world, so the phenomena of threat systems on the one side or integrative systems on the other can be abstracted out of the social complex and developed in a pure form. It is tempting, indeed, to try to assign the threat systems to the political scientist and integrative systems to the sociologist, as we assign exchange to the economist, though this division is too neat to correspond to the untidy facts of academic specialization.

Let us then look at the threat as an abstract human relationship, as an economist might look at exchange, and consider how this might be used as an organizer of society. Like exchange, threat in its simplest form is a relationship between two parties. Exchange, however, originates in a conditional promise to do something good if something good is done in return, whereas a threat originates in a promise to do something bad. The threat relationship begins when the threatener says to the threatened, "You do something nice to me or I will do something nasty to you." The exchange-like form of the threat can be seen in the threat of the holdup man, "Your money or your life," which looks like "Give me your money and I will give you your life." There is a real difference, however, between the commodity which is offered in exchange and the discommodity which is offered in the threat. "I will give you your life" means "I will not take away your life." In order for the threat to be perceived as a threat by the threatened, the threatener must be able to create a perception of credibility; that is, of both capability and will of carrying out the threat if the threatened does not do what the threatener wants him to do. We may note that we have exactly the same problem of credibility in exchange, but it is less prominent because the information is usually more obvious. We visualize the exchanger as handing out the object which he offers to exchange. In financial exchanges, however, when what is offered is a promise to pay in the future, the problem of credibility becomes very real and takes much the same form that it does in threat systems.

The subsequent course of the system depends very much on the nature of the response to the initial threat. Four responses may be distinguished which may be labeled submission, defiance, counterthreat, and the integrative response.

Submission is a not infrequent, though seldom popular, response to threat. When the holdup man threatens us we give him our wallet. When the parent says, "Don't steal the cookies or you'll be spanked," the child refrains from stealing the cookies. When the master says to the slave, "Work for me or I will kill you," the slave frequently obeys. When the state says, "Become a soldier or I will put you in prison," the young man allows himself to be conscripted. The threat-submission system is likely to be a conflict system; that is, it is likely to move the parties to a state in which the threatener is better off and the threatened is worse off than in the initial condition. The welfare situations can be illustrated in a field such as Figure 1, in which we plot A's welfare horizontally and B's welfare vertically. Suppose P_1 represents the welfare of both parties before the threat is made. The very act of making the threat is likely to change the positions of the parties in this field. They may, for instance, move to P_2, where both parties are worse off; the threat creates a state of anxiety in the minds of both parties. On the other hand it is possible that A, the threatener, may enjoy making threats, in which case the actual making of the threat might move the parties to P_2'. When submission takes place, we may move to a position like P_3, where A is better off and B is worse off. This is a typical conflict move. On the other hand, it is not impossible that B's submission to the threat may make him better off as well as A, though this is unlikely; that is, submission might move us from P_2 to a position such as P_3'. This is the situation where A is threatening to make B do something for B's own good which B would not be motivated to do in the absence of threat. The moral justification for threat frequently revolves around this hypothesis. We threaten to spank the

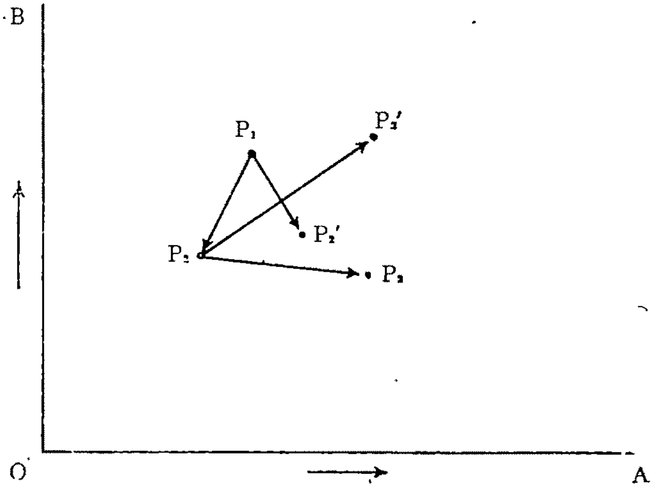

FIGURE 1

child or to fail the student or to hang the murderer strictly for his own good, even if a little skepticism as to these protestations may not be out of order.

The threat-submission relationship has been a powerful social organizer. It is, indeed, one of the major organizers of classical civilization insofar as this rests on slavery. It is also the foundation of a good deal of obedience to the law and of the authority of the state. As Baumol has pointed out in his *Welfare Economics and the Theory of the State,* we may quite rationally vote to threaten ourselves where there is something, like paying taxes, which everybody wants to do if everybody does it, but which nobody wants to do if other people do not. The possibility of nonconflictual threat-submission relationships, therefore, is not to be taken lightly. A key concept here is that of the legitimacy of threat. We submit to the traffic cop and the stoplight because we all recognize their legitimacy. The concept of legitimacy is perhaps more in the domain of integrative systems than of threat systems, but this only illustrates how intertwined these two systems may be.

Threats can be perceived as legitimate only if they are also perceived as appropriate; the threatened punishment must fit the deterred crime. Even if what we are trying to accomplish is not perceived as preventing a crime by the threatened party, that is, even if the threat is not perceived as legitimate, its credibility still depends on its appropriateness. To say to a child, "If you steal the cookies I will kill you," is only credible, and only deters, if not taken literally. This is why "massive retaliation" is impotent against "salami-slicing," to slip once more into the repulsive jargon of strategic science.

The second possible response to threat is defiance. This is somewhat analogous to the nonconsummation of exchange because of a refusal to trade. A says to B, "You give me *X* and I will give you *Y*"; B simply says, "No, it's not worth it to me," and the situation returns to the *status quo ante*. In threat systems the situation is more complicated because defiance puts a burden of response on the threatener and hence is in some sense a challenge to him. The threatener then has to decide whether or not to carry out the threat. If he does carry out the threat, this is likely to have a cost to him as well as to the threatened. We have here a very clear negative-sum game. If he does not carry out the threat his credibility is impaired; there is still a cost to him. Defiance therefore always imposes a cost on the threatener which is not found in the corresponding situation in exchange. The probability of this should presumably be taken into account by a rational threatener in deciding whether or not to make the initial threat.

The third alternative is the counterthreat; that is, "If you do something nasty to me, I will do something nasty to you." This is deterrence. This also imposes upon the original threatener a choice of whether or not to carry out the threat. If he is in fact deterred, his credibility is thereby apt to be weakened the next time he makes a threat; that is, there is a decline, as it were, in the capital value of his threat potentiality. If he carries out his original threat, there is likely to be a mutual exchange of "bads," or carried-out threats, which is very clearly a negative-sum game. This is perhaps the greatest difference between threat systems and exchange systems, for when exchange is consummated it is almost always positive-sum, whereas when threats are consummated they are usually negative-sum. This is why exchange systems have a much higher horizon of development than threat systems. This explains, I think, why in the long pull the free labor market, for instance, has always been able to outdo slavery, and why all classical civilizations have ultimately perished whereas the world of trade has grown slowly but persistently from its very origins.

Perhaps the most important single feature of systems of deterrence is their long-run instability. The whole history of threat systems is summed up succinctly in two verses from St. Luke (11:21-22): "When a strong man armed keepeth his palace, his goods are in peace; but when a stronger than he shall come upon him and overcome him, he taketh from him all his armor wherein he trusted and divideth his spoils." It is indeed a source of the basic long-run instability of all threat systems that the unilateral threat system, or the threat-submission system, which may be fairly successful for a time, almost inevitably degenerates into the bilateral threat system, or deterrence. Deterrence is successful as long as it deters, but deterrence itself seems to be unstable. The reason for this, I suspect, is that the credibility of threats depreciates with time if threats are not carried out. Hence threats occasionally need to be carried out in order to re-establish their credibility. Another reason is that threat capability declines if threats are not occasionally carried out, particularly where this capability is enshrined in complex social organizations and in apparatus such as armed forces.

For both these reasons, a state of stable deterrence is rarely stable for long. Eventually either the capability or the credibility, or both, of one of the parties will depreciate to the point where the other party will make new demands. At this point deterrence breaks down and the system slips either into submission or into defiance. Submission is particularly hard if the parties have lived under stable deterrence for a while, so the outcome of a new demand is likely to be defiance. This, then, forces on the imposer of the new demand the choice between carrying out the threat or acceptance of the defiance, which would further destroy his credibility. He is likely, therefore, to carry out the threat, which will lead to the carrying out of the counterthreat, which will result in a negative-sum game known as war. In this, it is true, one party may emerge relatively better off than the other, and indeed if the loser is very much worse off the victor may even be absolutely better off. In this case we may pass again to a unilateral threat system. This is the "successful" period of the new civilization, and this again leads to counterthreats and ultimate downfall. Thus the rise and fall of civilizations seems to be implicit in the very nature of the threat system itself—as, indeed, a pure exchange system is also likely to experience ultimate cycles of boom and collapse. It may be that, ultimately, only integrative systems can provide stability, and I am not sure even of that.

This brings us to the fourth possible response to threat, which is the integrative response. This is harder to analyze and to describe. It may take a great many different forms because of the very richness of the integrative system itself. The integrative response is that which establishes community between the threatener and the threatened and produces common values and a common interest. The integrative response may be mixed with any one of the first three responses. Submission, for instance, may be made in such a way that the threatener is eventually absorbed into a larger culture and the threat system disappears. The experience of the Negro in the United States may be such a case. By accepting and illuminating the ostensible value system of his conquerors, the Negro both made and produced an integrative response which eventually made slavery impossible. In the gandhian experience in India we have an example of defiance mixed with an integrative response; this is the essence of nonviolent resistance. The threatened party, by defying the threatener and by accepting the consequences without bitterness or complaint, eventually undermines the morale of the threatener and the threat system disintegrates. Examples of counterthreat used as an integrative response seem to be rarer, for whereas both submission and defiance, in a sense, unite the threatener and the threatened in some sort of a social system in which integrative factors may operate, the counterthreat divides the threatener from the threatened, breaks any bond which might develop between them, and hence is apt to be disintegrative rather than integrative. This perhaps is another reason for the fundamental instability of deterrence, and its ultimate certainty of failure. It is still not inconceivable, however, that a carefully controlled counterthreat might be combined with an integrative response. We may have seen something like this happen in the industrial relationship, where the threat to fire on the part of the employer may produce either the counterthreat to quit or the more organized counterthreat of the strike. In this case, however, the very nature of the relationship and the fact that if there is to be an industrial organization at all the parties have to live together, opens up the possibility of integrative solutions to counterthreat systems. Where, for instance, the threat or even the actuality of the strike is used to obtain union recognition and a contract, we see counterthreat being used as a device to establish an essentially integrative system of industrial jurisprudence. Parallel to this in international systems might be a counterthreat system used to achieve a disarmament organization or a world government.

Like exchange systems, threat systems have a geographical structure which is imposed on them by the fact that threats, like commodities, have a cost of transport. Because of this there are many striking parallels, as I have pointed out in *Conflict and Defense* (Harper and Row, New York, 1962), between the competition of states by means of threats and the competition of firms by means of exchange. Thus the theory of duopoly is very much like that of a bipolar system in international relations. Each of the competing organizations has a certain "home strength" as represented by the threat capacity in the case of the nation or the cost of production at the mill in the case of the firm. As it goes away from home it has to incur a cost of transport, so that the further from home the organization operates the smaller the threat that it can bring to bear or the higher the price it must charge. In each case this represents an application of the general principle of "the further the weaker." Between two states as between two firms there is likely to be a boundary of equal strength, with each organization dominating the area between itself and the boundary. Such a situation can easily lead to an arms race or a price war, as each organization tries to push the boundary of equal strength further from its home base, either by increasing its armaments or by cutting its mill price. This is pretty clearly a negative-sum game, from which the parties can usually only be rescued by an integrative system of some kind, for instance a merger in the case of the firms, or in less extreme forms a gentleman's agreement, and in the case of states a federation or a "security community."

These considerations lead to an interesting theory of viability which I have also expounded in some detail in *Conflict and Defense.* An organization may be said to have unconditional viability if no other organization has the capability of destroying it; that is, an organization is unconditionally viable if it is dominant over all other organizations at its home base. A system of unconditional viability is only possible if each organization is stronger than any other or any reasonable combination of others. This paradoxical result is attainable if the organizations are far enough apart and if their strength, that is, their threat capability, diminishes rapidly enough for each one as he goes away from home, that is, if there is what I call a high loss-of-strength gradient. A system of stable unconditional viability is threatened either by an increase in the number of organizations in a given field or by diminution of the loss-of-strength gradient. The less the loss-of-strength gradient, the fewer organizations can coexist in unconditional viability.

I have argued that the peculiar crisis of national defense today is a result of the loss of unconditional viability even by the largest nations in the light of the nuclear threat. If we are to exist at all, it must be on conditions of conditional viability, in which each organization can destroy the other but refrains from doing so. This is not an altogether unfamiliar system—indeed, in interpersonal relations we have had to live with it. In the age of the sword, unconditional viability for the individual was at least not inconceivable, even though one had to be a pretty good swordsman to achieve it. The crossbow and, following that, firearms ruined this system and in our personal relations we now live in a world in which we are literally all at each other's mercy. One would think that, in these circumstances, mercy would be taken seriously and studied, but this is the last thing that anyone seems to want to do. In international relations, especially, we are still living under the illusion of unconditional viability, and we may have a very rude awakening.

Concepts of liquidity and inflation which have been developed in economics have also some applicability to threat systems. The concept of generalized military capability is closely related to the economic concept of liquidity. A liquid asset is something with which anything can be bought; a military capability is something with which anything can be destroyed. It is, as it were, money for the doing of harm. Another reason for the instability of threat systems, incidentally, is that harm can be done much faster than good (I am indebted to T. C. Schelling for this observation). One might add, however, as a corollary that the ultimate victory of exchange and integrative systems over threat systems is assured by the fact that more good can be done than harm. The doing of harm has a limit of total destruction; that is, of zero good. The doing of good has no definite upper limit. The liquidity or the lack of specificity of a military capability is another factor which is likely to lead to the instability of the threat system. The military capability of A is a generalized threat directed, say, at B, C, D, and E. Each of these parties, however, perceives the threat as if it were directed wholly at him. The perception of the threat, therefore, on the part of the threatened is four times the amount of the threat on the part of the threatener at a maximum. It is not surprising that under these systems threat systems produce armament races which almost inevitably lead to the carrying out of the threat.

When threat systems become embodied in military organizations, we have problems arising which are strikingly parallel to those which develop for economics when production and exchange get to be concentrated in firms. The military organization, which I have elsewhere called the "milorg," is the equivalent in the threat system of the firm in the exchange system. It possesses many of the qualities and characteristics of the firm. It has a hierarchical organization, it is organized by a budget, it has flows of expenditures most of which are laid out in the purchase of inputs, and it has a flow of receipts. The major difference between the milorg and the firm arises from the source of the receipts: in the case of the firm, receipts are derived from the sale of a product on the market; in the case of the milorg, the receipts are derived from a government budget and ultimately from taxation or from the creation of money. The milorg, that is to say, does not produce a clearly defined product in the way that a firm does; its receipts have more the character of transfer payments than of returns from sales;

that is, exchange. In this respect it is more like a philanthropy than it is like a firm. If we ask what it produces, we would have to answer in terms of a generalized threat; that is, a capability of doing harm to unspecified persons or things.

We may go further, indeed, and distinguish a number of different "commodities" which the milorg produces. One in the currently fashionable terminology is the "counterforce." This is the diminution of a potential enemy's capacity to harm the home organization. This might take the form of a defensive work such as a wall, a buffer state, a Maginot Line, or anything which would sharply increase the potential opponent's loss-of-strength gradient as he moved towards the defended center. It might also take the form of an ability to destroy the opponent's milorg or those aspects of it which have capability of destroying the home organization. Another product of the milorg might be, again in the fashionable terminology, "countervalue." This is the ability to threaten, not the threat-makers of the opponent, but the things which he values—his cities, people, social organizations, and so on. Countervalue weapons and organizations are generally associated with deterrents, that is, with counterthreats, but they may also be used unilaterally to threaten the opponent into doing something which the threatener wants. The great difficulty of operating a threat system is that all these different threat-commodities are very hard to distinguish in practice, and hence an action which is intended to be one kind of threat may be interpreted at the other end as a totally different kind. Insofar as threats are used as instruments of persuasion to change people's opinions or behavior, they tend to suffer from a defect of all persuasion systems: that what is persuasive to the persuader is not always persuasive to the persuadee. The threatener or the persuader has an image of the other which is derived largely from his own experience, and hence is likely to be false in many important regards. Our threats, like our persuasions, tend to be directed at some imaginary person whom we have made in our own image, not at the real person at whom the activity is directed.

The analogue of inflation in threat systems is found in the arms race, in which both parties continually increase the amount of economic resources which they are putting into organized threats (milorgs) without changing their relative power position. Today, indeed, we may be approaching a hyperinflation of the threat system under the impact of military research and development, which effectively destroys any validity which the system once may have had. We can then look at arms control as an attempt at stabilizing the "price level" after a hyperinflation.

The subject could be pursued much further, and there seems to me to be no reason why a science of threat systems should not be developed at least as elaborately as economics builds from exchange. This science, perhaps, still has to find its Adam Smith, but one feels that so much development has gone on in it in recent years that its "Wealth of Nations" must be just around the corner. The need for this science, furthermore, is very urgent. Because of the development of the nuclear weapon and the consequent disappearance of unconditional viability, the whole threat system, that is, the system of national defense, is suffering a grave crisis—indeed, I would argue, a breakdown. The control of the threat system, therefore, is a matter of the topmost priority for the human race. Unless we do this we may not have a chance to develop any other systems. In order to control a threat system, however, we must understand it. It is one of the most astonishing features of human society that governments are willing to invest so much money in threat systems with only crude folk knowledge as to how these systems actually work, and without being at all interested, apparently, in finding out more about them. This is perhaps because the threat system is an important element in the integrative system of most societies. Nations are built on the solid foundations of violence and cruelty, threats and counterthreats. Our national heroes are soldiers and our national mythology is a mythology of successful threat. A threat to the threat system, therefore, is seen as a threat to the integrity of the society itself. We love making threats and it satisfies our masculine demand for "strength" to make them, and hence a threat to the threat system is seen as a threat to that which we love. To make a science of threats, however, is to threaten the threat system itself, for a system so inefficient in producing welfare as the threat system is can only survive as long as it is supported on folk ignorance. It is little wonder, therefore, that the science of threat systems has been so slow to develop.

My thesis may perhaps be summarized in the following lines of verse:

Four things that give mankind a shove
Are threats, exchange, persuasion, love;
But taken in the wrong proportions
These give us cultural abortions.
For threats bring manifold abuses
In games where everybody loses;
Exchange enriches every nation
But leads to dangerous alienation;
Persuaders organize their brothers
But fool themselves as well as others;
And love, with longer pull than hate,
Is slow indeed to propagate.

THE IMPACT OF ARMS REDUCTION ON RESEARCH AND DEVELOPMENT*

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Introduction

Why Are We Concerned? It is not just a coincidence that the fifties, which were marked by the largest peacetime military budgets in American history, also were marked by a striking rise in the percentage of our economic resources allocated to scientific research and development. Indeed there are tight links between these two phenomena running both ways.

Somewhere in the neighborhood of 15 per cent of our defense outlays are for research and development (henceforth denoted R and D), and somewhere between 50 per cent and 60 per cent of our total R and D expenditures is financed by defense agencies.[1] Although these measures are crude, it is clear that our defense expenditure has had a very major impact on the magnitude and allocation of our R and D effort.

Looking at the matter the other way around, our post-World War II history of no major central war between the two big power blocs and at the same time extremely high levels of peacetime military expenditure must be attributed, in large part, to R and D. Modern science has created a world in which both the potential devastation to an aggressor against a well-prepared adversary, and the costs required to be well prepared, are unprecedented. It is modern science which lies behind both the balance and the delicateness of the delicate balance of terror.

It is clear that significant arms reduction would have a major impact on the size and allocation of our R and D effort. Why should this be of concern? There are two conceptually distinct reasons. One is the familiar problem of frictional unemployment. The second is rather special and is related to the nature and role of R and D in a basically free enterprise economy. Research and development is an activity yielding a large crop of external economies. A significant cutback in R and D could retard the rate of technological progress in our econ-

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[1] In this paper, defense R and D is defined to include the programs of the Department of Defense and the military programs of the Atomic Energy Commission. NASA work is not included.

omy and might present a socially undesirable reallocation of resources.[2] This point will be elaborated—and qualified significantly—later.

Measuring the R and D Impact of Defense. Anyone who thinks it an easy matter to separate military from nonmilitary R and D should try it. Civilian uses of electronic and other equipment developed for military purposes are numerous and important, and, on the other hand, much of our military equipment incorporates hosts of devices, materials, processes, designs, and concepts not originally conceived or developed for military purposes. In research, it is difficult to think of many areas of physics or mathematics where the results of really significant break-throughs would not affect both civilian and military technology; and nowadays many areas of genetics, medicine, and even geology may have important military applications.[2]

The fact that many research areas and many development projects may have both military and civilian applications leads to several difficult problems in measuring the impact of our defense effort on R and D. One problem is that defense support of research often is a substitute for, not an addition to, nondefense support. For example, a good deal of the work in space technology financed by the Department of Defense (henceforth denoted DOD) would probably be picked up by NASA if the DOD stopped its support, and similarly if NASA closed down, a good part of its work would almost certainly be financed by the DOD. Much of the work undertaken in private industrial labs on semiconductor devices would be financed by the companies themselves if DOD support was withdrawn, and the DOD might well kick more into the kitty if the companies cut back on their own financing.[3]

The "substitution" problem tends to make R and D spending by the DOD overstate the real impact of defense on R and D. The "accounting" problem tends the opposite way. To understand the accounting problem, assume that a $4 billion defense contract has been let with a major aircraft company for the design, development, and procurement of a new ballistic missile. Assume that $1 billion of the contract are especially labeled R and D, and thus appear on the NSF accounts as "defense" R and D.[4] But much of the work on the contract will be subcontracted out, and though much of this subcontracting work may involve considerable research and development on components, R and D work done by subcontractors may or may not be accounted under the $1 billion figure. It is if the prime contractor allots part of the R and D contract to subcontractors. But when the prime contractor has an option to shop around to find the best components and possible subcontractors compete with each other to provide the best component design or prototype, the R and D work is done on the potential subcontractor's own financing. When a firm gains the subcontract, the terms of the formal contract are for delivery of hardware. The R and D expense is covered in the contract sum, but in the DOD's books this is hardware outlay, and in the NSF's books this is privately-financed R and D. With the materials, sub-subcomponents and other inputs to the weapon system well down at the base of the assembly pyramid the story is similar. And to get all of the R and D associated with the $4 billion weapon system contract, we must trace our way back through the entire input-output matrix, attempting to estimate the induced R and D outlays of the firms supplying the subcontractors, the firms supplying these firms, and so on.

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[2] See, for example, Striner, Sherman and Karadbil, *Defense Spending and the U. S. Economy* (ORO, Johns Hopkins, 1959).

[3] The history of the synthetic rubber industry testifies to the importance of this substitution effect. See Robert Solo, "Research and Development in the Synthetic Rubber Industry," *Q.J.E.*, 1954.

[4] The NSF calculates that the total government contribution was about $9 billion. It is estimated that between 75 and 80 per cent of this is military.

A third problem of measuring the impact relates to the effect of rising demand upon supply. To the extent that net defense demand for R and D has been positive, there probably has been some induced increase both in the number of students who take training in the sciences and engineering and in the per cent of these people who have such training who work in R and D. In addition, the Defense R and D programs have had a quite noticeable effect on the type of science and engineering training graduate students and undergraduates take.

Despite these difficulties, in this paper the spending of the DOD and the AEC in defense programs will be used as the measure of defense-related R and D.

A Simple Linear Model

The Naïve Model. For present purposes we are interested in the impact of certain specified reductions in defense spending upon R and D. For convenience, we shall take as reference a 50 per cent cutback in defense spending, but all results will be in terms of any cutback in defense spending. In order to set an upper bound on the impact, in this section it will be assumed that the cutback is all at once; in a later section this assumption will be relaxed.

The model rests on the assumption that the ratio of defense R and D to defense final demand can be treated as a constant, as can the ratio of nondefense R and D to nondefense final demand. These constants can be used to estimate what might happen to R and D expenditures when demand shifts from defense to nondefense. We shall assume that appropriate fiscal and monetary policy assures that all declines in defense final demand are compensated for by equal dollar increases in civilian final demand.

In 1960, about $14 billion of our economy's resources were spent on R and D. The Department of Defense and the military part of the Atomic Energy Commission supported somewhere around $7 billion of this work. Total 1960 defense expenditure was about $45 billion, including some of the AEC's work, and thus defense R and D was between 15 and 16 per cent of defense final demand. Since gross national product for 1960 was about $500 billion, nondefense R and D represented about 1½ per cent of nondefense final demand. If these ratios stay constant, a dollar reduction in military final demand compensated by a dollar increase in civilian demand would result in a 14 cent reduction in total R and D. Cutting the military budget in half and increasing civilian demand to compensate would result in a reduction in total R and D outlay of about $3.2 billion, or about 23 per cent. It appears, then, that on these assumptions the proportional drop in total R and D expenditure in the economy would be about half as great as the proportional cutback in military expenditure. Or, to put it another way, the elasticity is approximately .46.

Composition of Demand. The preceding calculations did not take into account the significant differences in R and D for different industries of the civilian economy. Clearly, if civilian demand expands in such a way as to call forth a high level of output in the electrical equipment industry, for example, this would lead to a larger total national R and D effort than if civilian demand increased principally for, say, agricultural products. It therefore is useful to explore the effects of different compositions of increases in nonmilitary demand.

Leontief and Hoffenberg have estimated,[5] by input-output techniques, the increased sales levels of the various industries which would be induced by a $1 million increase in investment, consumption, military spending, and other final demand categories. Although the Leontief-Hoffenberg industry break does not exactly match the industry break used by the NSF, I have tried to link the two. My rough calculations indicate that one dollar of consumption final demand generates 1.2 cents of R and D expenditure, a dollar of investment final demand about 4.5 cents of R and D spending, with the other Leontief-Hoffenberg nonmilitary final demand categories falling between these two categories in their R and D impact.

If we calculate the elasticity of R and D expenditures with respect to defense expenditure, using the above ratios and assuming that defense R and D is cut back proportionately with defense expenditure, we find that if investment final demand fills the gap, the elasticity is about .4 and if consumption final demand fills the gap, the elasticity is nearly .5.

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[5] W. Leontief and M. Hoffenberg, "The Economics of Disarmament," *Scientific American*, Apr., 1961.

The reason for the rather surprising lack of sensitivity is obvious. The amount of R and D generated per dollar of defense final demand is so large relative to the amount generated by any other category of final demand that differences in the other categories have but a small effect on the computed elasticity.

Employment Versus Spending. This estimate of the expenditure impact significantly overstates the impact of disarmament on employment of scientists and engineers. There are two reasons for this. First, expenditure on R and D covers a good deal more than the salaries of scientists and engineers. A good percentage (more than half) of the expense is for materials and equipment and the salaries of technical assistants and administrative people. In military R and D, because of the emphasis on hardware development which involves heavy materials outlay, the percentage of total R and D outlay attributable to salaries of scientists and engineers is significantly smaller than in nonmilitary R and D. It is estimated that the defense R and D dollar hires only three-quarters the number of R and D scientists and engineers as the nondefense R and D dollar.

Second, the preceding calculations have treated only R and D scientists and engineers, and only about one-third of our scientists and engineers are engaged in R and D (on a full-time equivalent basis). About two-thirds of their time is spent on problems of production control and planning, administration, sales, and teaching. In the defense industries, R and D scientists and engineers comprise a much larger share of the total number than in nondefense industries. It is estimated that approximately 50 per cent of the engineers and scientists in defense work are in R and D, while in nondefense work the figure is only 25 per cent.

Taking into account both the relatively high R and D outlay per scientist and engineer and the relatively high ratio of R and D scientists and engineers to total scientists and engineers in military work, as contrasted to civilian work, it can be calculated that the preceding calculations overstate, by approximately a factor of two, the impact of a compensated decline in defense spending on the employment of scientists and engineers. While a 50 per cent decline in defense spending would lead to a 23 per cent cutback in R and D spending, it would lead to only a 12 per cent cutback in employment of scientists and engineers.

Sectoral Impact. The figures ground out by these rough calculations suggest, first, that if R and D sales ratios do not change very much, a cutback in defense expenditure compensated by an increase in nondefense final demand will lead to a proportional reduction in R and D outlay somewhat less than half as great. Second, if the ratio of scientists and engineers to total sales does not change in the different industries, a given proportional cutback in defense spending will result in a proportional cutback in employment of scientists and engineers about one-quarter as great. It is worth while to look beneath the aggregative figures to try to get some feel for the organizations and industries which would be most affected.

In 1960, the Department of Defense financed about 75 per cent of the work done in government facilities, slightly more than 50 per cent of the work performed by industry, and about 25 per cent of the work performed by colleges and universities and other nonprofit institutions. If one-half of the expenditure of the AEC is included, the figure for the universities and other nonprofits rises to about 35 per cent. Thus defense cutbacks would seem likely to fall with greatest proportional impact on government-owned facilities, with next greatest on industry, and least upon universities and other nonprofit centers.

Consideration of the type of work done for the DOD by these different sectors strengthens the conclusion that universities would not be hard hit. It is likely that basic research projects financed by the DOD would not be cut back as much as R and D tied to weapons. And where defense spending for basic research is cut back, these projects would become prime candidates for nondefense support. While only about 3 per cent of DOD-supported work in industry is basic research, about 40 per cent of the work in colleges and universities financed by the DOD is classified as basic research.[6] Thus, both because the DOD finances a relatively small share of R and D at the universities and because much of that work is basic research which is unlikely to be cut back drastically, the impact of disarmament on R and D at the universities should not be particularly great. It also should be noted that about one-third of defense-related work conducted by the universities is done in special centers set up for that purpose.

A large proportion of DOD-financed work carried on by industry is closely tied to specific weapon systems, and thus is apt to be very sensitive to arms reduction. Somewhere around 85 per cent of it is development work. Moreover, the impact of disarmament on industry R and D is likely to be concentrated in a few industries. Of the DOD's industrial R and D spending, more than 50 per cent goes to the air-

[6] It should be noted that the DOD supports a much smaller fraction of basic research than total R and D. Indeed, the DOD supports only about 15 per cent of the total basic research done in the United States. If AEC support is added, the figure comes to about 25 per cent. For this reason, as well as those stated above, the impact of disarmament is likely to be much less on basic research than on other kinds of R and D.

craft and parts industry and about 20 per cent to the electronics industry. Looked at in another way, the Department of Defense supports better than half of the R and D done in three industries—aircraft 85 per cent, electrical equipment about 60 per cent, and communications about 50 per cent—while in such industries as food, chemicals, petroleum, and primary metals, it provides less than 10 per cent of the funds.

The fact that military R and D has a different "mix" than nonmilitary R and D suggests that disarmament would affect the various scientific and technical professions very differently. Unfortunately, the scientific and technical manpower data are much to aggregate to provide support for this conjecture.

The Workings of the Market

The General Framework. The previous analysis was based on given and assumed constant R and D to sales ratios. But before it is assumed that the figures calculated by the model provide a rough measure of the job for public policy instruments, we should examine how the market mechanism is likely to contribute to the solution to the problem. In particular, how might R and D to sales ratios change in response to an excess supply of scientists and engineers?

The determinants of R and D spending have been under increasing study by economists. There is mounting evidence that, over the short run, the level of R and D spending can be reasonably well explained by a model which assumes that firm managers act as if they have a target R and D to sales ratio, and that they try to reduce a discrepancy between this target ratio (or rule of thumb) and the actual R and D to sales ratio. The widespread (but far from universal) existence of a "target" R and D to sales ratio has been uncovered by NSF interviews. Norton Seeber has collected considerable evidence on this point and Edwin Mansfield has tested such a model statistically and found it quite reliable.[7]

But over relatively long periods of time, particularly when prices and technologies are changing, it seems likely that short-run rules of thumb also change. On the average the rules of thumb (the R and D to sales ratio, the price markup, the inventory sales ratio) should change so as to move closer toward that which is optimal. For present purposes, optimal and expected profit maximizing will be considered as synonomous.

The optimal expenditure on R and D obviously requires that the discounted future returns from increasing the quantity of resources applied to R and D equals the cost of these additional resources. The effect of decreases in R and D costs upon the optimal quantity of R and D resources depends on the shape of the marginal efficiency curve. If, for example, given the level of sales, this curve is of unitary elasticity (is a rectangular hyperbole), then a 1 per cent decline in R and D unit costs will lead to a 1 per cent increase in the optimal quantity of resources applied to R and D.

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[7] Seeber's work is as yet unpublished. Mansfield's results are published in *The Rate and Direction of Inventive Activity* (NBER, 1962).

It can be shown that the unitary elasticity assumption is consistent with the observation that, for firms that spend anything at all on R and D, the R and D to sales ratio is not systematically related to firm size.[8] Perhaps, therefore, the unitary elasticity assumption suggests the orders of magnitude involved. Earlier in this paper it was suggested that a 50 per cent cutback in military expenditures would lead to approximately a 23 per cent cutback in R and D spending, holding R and D unit costs constant. The preceding analysis suggests that a 23 per cent reduction in R and D unit costs would enable the market itself to take up the slack. To restore R and D employment would require a cost reduction of less than 20 per cent.[9]

Factor Substitution. The costs of R and D, however, include much more than the salaries of scientists and engineers. In fact, the salaries of scientists and engineers comprise only a little more than a quarter of total R and D costs in American industry. This makes it difficult to estimate the elasticity of R and D employment of scientists and engineers with respect to a cut in wage and salary levels. The crucial factor is the elasticity of substitution between scientists and engineers and other R and D factors of production.

At one extreme, if R and D factor proportions are relatively fixed, in order to achieve an x per cent increase in employment of scientists and engineers the market will require a fall in the salaries of significantly more than x per cent, unless the costs of other R and D inputs fall also. However, if R and D factor proportions are relatively flexible, then the stickiness of other factor prices may actually help the employment situation for scientists and engineers. To gain an x per cent increase in employment will require a less than x per cent decline in salaries. Unfortunately, there exist almost no data from which either the elasticity of substitution or the likely market response of the cost of the other factors can be estimated.

The Dynamics of Adjustment. The dynamics of the adjustment mechanism, however, are likely to be relatively favorable. The target R and D to sales ratios are likely to be adjusted upwards in response to a slack market for scientists and engineers. The recent work of Cyert and March[10] suggests that the decision rules established by a firm are likely to be relatively stable if they generate results which are satisfactory. However, if the results are less than satisfactory, the organization or individual tends to re-examine the decision rules and to search for improvement. For the purpose of this analysis, satisfactory profits will be defined as the rate of return the capital and management could be earning elsewhere, over the long run.

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[8] See J. Schmookler, "Bigness, Fewness, and Research," *J.P.E.*, Dec., 1959.

[9] The reduction in R and D employment calculated by the model falls between the reduction in R and D spending and the reduction in total employment of scientists and engineers.

Let us assume that as other firms increase their R and D spending a firm which does not increase its own spending will experience a decline in market share. And the fall in market share is likely to, although it need not, be reflected in smaller profits. Then, if disarmament leads to an increase in the optimal R and D sales ratio, as soon as a few firms act to increase their spending, this will put pressure on other firms to re-evaluate their own R and D decisions.[11]

Firms under profit pressure may well be able to determine their appropriate R and D response by imitating the more successful firms; the firms which increased their R and D spending earlier. As Sidney Winter has pointed out in an unpublished paper, the economic system tends to enforce good decision rules not only by killing off inefficient firms but by providing models for them to imitate to become more efficient.[12] Norton Seeber and the National Science Foundation have found considerable evidence that firms in an industry pay a considerable amount of attention to the R and D policies of the more successful firms.

Problems of Public Policy

Employment. Although scientists and engineers will probably be harder hit by arms reduction than any other occupational group (save military people), it is not likely that unemployment of scientists and engineers will be more than a very short-run problem. R and D is one of the fastest growing activities in our economy, despite some retardation in the last few years, and the employment of scientists and engineers is growing at a faster rate than employment of almost any other occupational group.

In the earlier analysis it was assumed that arms reduction would come suddenly and sharply, and the calculated cutbacks in spending and employment are, therefore, an upper bound. It can be shown that if the ratio of civilian R and D to civilian final demand does not decrease its rate of growth dramatically, then a phased disarmament program might not reduce total R and D spending at all but rather would (temporarily) retard its rate of growth.

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[10] *A Behavioral Theory of the Firm* (to be published).

[11] Notice that the dynamics are nonlinear. Thus when the optimum R and D ratio falls, the first firms who respond reduce the pressure on other firms to respond.

[12] Winter's unpublished paper, "Economic Natural Selection and the Theory of the Firm," points out some important ways in which "imitation" may mislead.

If salaries are at all flexible, if the response of R and D employment to changes in salaries is reasonably great, and if scientists are reasonably mobile, this suggests that the impact of arms reduction on employment of scientists and engineers is likely to be more a temporary retardation of the rate of growth of salaries than significant unemployment.

However, in a situation where demand has shifted greatly, the federal government can play a very helpful role in oiling the adjustment mechanism. Programs might be developed to help scientists and engineers previously engaged in defense work to learn to tailor their training to the problems of civilian industry. Quite possibly something should be done to reduce the cost of moving, since there is likely to be a significant shift in the geographical composition of demand. Making moving expenses fully tax deductible seems a reasonable step. Perhaps the government should go further and develop tax credit schemes to facilitate mobility.

If more action is deemed necessary, then a step-up in the peacetime space program is ideally suited for the job. The industries involved, the locations, the skills are almost a perfect match for those which will be hit hardest by arms reduction. Whether an increase in the space program is justified on the basis of other criteria, is another question.

Welfare. As has been stated earlier, our concern about a cutback in R and D resulting from a reduction in defense spending transcends our concern with maintaining full employment. We tend to think (rightly) of R and D as an activity yielding a bountiful crop of external economies—as an activity to which the market will allocate too small a fraction of our resources in the absence of public action. If we take this point of view, our defense effort is yielding us a bonus by causing a larger R and D effort than we would have in the absence of our defense programs, and one of the real costs of arms reduction might be the shifting of resources away from R and D.

However, much of the discussion of the economic impact of our defense and space R and D efforts has tended to ignore the fact that the overwhelming percentage of defense R and D dollars is spent on work aimed at creating a specific new component or system for the military. Certainly this kind of R and D contributes far less to the advance of civilian technology than R and D concerned more directly with civilian problems.

The civilian economy obviously goes gain some by-product advantage from the R and D financed by defense agencies. But military

R and D increasingly is exploring areas far away from those of clear relevance to the civilian economy. While it is difficult to measure the degree of civilian relevance of defense R and D programs, clearly research on mildew resistant fabrics for military use is much more likely to have immediate civilian application than research on nose cones. A very large percentage of the civilian applications of military R and D listed in the ORO study mentioned earlier is of the "mildew resistant cloth" type, and this type of R and D probably is a much smaller percentage of total military work today them before the missile age.

Indeed, the case can be made that in recent years the growth of military and space R and D has significantly retarded the growth of civilian R and D. Between 1954 and 1961, while the number of total scientists and engineers in R and D in industry grew at approximately a 10 per cent annual rate, the number financed by private industry out of their own funds and, presumably, involved principally on civilian programs grew at only one-third that rate. There is considerable feeling among R and D directors that the growth of defense R and D, by bidding up salaries and by taking the cream of the new science and engineering graduates, has tended to reduce significantly the quantity and quality of R and D undertaken in civilian-created laboratories.

In any case, a significant increase in R and D resources could be used in the civilian sector, with large benefit to society. In many of the civilian industries very little R and D is presently directed toward improving products and processes. While in the aircraft and parts industry and the communications and other electrical equipment industries (the industries accounting for more than 80 per cent of government research funds spent in industry) R and D spending exceeds 10 per cent of sales, for all other industries taken together the figure does not exceed 3 per cent. Increased R and D spending undoubtedly would yield high returns in many of these industries. The civilian economy would benefit especially from increased long-range research and experimentation with advanced technological possibilities of the sort that the research teams presently employed by defense industries have conducted so successfully. The freeing of these highly trained research resources for application to civilian technology would hold great promise for increasing the welfare of the American people.

In addition to the nonmilitary domestic uses for R and D resources that would be freed by disarmament, these resources could be used to complement an expanded foreign aid program. Research and development might be focused on such problems as development of simple teaching machines and related communications equipment, techniques for overcoming aridity, efficient and low-cost transportation systems, cheap and reliable power sources, and other equipment and processes spe-

DISCUSSION

RICHARD U. SHERMAN, JR.: Despite the wide range of subject matter and approach displayed, all three of the papers presented at this session have one thing in common: they indicate that economists have much to contribute to the multifarious and perplexing problems of national security. I shall comment briefly on each of the three papers.

Professor Boulding, in his highly original and provocative paper, makes an urgent plea for the development of a "science of threat systems." He arrives at the conclusion that "the whole threat system, that is, the system of national defense, is suffering a grave crisis—indeed, I would argue, a breakdown"; hence, we must learn to control the threat system (e.g., to stem the arms race through arms control), and to do this we must first understand the threat system and how it works. His paper provides a powerful stimulus and some indicated direction for such study. However, his plea for the development of a science of threat systems is weakened, it seems to me, by his use of extreme statements—assertions which are not supported and which I do not find to be readily acceptable from my own observations. For example: "We love making threats and it satisfies our masculine demand for 'strength' to make them, and hence a threat to the threat system is seen as a threat to that which we love."

Dr. Nelson's detailed analysis of the impact of arms reduction on R and D is both worth while and comforting—worth while because of the dominant position of R and D in the nation's total R and D effort and comforting because of its conclusions that "the impact of disarmament on R and D at the universities should not be particularly great" and "it is not likely that unemployment of scientists and engineers as a result of arms reduction will be more than a very short-run problem." The model developed by Nelson does indeed shed some light on the problem it attacks—by emphasizing its complexity and by calling attention to the large number of factors which require consideration. On the other hand, the validity of the conclusions rests necessarily upon the many assumptions which Nelson has been forced to make and which he openly states with clarity, but which may or may not be realistic. Possibly the model could be programmed for a computer, so that it would be possible to study the effects of varying the assumptions, and thereby obtain a firmer base for determining appropriate public policies. As Dr. Nelson says, given disarmament, there would be a wonderful opportunity to shift R and D resources from the military to the civilian sector. But are we prepared to take advantage of it? Alternative plans to do so should be under development now.

Dr. Enthoven describes his paper as a progress report on some of the work of economists in the Department of Defense, with the focus on the problems of determining requirements for weapon systems and forces; that is, the allocation of the defense budget. Accordingly, I was hoping to hear how the Programming System has actually worked during the year or so since its inception. Instead, however, much of the content of the paper appears to be pretty much a rehash of the rationale for the system contained in speeches by Enthoven and his boss, DOD Comptroller Charles J. Hitch, as much as a year ago. It leaves me still wondering: (1) How well is the system working? (2) What weaknesses have been uncovered? (3) Has the system been accepted—by Congress? By the military? By industry? (4) How good are the estimates of costs and benefits, projected five years ahead? (5) How are estimates of marginal costs and marginal products actually made? (6) Do the military departments receive DOD guidance in time to use for their budget estimates now? (They never used to.) (7) Is any consideration being given to changes in the nine major programs, specifically to a further breakdown in the huge program, General Purpose Forces, which alone amounts to one-third of the entire defense budget?

I was much interested in the approach developed and adopted with respect to estimating conventional force requirements, specifically to the formulation of a range of carefully defined limited war problems, by theater. This sounds like an excellent attack on a very difficult problem. However, it is hard to believe that "the uncertainties appear to be much greater in the case of conventional forces," since, as Enthoven points out, the United States has fought many nonnuclear wars; and not only has the military profession studied the experience of these wars, but research organizations, such as RAND and the Research Analysis Corporation (formerly ORO of Johns Hopkins), have made studies of morale, leadership, etc., and of the interactions mentioned by Enthoven. The discussion of practical ways developed by DOD economists for dealing with uncertainties is illuminating, particularly the emphasis on the use of the simplest, most fundamental economic methods. This raises some question in my mind as to the usefulness of developing an "elaborate" theory of threat systems as proposed by Boulding.

At the close of his paper, Enthoven cites the many interesting and important opportunities open to national-security-minded economists. Where will these trained economists come from? An expanding number of universities are offering courses in national security (as will be evident when the United States Air Force Academy publishes the results of its current National Security Policy Teaching Survey). My own University (Ohio State) has just established "the economics of national security" as an approved field for graduate students seeking an M.A. or Ph.D. degree in economics. In direct support of this field the Economics Department offers a course and a graduate seminar specifically in economics of national security and also gives credit to economics students taking the interdepartmental "National Security Policy Seminar."

Ever since Sputnik I in 1957 there has been an awareness of the pressing need to train more scientists and engineers. Unfortunately, this awareness does not appear to have extended to the social sciences even though it would seem obvious that it is equally, if not more, important that we learn better how to control the weapons we have already developed than that we go on developing still more destructive weapons.

HAROLD J. BARNETT: The Boulding paper is most interesting, stimulating, and original. It is not really economics. Society has gained, and in a significant sense economics has contributed, a brilliant social analyst and philosopher. In this role, Boulding greatly enriches our intellectual lives; he may, in the original insights of such papers as this one, influence the future path of society; and I am personally grateful for the sheer pleasure of (to use Schelling's words) his inexhaustible fund of wit and wisdom.

The Nelson contribution is interesting and enlightening in the rough estimates he has ingeneously contrived. Observing that the defense budget is, per dollar, about ten times as intensive in utilization of R and D resources as civilian final demand, he is concerned about the effect on employment of R and D resources, in the hopeful event that the arms race were cut back. I offer the datum that "education" may be even more intensive in utilization of R and D type resources than defense outlays. And I offer the value judgment that allocation of unutilized R and D resources to education, in the event of arms cutback, should have the highest priority, for three reasons: (1) it is more reasonable to view the origin of technological advance in civilian society as R, D, and E(ducation) than R and D alone (see my *Annals*, January, 1960, article, and Boulding's and my comments at the NSF—Ohio State U. technology conference, fall, 1962, to be published); (2) education upgrades the seed from which basic R and D originates and provides the favorable climate in which it flourishes; (3) the contemporary society needs improvement in education and social decision-making ability more than government accelerated physical research. On this last point, I think society is experiencing indigestion from a recent pace of physical technological change which is already beyond our ability to assimilate without serious social distress. Such technological change is yielding sharply diminishing social welfare returns. Enlarged, basic R, D, and E in universities would be preferable, I think, to present federal government intentions to accelerate purely physical and engineering research in such fields as textiles, housing, and urban transportation, whose difficulties are, in various respects, institutional rather than physical.

The lucid Enthoven paper stimulates mixed feelings. Beyond the parochial pleasure of seeing economist friends in subcabinet positions, the introduction of economic rationality into Defense Department decisions during the past two years has been heartening. If big-war deterrence is defined as a second-strike thermonuclear force capable of destroying Russia, we now have it. In small-war deterrence, defined as capability in conventional forces to respond quickly and effectively to Soviet and Chinese exploratory adventurism, we have made considerable progress. Both of these became possible because of the new Pentagon economic rationality in conception of problems and economic efficiency within the politically constrained total budget for defense. Moreover, a significant degree of proper civilian control has been restored in high policy defense decisions.

I shall now, ungraciously and greedily, list several improvements I still hope for from the economists and others in the Pentagon:

1. During his 1960 campaign, President Kennedy criticized the Republican administration for having (I think the figure was) less than a hundred people engaged in research and analysis on means for changing the present world society, tetering on the edge of war, to one bent toward peace. I am waiting for signs of serious and large-scale efforts, utilizing Pentagon funds, for open (unclassified), uncontrolled, and long-term peace research, by such organiza-

tions as the Institute for Defense Analysis, RAND Corporation, and (most important) universities. I am hungry to see research on what Boulding called "integrative systems": on the so far intractable problem of persistently unemployed resources in the advanced nations and desperate, war-inciting needs in the low-income ones; on possible long-term pathways to a world police force; on the emergent nations' tendency to disturb the already precarious world peace, by playing ourselves and the Russians off against each other; on the distorted images each major nation has of the others; on how to avoid a situation in which many nations have thermonuclear weapons; on the long-term implications for peace of the Common Market and of the Triffin proposal for internationalization of gold reserves; on any researchable hypothesis which might contribute to greater peace-stability a decade or a generation from now.

2. During the visit of the Russian economists to this country about three years ago, their most persistent question concerned the depressive effects on the U.S. economy (employment, incomes, etc.) of a great reduction in military outlays, in the event of major world disarmament. The U.S. economists told them that no major research and definitive planning for this event was under way. The Administration has changed, but I am not aware that the government's lack of national economic plans and preparations for a cutback in the arms race has. I wonder what effect this has on the image of America, as a nation strongly desiring reduction in tension and cutback in the arms race, to the Communist world, to the neutrals, and even to the populations of our Allies. Mr. Enthoven's paper indicates that one of the major military accomplishments of the New Frontier is its preparation for a greater array of contingencies. Is this contingency too remote or unrealistic to plan for?

3. Secretary McNamara and the Pentagon have been tardy in realizing that their massive procurement power cannot be wielded as if they were individual shoppers, searching for best value without concern for economic, political, and other social effects, including geographic implications. There have been some recent signs of awareness, but I have seen little or no evidence of genuine understanding. The situation requires more than *ad hoc,* short-run political, public relations response. There are three aspects of this myopia, each of which suggests that the Pentagon should accept the proposition that defense economics is political economy and not merely decision making of the operations research type:

First, there is the matter of understanding why the Defense Department is not privileged to ignore drastic regional dislocations and unemployment effects of its procurement policies and decisions. The reason is simply that a national government agency which spends every tenth dollar of the national income and much larger fractions in particular industries and localities cannot engage merely in narrow "suboptimization" (to use Charles Hitch's term). Pentagon procurement, because of its size, requires explicit balancing of social costs and benefits and concern for regional employment and income consequences.

Second, like former President Eisenhower, I am concerned over the development in recent years of a military service-industrial corporation axis of frightening social power. The political virility of the military-industrial power complex, however, derives ultimately from Congressional and grass-roots observation that the Pentagon does not seriously appraise the social costs which its procurement policies engender, and thereby unnecessarily inflicts drastic economic dislocation on regions and communities. In order to undermine the political strength of the military-industrial complex of which President Eisenhower warned before it becomes as permanent and strong a force as (say) the Corps of Engineers-Rivers and Harbors–Congress alliance, explicit consideration should be given to social costs and to avoidance of unnecessary employment and income dislocations in procurement decisions.

Finally, a compelling reason for considering the political economy of procurement decisions is that the nation will not otherwise permit the new economic rationality which Mr. Enthoven describes to continue. I think this is what Congressman Vinson was trying to tell the Administration when, after terming McNamara the best Defense Secretary we have had in recent history, he nevertheless refused to permit the Secretary to cancel the RS-70 program and only postponed the showdown. There may be a Congressional hearing on Skybolt. Why is it so difficult for the Pentagon to understand that the breadth of resistance to its sensible weapons system choices, in Congress and in states, is ultimately an insistence that the Pentagon avoid inflicting unnecessary, massive, local unemployment and dislocation shocks. At the fundamental level of widespread Congressional and grass-roots acceptance of the Pentagon decisions, the insistence is not on the RS-70 or Skybolt, or on North American or Douglas, or on manned aircraft, or on Air Force aspirations, but on jobs and local income. The economic history of agriculture subsidies, of water development, of tariffs, etc., is instructive.

A procurement decision which costs $1,000 million in direct contract outlays and causes additional net social costs to the nation of $300 million is less efficient for the nation than one which costs $1,100 million and inflicts no additional net social costs. But I doubt that even direct contract costs need be affected, if the Pentagon would merely consider the new awards and cancellations of its five- or ten-year program with adequate overview. Most cancellations are not for company inefficiency but because of changes in weapon systems. And even where companies are inefficient, the government-owned facility and work force are not. New awards to nationwide, efficient companies need not leave workers stranded and greatly reduce the value of social capital.

PRICING AND RESOURCE ALLOCATION IN TRANSPORTATION AND PUBLIC UTILITIES

PRICING IN URBAN AND SUBURBAN TRANSPORT

By WILLIAM S. VICKREY
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I will begin with the proposition that in no other major area are pricing practices so irrational, so out of date, and so conducive to waste as in urban transportation. Two aspects are particularly deficient: the absence of adequate peak-off differentials and the gross underpricing of some modes relative to others.

In nearly all other operations characterized by peak load problems, at least some attempt is made to differentiate between the rates charged for peak and for off-peak service. Where competition exists, this pattern is enforced by competition: resort hotels have off-season rates; theaters charge more on weekends and less for matinees. Telephone calls are cheaper at night, though I suspect not sufficiently so to promote a fully efficient utilization of the plant. Power rates are varied to a considerable extent according to the measured or the imputed load factor of the consumer, and in some cases, usually for special-purpose uses such as water heating, according to the time of use. In France, this practice is carried out logically by charging according to season and time of day for all consumption but that of the smallest domestic consumers; rate changes at the consumers' meters are triggered by a special frequency signal actuating a tuned relay which connects or disconnects auxiliary registers. But in transportation, such differentiation as exists is usually perverse. Off-peak concessions are virtually unknown in transit. Such concessions as are made in suburban service for "shoppers tickets" and the like are usually relatively small, indeed are often no greater than those available in multitrip tickets not restricted to off-peak riding, and usually result in fares still far above those enjoyed by regular commuters who are predominantly peak-hour passengers.

In the case of suburban railroad fares, the existing pattern is even contrary to what would be most profitable in terms of the relative elasticities of demand. Both on a priori grounds and on the basis of the analysis of the historical experience recently made by Elbert Segelhorat in a forthcoming Columbia dissertation, it is clear that the price elasticity of the off-peak traffic, at current fare levels at least, is substantially higher than that of peak-hour traffic. If, for example, the average suburban family spends $300 per year for commuting and peak-hour trips and $50 per year for occasional off-peak trips and the commutation fares were increased by 5 per cent, causing a 1 per cent drop in this traffic, while off-peak fares were reduced 40 per cent, with a 30 per cent increase in traffic, gross revenues per commuting family would go up from $350.00 to $350.85, with operating costs if anything reduced slightly, since nearly all costs are determined by the peak traffic level. The riding public would on the average be substantially better off: the above typical family, if it maintained the same pattern of usage, would pay only $315 + $30 = $345 instead of $350 as formerly, and any adaptation that it chose to make to the new rates would represent a further benefit, since the alternative of no change would still be open to it if it preferred. Things may not work out quite this neatly in practice, but the potential for substantial gains from even more drastic revisions in the rate structure is certainly there.

Fare collection procedures are sometimes urged as an excuse for not going to a more rational fare structure, but here there has been a deplorable lag behind what a little ingenuity or modern technology makes possible. There would be relatively little difficulty in devising apparatus for collecting subway fares on as elaborate an origin, destination, and time basis as might be desired, simply by dispensing a coded check at the entrance turnstile against the deposit of an interim fare, this check being deposited in an exit turnstile which will then either refund any excess or release only on the deposit of the remainder of the fare. Bus fares represent a problem that has yet to be satisfactorily solved, but considering the vast waste of the time of operators and passengers through delays caused by present fare collection methods, a concerted attack on this problem should yield high dividends. For commuter railroads, the possibility exists of issuing machine-readable subscriber's cards, with passengers making a record of their trips by inserting the card in a register at the origin and destination stations and being billed according to the time, origin, and destination of the trips actually made by the subscriber, his family, and guests. Something like this seems to be in the offing for the new San Francisco system, which in many respects is more of a commuter service than an urban transit system. Actually, it is not even necessary to enclose the stations in order to use such a system: proper registering at the stations can be enforced by dispensing a dated seat check to be displayed during the trip and deposited in registering out at the destination.

Even short of such mechanization, existing ticketing arrangements are needlessly clumsy, involving in many cases a duplication of effort between station agent and conductor and fairly elaborate accounting and auditing procedures. The New York Central has recently taken a step forward in this respect by arranging to mail monthly commutation tickets to patrons and receive payment by mail. Gross delinquency appears to be running appreciably less than the saving in ticket agents' time, and the net credit loss is undoubtedly much less than this, since many who fail to return or pay for their tickets in fact do not use them, as when they die or move away. Another wrinkle worth trying would be the use of a universal form of multiride ticket, to be sold by ticket agents or conductors at a flat price of $5.00 or $10.00, validated for bearer and those accompanying him, with a liberal time limit, for a number of rides or trip units depending on the stations between which it is designated to be used by appropriate punches at time of sale. An off-peak differential could be provided in conjunction with this type of ticket by providing that two units would be charged for an off-peak ride as against three units for a peak-hour ride. The ticket itself, for a typical suburban route, need be no larger than an ordinary playing card. Accounting would be greatly simplified, conductor's cash fare transactions would be both simplified and greatly reduced in number, and the use of the service would become much more convenient for passengers. Such a ticket would provide a more effective off-peak differential than the shoppers' type of ticket, since those who are either going or returning during the peak or are returning at a later date cannot usually avail themselves of such tickets.

But while suburban and transit fare structures are seriously deficient, the pricing of the use of urban streets is all but nonexistent. Superficially, it is often thought that since reported highway expenditures by the state and federal government are roughly balanced by highway tax and license revenues, the motorist is on the whole paying his way. But what is true on the average is far from true of users of the more congested urban streets. Much of the expenditure on such streets is borne by city budgets supported slightly if at all by explicit contributions from highway sources, in most states. More important, much of the real economic cost of providing the space for city streets and highways does not appear in the accounts at all, being concealed by the fact that this space has usually been "dedicated" to the public use at some time in the past. It is extremely difficult to make close evaluations from the scanty and scattered data available, but very roughly it appears to me that if we take the burden of all the gasoline and other vehicular taxes borne by motorists by reason of their use of city streets, this amounts to only about a third of the real economic cost of the facilities they use. In current terms, the high marginal cost of increased street space becomes painfully apparent whenever a street widening scheme is evaluated. Even in terms of long-range planning, urban expressways cost many times as much as expressways in rural areas built to comparable specifications, and while the flow of traffic may be greater, this is not enough to come anywhere near amortizing the cost out of the taxes paid by the traffic flowing over the urban expressways. Even when tolls are charged in conjunction with special features such as bridges or tunnels, these seldom cover the cost of the connecting expressways and city streets. And except where the street layout is exceptionally favorable, such tolls usually have an unfavorable effect on the routing of traffic.

The perversity of present pricing practices is at its height, perhaps, for the East River crossings to Long Island and Brooklyn. Here the peculiar political logic is that the older bridges are in some sense "paid for," and hence must be free, while tolls must be charged on the newer facilities. The result is that considerable traffic is diverted from the newer facilities that have relatively adequate and less congested approaches to the older bridges such as the Manhattan and the Queensboro bridges, which dump their traffic right in the middle of some of the worst congestion in New York. The construction of the proposed expressway across lower Manhattan from the Holland Tunnel to the Manhattan and Williamsburgh bridges would be at least less urgent, if not actually unwarranted, in view of its enormous cost, if, as would seem possible, traffic could be diverted from the Manhattan Bridge to the Brooklyn-Battery tunnel by imposing tolls on the Manhattan and other East River bridges and reducing or removing the toll on the tunnel. The delusion still persists that the primary role of pricing should always be that of financing the service rather than that of promoting economy in its use. In practice there are many alternative ways of financing; but no device can function quite as effectively and smoothly as a properly designed price structure in controlling use and providing a guide to the efficient deployment of capital.

The underpricing of highway services is even more strongly pronounced during peak hours. Even if urban motorists on the average paid the full cost of the urban facilities, rush hour use would still be seriously underpriced; moreover, this underpricing would be relatively more severe than for transit or commutation service. This is because off-peak traffic on the highways and streets is a much larger percentage of the total than is the case for either transit or commutation traffic; and therefore in the process of averaging out the costs, much more of the costs properly attributable to the peak can be shifted to the shoulders of the off-peak traffic than can be thus shifted in the case of transit or commutation service. The effect of this is that while the commutation fare problem is chiefly one of the overpricing of off-peak travel, and to a minor extent if at all one of underpricing of peak travel, the problem of the pricing of automobile travel is chiefly that of remedying the underpricing of peak travel, and to a relatively minor extent if at all of the overpricing of off-peak travel. These two relationships combine to give the result that even if motor traffic and commuter train traffic each on the whole fully paid their way on the basis of a uniform charge per trip, the proportion by which the peak-hour motorist would be subsidized by the off-peak motorists would be far greater than the proportion by which the peak-hour commuter is subsidized by the off-peak commuter.

A quantitative indication of the seriousness of the problem of peak-hour automobile traffic is derivable from some projections made for Washington, D.C. Two alternative programs were developed for taking care of traffic predicted under two alternative conditions, differing chiefly as to the extent to which express transit service would be provided. The additional traffic lanes required for the larger of the two volumes of traffic would be needed almost solely to provide for this added rush hour traffic, the less extensive road system being adequate for the off-peak traffic even at the higher over-all traffic level. Dividing the extra cost by the extra rush hour traffic, it turned out that for each additional car making a daily trip that contributes to the dominant flow, during the peak hour, an additional investment of $23,000 was projected. In other words, a man who bought a $3,000 car for the purpose of driving downtown to work every day would be asking the community, in effect, to match his $3,000 investment with $23,000 from general highway funds. Or if the wage earners in a development were all to drive downtown to work, the investment in highways that this development would require would be of the same order of magnitude as the entire investment in a moderate-sized house for each family. It may be that the affluent society will be able to shoulder such a cost, but even if it could there would seem to be many much more profitable and urgent uses to which sums of this magnitude could be put. And even if we assume that staggering of working hours could spread the peak traffic more or less evenly over three hours, this would still mean $8,000 per daily trip, even though achievement of such staggering would represent an achievement second only to the highway construction itself. At 250 round trips per year, allowing 10 per cent as the gross return which a comparable investment in the private sector would have to earn to cover interest, amortization, and property and corporate income taxes, this amounts to over $3.00 per round trip, or, on a one-hour peak basis, to $9.00 per round trip, if staggering is ruled out. This is over and above costs of maintenance or of provision for parking. When costs threaten to reach such levels, it is high time to think seriously about controlling the use through pricing.

It is sometimes thought that pricing of roadway use would apply chiefly to arterial streets and highways and that it would have no application to streets used mainly for access, which should allegedly be paid for by property taxes on the abutting property to which access is given. But the relevant criterion is not the function performed, but the degree of congestion that would obtain in the absence of pricing. To be sure, there would be little point in levying a specific charge for the use of suburban residential side streets or lightly traveled rural roads, since the congestion added by an increment in traffic is virtually nil in such circumstances and the wear and tear usually negligible. In effect, at these levels of traffic the economies of scale are such that marginal cost is only a small fraction of the average cost. But this does not hold for roadways used for access at the center of a city. A truck making a delivery on a narrow side street may cause as much congestion and delay to others as it would in many miles of running on an arterial highway. Even in the case of a cul-de-sac that is used exclusively for access and carries no through traffic, a firm with frequent deliveries will make access more difficult for his neighbors; only by specific pricing of such use can the firm requiring much access be differentiated from firms requiring relatively little, and encouraged to locate where its activities will be less burdensome to the remainder of the community; or to receive and ship goods at times when less congestion is generated. Some of the worst traffic congestion in New York occurs as a result of the way access is had to firms in the garment district; restrictions on truck size and exhortations have produced only minor improvement. It seems likely that a suitable charge for such use of road space would be more acceptable than an arbitrary and drastic ban, and that with a definite financial incentive methods might be found to avoid the creation of congestion.

But talk of direct and specific charges for roadway use conjures up visions of a clutter of toll booths, an army of toll collectors, and traffic endlessly tangled up in queues. Conventional methods of toll collection are, to be sure, costly in manpower, space, and interference with the smooth flow of traffic. Furthermore, unless the street configuration is exceptionally favorable, tolls often contribute to congestion over parallel routes. However, with a little ingenuity, it is possible to devise methods of charging for the use of the city streets that are relatively inexpensive, produce no interference with the free flow of traffic, and are capable of adjusting the charge in close conformity with variations in costs and traffic conditions. My own fairly elaborate scheme involves equipping all cars with an electronic identifier which hopefully can be produced on a large-scale basis for about $20 each. These blocks would be scanned by roadside equipment at a fairly dense network of cordon points, making a record of the identity of the car; these records would then be taken to a central processing plant once a month and the records assembled on electronic digital computers and bills sent out. Preliminary estimates indicate a total cost of the equipment on a moderately large scale of about $35 per vehicle, including the identifier; the operating cost would be approximately that involved in sending out telephone bills. Bills could be itemized to whatever extent is desired to furnish the owner with a record that would guide him in the further use of his car. In addition, roadside signals could be installed to indicate the current level of charge and enable drivers to shift to less costly routes where these are available.

Other methods have been devised in England, where the country can less well afford the vast outlays demanded by our rubber-tired sacred cow, and where street layouts are such as to make provision for large volumes of vehicular traffic both more costly and more destructive of civic amenities. One scheme suggested for use in a pilot scheme for the town of Cambridge involves the use of identifiers to actuate a tallying register, the rate of tallying being governed by impulses the frequency of which would vary according to the degree of traffic congestion existing in the zone in which the car is reported to be. Another extremely simple and low-cost but less automatic device would consist of a meter installed in each car so as to be visible from outside, which could be wound up by the insertion of a token sold at an appropriate price—the token being subject to inspection through a window and being destroyed when the subsequent token is brought into place. The driver can control the rate at which the meter runs down by a lever or switch which simultaneously displays a signal which will indicate to outside observers the rate currently being charged. The driver is then required to keep this signal set to correspond with the rate in effect in the zone in which he is driving as indicated by appropriate wayside signals. Extremely simple methods of varying the rate at which the meter runs down have been devised in England, which for the time being I must treat as confidential. The rate can appropriately be a time rate rather than a distance rate, since the greater the congestion the greater is the appropriate charge, so that no connection to the wheels is needed and the whole meter can be extremely compact, rugged, and cheap. The chief difficulty with this method is the likelihood that drivers will "forget" to turn the rate of the meter up promptly on entering a higher rate zone, but given a reasonable amount of policing this difficulty might be overcome after an initial period of habituation.

A slightly more elaborate version of this method would call for the changes in the meter rate to be actuated automatically in response to signals emitted from wayside equipment at the boundaries of the various zones. This would probably raise the cost to something above the level of the response block method. On the other hand, both this and the previous method are somewhat better adapted to serving to assess charges for parking as well as for moving about within an area, so that the cost of servicing and installing parking meters could be properly credited against the cost of the new system.

Another version would call for the meter to be run down by pulses emitted from cables laid along the roadway, with the pulse rate varied according to traffic density and other factors. Alternatively, the cables could be arranged to emit continuously and located across the roadway —the number of cables turned on at any one time being varied according to traffic conditions. Reliability of operation can be assured by using two alternative frequencies in alternate cables successively. The cables need not be spaced evenly, but for economy in operation may be placed in groups so that they can be energized from a single source. With either of these methods, any failure of the meter to operate could be checked by requiring the meter to be placed in plain view and arranging for a visible signal to be changed cyclically as the meter is actuated.

Adequate methods for enforcement of each of the schemes seem available which are reasonably simple, with the possible exception of the manual system, where minor negligence might be difficult to check and lead to major negligence. With identifier methods, the registering of the proper vehicle number could be checked by having a few of the detector stations equipped with apparatus to display the number being registered, which could be compared with the license plate by observers. Errors due to malfunction, as well as most fraudulent tampering, would show up as a matter of course during the processing of the records, as each record showing a car entering a zone must match the immediately succeeding record for that car leaving that zone. Cameras can also be arranged at some locations to take pictures of cars not producing a valid response signal. With meters, arrangements can be made to hold used and mutilated tokens in a sealed box; these could be inspected and their number compared with a non-resettable counter with a capacity not likely to be exceeded during the life of the car, as a part of an annual safety inspection program.

Ultimately, one would expect that all cars in an entire country would be equipped with meters or electronic identifiers. Initially, however, it would be necessary to make some provision for cars from other areas. Cars in transit or making infrequent visits to the congested area could be given the freedom of the city in a spirit of hospitality. Cars making a longer stay or more frequent visits would be required to equip themselves—say at cordon points established along the major arteries entering the controlled area. Unequipped cars would be prohibited from using the minor streets crossing the boundary of the controlled area. Such provisions would be particularly easy to enforce with electronic identifier methods: unequipped cars passing major control points would set off a camera; unequipped cars using routes prohibited to them would set off an alarm signal, facilitating their apprehension. With a meter system, checking on unequipped cars would have to be largely a manual operation and would probably be considerably less rigorous. Actually a similar problem occurs at present in enforcing provisions against the use of out-of-state license plates in a given state for longer than a limited period.

Such charging for street use could have a far-reaching impact on the pattern of urban transportation and even on the patterns of land use, by promoting a more economical distribution of traffic between various modes, the various modes being used in accordance with their suitability for the particular trip in the light of the costs involved, instead of, as at present, being chosen to suit the preferences and whims of the individual regardless of the impact on others. Motorists will no longer be manoeuvered into the position of being forced to pay for a luxury that they can ill-afford. Mass transportation will have an opportunity to develop in line with its inherent characteristics, eventually developing a quality and frequency of service that will in many cases be preferred even to the spuriously low-priced private car transportation that might be provided in the absence of a system of specific charges. Traffic-generating activities will tend to be located more rationally in relation to real transportation costs. For example, appropriate transportation charges might have been sufficient to have inhibited the construction of the Pan-American subway-jammer over Grand Central. Rapid vehicular transportation within congested areas, not now available at any price, will be generally available for meeting emergency and high priority needs where the cost is justified. Traffic will be routed more efficiently, so as to provide a smoother functioning of the roadway system as a whole. The levels of charge required to balance marginal cost and marginal benefit in the short run will provide a much more definite and reliable guide than is now available as to where and to what extent the provision of additional facilities can be justified. One can cite, for example, the extra half hour that the airlines have to allow during rush hours for the trip from East 38th Street to Idlewild, in spite of the fact that this route is almost entirely over grade-separated expressways.

One effect of such charging would be to change the relative attractiveness of different forms of mass transportation. Under present conditions, buses are involved in the same traffic tangle as the private car and are often further handicapped by their inferior maneuverability. It is then difficult to make a bus service sufficiently attractive relative to use of a private car to attract a sufficient volume of traffic to make the frequency of service satisfactory. In order to give the transit facility a chance to compete with the private automobile, it becomes necessary to provide some sort of reserved right of way. With buses this in theory takes the form of a lane reserved for them, but in practice this faces formidable problems in dealing with intersections and pickup points, and at best means that the lanes thus provided are likely to be underutilized, since it is seldom desirable to schedule just enough bus service to fully utilize a whole lane of capacity. These difficulties provide a strong argument for going to the very substantial expense of a rail rapid transit system.

With street use controlled by pricing, however, it is possible to insure that the level of congestion is kept down to the point at which buses will provide a satisfactory level of service, and rail rapid transit systems will be required only where a volume of traffic arises that will warrant their high cost on the basis of superior service and operating economies.

But while the most dramatic impact of street use pricing would be to permit the economical allocation of traffic among the various modes, it would be of great importance even in cases where intermodal substitution is not a factor. Even in a community entirely without mass-transit service, street pricing could have an important function to perform. For example, traffic between opposite sides of town often has the choice of going right through the center of town or taking a more circuitous route. Left to itself, this traffic is likely to choose the direct route through the center, unless indeed the center becomes so congested as to make it quicker to go the longer way around. In the absence of pricing, one may be faced with the alternatives of either tolerating the congestion in the center of town, or if it is considered mandatory to provide congestion-free access to the center of town, of providing relatively costly facilities in the center of town adequate to accommodate through traffic as well. With pricing it becomes possible to restrict the use of the center streets to those having no ready alternative and provide for the through traffic on peripheral roadways at much lower cost. Without pricing, bypass routes, though beneficial, often attract only part of the traffic that they should carry for the greatest over-all economy of transport.

Pricing of street use can in the long run have significant effects on the whole pattern of development of urban communities and on property values. While on general principles one can hardly imagine this impact to be other than beneficial, it is a little difficult to discern the net direction in which it would tend—for example, whether the concentration of activity at the center would increase or decrease. In order to gain insight into this problem I have been toying with a model which attempts to incorporate the essential element of choice of route, but in spite of drastically simplified structure and assumptions this model has so far resisted an analytical solution and will probably have to be worked out by simulation and successive approximations on a large electronic computer.

The model is a follows: Consider a community with a system of streets laid out in a circular and radial pattern; for simplicity, assume that the mesh of this network is small enough to be negligible; that is, that we can travel from any point in a radial and in a circumferential direction, but not at an angle. Thus any trip must be made up of radial segments and circular arcs. In effect, we assume perfect divisibility of road space, or that the capacity of a street is directly proportional to its width. In the neighborhood of any given point at a radius r from the center, a proportion of $w(r)$ of the area is devoted to streets, the remainder being devoted to business activities that generate one unit of traffic for each unit of net area; i.e., one unit of gross area originates and terminates $[1 - w(r)]$ units of traffic. The traffic originating at any one point has destinations distributed at random over the remainder of the business area; i.e., any tendency of related businesses to group themselves close together is neglected. The average cost of transportation per ton-mile (or passenger-mile) is given by some functional relation in which the density of traffic per unit roadway width is an argument. For example, we may put $x = A + B\ (t/w)^k$, where t is the volume of traffic in tons per hour and w is the width of the roadway in feet, A, B, and k being constants. A may be thought of as the operating cost of the vehicle, where the volume of traffic is negligible, the second term being the additional costs experienced due to delays resulting from congestion; k is the elasticity of this congestion cost, which can be thought of as being proportional to the number of added minutes required to cover a given distance as compared to the time required in the absence of conflicting traffic. A relation of this form was found to fit data from the Lincoln tunnel extremely well up to close to the point where a queue begins to accumulate, with a value of k of about 4.5, so that the marginal congestion cost is some $k + 1 = 5.5$ times the average congestion cost per vehicle. In other words, according to this data, an individual who has to take ten minutes longer to make a given trip than he would if there were no interference from other traffic causes 45 vehicle minutes of delay in the aggregate to other vehicles with whose movements he interferes. Unfortunately, comparable data for the more interesting case of travel over a network of city streets could not be found, but something of this order of magnitude is generally to be expected.

It can readily be shown that optimum allocation of the street space in a given small area between radial traffic and circular traffic calls for the space to be allocated in proportion to the traffic so that the average and marginal costs are the same in both directions. We can thus combine the circular traffic and the radial traffic and speak of the relation of costs to traffic in terms of aggregate ton-miles of traffic in both directions per acre of street area. Thus the cost per ton-mile can be taken to be $x = A + B(t/w)^k$, with x in cents per ton-mile, t in ton-miles per gross acre of land, and w the fraction of the land devoted to streets, in the particular neighborhood.

Given the density of traffic as a function of r, it is possible to determine the least-cost route for any given trip on an average cost basis in which the shipper bears only the delay costs experienced by him, and alternatively on a marginal cost basis where he must pay in addition a toll corresponding to the delay his trip imposes on others. By imposing the condition that the traffic distribution thus generated shall be one which produces the cost structure leading to the traffic distribution, one gets a differential equation which in principle can be solved to give equilibrium traffic patterns. The cost of this equilibrium traffic pattern can then be integrated over the entire area to give the total cost of transportation, and this can be done both for the marginal cost case and the average cost case to get the total saving in transportation cost over a given street network brought about by the pricing of street use.

Unfortunately, the differential equation that results is of the second order and third degree, and I suspect does not admit of an analytical solution in terms of well-known functions. The next step is recourse to solution of specific cases by successive approximations.

As a by-product of this calculation, one could then also derive the equilibrium rentals that would be paid by businesses at various distances from the center, on the assumption that rental differentials would correspond to the differentials in the costs of transportation borne by the business; because of the symmetry of origins and destinations, it would make no essential difference whether shipping costs were borne entirely by shipper, entirely by consignee, or shared between them.

A further step in the analysis would be to take total cost as determined by the distribution $w(r)$ of land between business and transportation uses at various distances from the center and treat this as a calculus of variations problem of choosing the function $w(r)$ so as to minimize the total cost of transportation. In this way one could compare the pattern of land allocation that would be optimal without pricing to that which would be optimal with pricing. Considering the complexity of the problem, I hesitate to make any guesses as to the nature of this difference, except to speculate that it is likely to be somewhat surprising to many of us.

I will wind up by laying before you one final piece of unfinished business, which is the problem of developing criteria for determining how much of the area in a particular neighborhood should be devoted to transportation, given the pattern of rents in the area. Conventional cost benefit analysis, if employed here at all, would tend to take the form of comparing the rent which private business would pay for the space with the reduction in transportation cost which would result from increasing the area used for transportation and decreasing the effective density of traffic. But in connection with the present model, this rule fails to yield optimal results. Let us imagine, to make things a little more explicit, that a Comprehensive Transportation Authority stood ready to rent or lease land, to be converted from or to transportation use, to or from private business, at a price reflecting the marginal productivity of land area in a particular location in reducing the total cost of carrying out a given number of ton-miles of traffic within a given neighborood. In terms of our cost formula, the rental would be given by the partial derivative of total cost per unit area xt, with respect to changes in the proportion of total area devoted to transportation, w, the density of traffic t remaining unchanged. Thus:

$$-\frac{\partial(xt)}{\partial W} = \frac{-\partial}{\partial w}(At + Bt^{k+1}w^{-k}) = +kBt^{k+1}w^{-k-1}.$$

A business will then move to a higher rent location only if the saving in transportation costs borne by the particular business is greater than the difference in rent. However, since transportation costs are in this model borne in part by the firms with which a given firm deals, only half of the change in the transportation costs of the goods he receives and ships resulting from his change in location will be felt directly by the firm making the change, so that on the whole a firm will fail to move unless the saving in the costs of the shipments to and from the firm is twice as great as the net increment in the costs of transportation resulting from the reapportionment of the space devoted to transportation. In other words, the conventional cost-benefit analysis in terms of going rents has a strong tendency to leave business uneconomically dispersed and to result in too much space in the center of the city being devoted to transportation.

This conclusion is derived from an admittedly highly simplified model, which neglects such factors as the clustering of interrelated firms, the wide variations in the ratio of land to transportation requirements of various activities, and the possibilities for creating additional space by construction of multistory buildings, or for that matter, multilevel highways. But the model can plead not guilty to the charge of having ignored the journey to work and other passenger transportation, for, input-output analysis style, we can regard labor as the product of the household sector, and, Clayton Act to the contrary notwithstanding, as an article of commerce with a peculiarly high transport cost. The essential difference between this model and classical space economics models such as those of Von Thunen is that the latter imply a well-defined shadow price for each commodity at each point in the space, with transport taking place only between points where the price differential balances the transportation cost, whereas the present model allows for crosshauling and a certain amount of particularism in the relations between economic units. The real world presumably lies somewhere in between these two extremes. Study of journey-to-work patterns seems generally to reveal a situation a fairly long way from the Von Thunen extreme, with a great deal of crosshauling of labor of roughly comparable skill. According to this, a cost benefit analysis can justify devoting land to transportation only when the savings in transportation costs yield a return considerably greater than the gross rentals, including taxes, that private business would be willing to pay for the space. This in turn means that an even greater preference should be given to space economizing modes of transport than would be indicated by rent and tax levels. And our rubber-shod sacred cow is a ravenously space-hungry, shall I say, monster?

COMPETITION UNDER UNEVEN REGULATION

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In April, 1962, the President delivered a message on transportation. Shortly thereafter, two companion bills (H.R. 11583 and S. 3243) that incorporated one of the major policy recommendations of this message were presented to Congress. If enacted, these bills will profoundly alter the nature of transport regulation and competition. They extend the agricultural and bulk exemptions—the meaning of these terms will shortly be made clear—to carriers that do not now enjoy them. In addition, the bills propose that water, rail, and motor carriers be exempted from minimum rate regulation on passenger traffic.

The above mentioned bills represent a reversal in the trend of transport regulation. In recent decades, government and the regulatory bodies have increasingly sought administratively to overcome imperfections in the market for transportation. They have attempted to guide the various sectors of the industry toward their proper roles in the nation's transport economy. The two bills assign responsibility for determining a significantly larger part of the industry's total output to market forces. Perhaps these bills reflect a growing awareness that imperfections in administrative and regulatory processes may create distortions, difficulties, and inequities as great as those that arise from market imperfections. The present paper deals with some of these problems. Through theoretical and empirical analysis the authors attempt to justify the extension of the agricultural exemption to rail, water, and air carriers.[1] No position is taken on the bulk exemption and the new passenger exemption of the two transportation bills, since these policy changes involve issues that the authors and others at the Transportation Center have only recently begun to investigate.

The remainder of the paper is divided into two parts. First we explain the agricultural exemption as a market allocation process involving regulated and unregulated carriers. A theoretical scheme is presented that may contribute to an understanding of how the process operates at a given point of time. This scheme and some additional assumptions are then used to interpret empirical material on the allocation of agricultural exempt tonnage over time.

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[1] The latter will be ignored in the actual analysis since the tonnage of agricultural exempt commodities shipped by air is minute.

The Agricultural Exemption: Static Analysis

Efforts by railroads, large trucking firms, and the ICC to bring motor carriers under regulation were finally successful in 1935 and the Motor Carrier Act was passed. Farm groups had strongly resisted passage of the bill and, in an effort to placate them, an amendment was added to it. This amendment provided that motor carriers engaged in the haulage of unmanufactured agricultural commodities would be exempt from rate, entry, and other controls of an economic nature.[2] Thus, the bill left unchanged competition between motor and all other carriers for haulage of unmanufactured agricultural products. On the supply side there were still two transport sectors so far as regulation was concerned. One of them, the motor carriers, was free of economic regulation and closely resembled the operation of a competitive industry. Railroads were unaffected and still, therefore, required to carry under regulated rates.

Competition between these two transport sectors can be viewed as a variant of the price ceiling model—a variant in which government is assumed to have "unlimited" stocks of the goods whose price is being controlled. If market demand and supply functions intersect at a price that is below the control price, the government does nothing. If they intersect at a price that is above the ceiling price, the government releases from stock an amount equal to the excess demand. In these circumstances the market price is always less than or equal to the ceiling price.

Competition between regulated and unregulated carriers in the haulage of agricultural exempt commodities between any two points fits nicely into the above framework. The demand curve is the demand for transportation between any two geographic points. The supply function is the tonnage that the unregulated or competitive sector is prepared to carry at various prices between these two points. The ceiling price is the regulated rail rate. One element of the price ceiling model appears to be absent: the government's unlimited stock. However, if the regulated carriers conform closely to the common carrier concept, this element is also present. Given uncertain demand, a carrier that holds itself open to take "any" good in "any" quantity at a given price must have excess capacity. In other words, up to some quantity that is not ordinarily reached, the supply function of the common carrier is a horizontal line. The common carrier plays the role of residual claimant, supplying transportation services in suffi-

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[2] The 1940 bill to regulate water carriers also contained an exemption provision. In this case, carriers were exempted from economic regulation when involved in the carriage of bulk commodities that are not in containers and have neither transportation mark nor count.

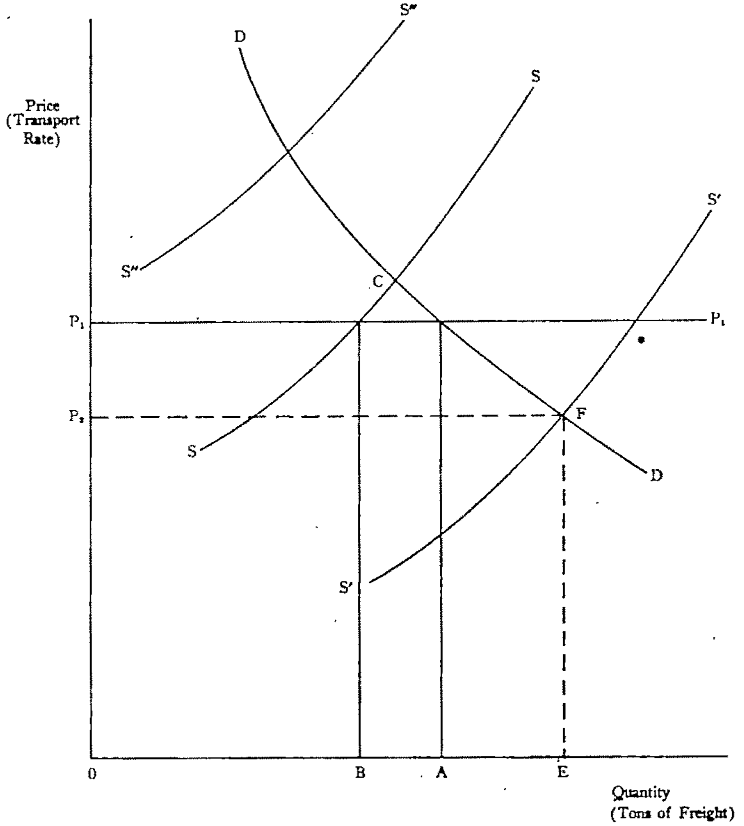

FIGURE 1

cient quantity to keep the market price from rising above the regulated price. It acts—albeit unwillingly—as "protector" of the regulated rate or ceiling price.

The above, simple scheme is illustrated in Figure 1. Here, *DD* is the demand for transportation between, say, a collection point for agricultural products and a city.[3] OP_1 is the regulated rail rate. Since this sector is assumed to behave as a true common carrier, P_1P_1 may be treated as its supply curve.[4] The diagram contains alternative supply functions for the competitive motor sector. If the competitive sup-

[3] A single demand curve can be drawn because we assume no difference in quality of service offered by the regulated and competitive sectors.

[4] It may be objected that status as a common carrier does not mean a firm has unlimited capacity. This condition is implied by our treating P_1P_1 as the supply function of the common carrier. The criticism is not well founded. The reader may, if he wishes, assume a fixed capacity. In this case the supply function has a vertical section as well as a horizontal section. Introduction of a capacity constraint will not, of course, affect our argument if the common carrier has excess capacity.

TABLE 1

1953*

Market Share Number	0	1	2	3	4	5
Commodity						
Apples	690	51	115	22	31	3
Cabbages	660	36	119	35	59	3
Cantaloupes	768	27	91	17	9	0
Carrots	777	51	40	26	18	0
Celery	821	22	30	20	18	1
Grapefruits	843	23	15	15	15	1
Grapes	848	14	31	11	7	1
Lemons	886	13	2	8	3	0
Lettuce	740	34	84	34	18	2
Mixed citrus	863	34	14	1	0	0
Mixed vegetables	763	118	21	6	3	1
Onions	611	124	61	57	55	4
Oranges	848	17	12	19	16	0
Peaches	667	63	95	33	53	1
Pears	820	37	32	12	9	2
Plums	801	51	39	12	9	0
Potatoes	501	167	80	93	70	1
Tangerines	888	3	9	7	5	0
Tomatoes	666	24	148	31	42	1
Watermelons	755	23	62	34	37	1
Total all 20 fruits and vegetables	15,216	932	1,100	493	477	22

Total possible markets for all 20 fruits and vegetables 18,240

* U.S. Agricultural Marketing Service. Fruit and Vegetable Division. *Carlot Unloads of Certain Fruits and Vegetables in 100 U.S. and 5 Canadian Cities, and Truck Unloads in 20 U.S. Cities*, calendar year 1953 (Washington, 1954). We used 19 cities and all states except Alaska and Hawaii. Thus each commodity has a total number of possible markets of 912, where a market is defined as a possible shipment from a given state to a given city.

ply function is SS and intersects the demand function at a point (C) above the regulated rate, the equilibrium quantity hauled is OA and the price is OP_1.[5] This quantity is determined by the intersection of the demand function and the regulated sector's supply function. Competition results in the unregulated sector's obtaining OB of the total tonnage. The residual tonnage, AB, that exempt carriers are unwilling to transport at the regulated rate goes by rail; i.e., the common carrier. If, alternatively, the competitive sector's supply function is $S'S'$, and intersects the demand function at a point (F) below the regulated rate, a different situation results. The quantity OE and price OP_2 are determined by this intersection; and the regulated sector carries nothing. It is still the residual carrier but there is no residue. Finally, the competitive sector's supply function may be above the regulated rate throughout, as is the case with $S''S''$. The competitive sector is unable to carry any tonnage: the residual claimant satisfies total transport demand.

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[5] The price and quantity corresponding to point (C) are not relevant. They are descriptive of the situation that would obtain if the only transport capacity in existence were that of the competitive sector.

We have been dealing with competition between regulated (rail) and exempt (motor) carriers at a given point of time in a single geographic market for transportation. Our basic reasoning can be extended to cover competition in numerous geographic and commodity markets. This more realistic case requires a generalized spatial competition model. Space limitations and a desire to emphasize policy issues rule out the development of such a model in the present paper. However, the below interpretation of empirical material on agricultural exempt traffic will reflect the more general model's structure in a respect that will be indicated.

The Department of Agriculture gathers data on truck and rail shipments of agricultural commodities from all states, except Hawaii and Alaska, to nineteen major city markets. This information is available for a number of years. Tables 1 through 3 are based on this source of data, each table pertaining to a different year. The column headings, 0

TABLE 2

1956*

Market Share Number	0	1	2	3	4	5
Commodity						
Apples	640	31	182	21	35	3
Cabbages	668	28	125	27	63	1
Cantaloupes	754	18	85	32	23	0
Carrots	776	48	51	19	18	0
Celery	802	32	42	18	16	2
Grapefruits	841	18	16	16	21	0
Grapes	848	14	32	11	7	0
Lemons	880	12	5	10	5	0
Lettuce	718	32	97	38	26	1
Mixed citrus	871	39	1	0	0	1
Mixed vegetables	795	104	4	7	0	2
Onions	616	107	78	44	63	4
Oranges	849	10	13	21	19	0
Peaches	685	29	116	24	53	5
Pears	821	26	34	18	11	2
Plums	819	35	32	15	9	2
Potatoes	497	122	116	99	77	1
Tangerines	889	0	10	6	7	0
Tomatoes	684	10	144	35	38	1
Watermelons	754	12	75	27	44	0
Total of all 20 fruits and vegetables	15,207	727	1,258	488	535	25

Total possible markets for all 20 fruits and vegetables 18,240

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* U.S. Agricultural Marketing Service. Fruit and Vegetable Division. *Carlot Unloads of Certain Fruits and Vegetables in 100 U.S. and 5 Canadian Cities, Also Truck Unloads in 32 U.S. Cities and 5 Canadian Cities*, calendar year 1956 (Washington, 1957). We used 19 cities and all states except Alaska and Hawaii. Thus each commodity has a total number of possible markets of 912, where a market is defined as a possible shipment from a given state to a given city.

TABLE 3

1961*

Market Share Number	0	1	2	3	4	5
Commodity						
Apples	660	15	216	14	7	0
Cabbages	674	16	161	20	41	0
Cantaloupes	783	30	62	21	16	0
Carrots	802	14	58	23	14	1
Celery	808	12	61	17	13	1
Grapefruits	841	8	21	24	17	1
Grapes	848	7	32	15	10	0
Lemons	877	14	4	12	5	0
Lettuce	719	19	112	29	33	0
Mixed citrus	871	37	2	1	1	0
Mixed vegetables	823	83	2	3	1	0
Onions	659	78	93	41	39	2
Oranges	840	5	24	21	22	0
Peaches	707	16	135	14	36	4
Pears	823	21	41	16	10	1
Plums	803	26	50	19	13	1
Potatoes	504	97	135	95	79	2
Tangerines	881	2	15	5	8	1
Tomatoes	711	2	134	23	41	1
Watermelons	755	9	103	5	39	1
Total all 20 fruits and vegetables	15,389	511	1,461	418	445	16

Total possible markets for all 20 fruits and vegetables 18,240

* U.S. Agricultural Marketing Service. Fruit and Vegetable Division. *Fresh Fruit and Vegetables Unload Totals in 100 Cities*, calendar year 1961 (Washington, 1962). We used 19 cities and all states except Alaska and Hawaii. Thus each commodity has a total number of possible markets of 912, where a market is defined as a possible shipment from a given state to a given city.

to 5, correspond to the below classification of markets for the transport of agricultural exempt commodities.[6] Each table was obtained by tabulating the number of cases corresponding to each market classification.

0 Indicates there were no shipments of a commodity from a particular state to a particular city.

1 Indicates that all tonnage of a commodity from a particular state to a particular city was carried by rail.

2 Indicates that all tonnage of a commodity from a particular state to a particular city was carried by truck.

3 Indicates that rail and truck transported some tonnage of a commodity between a given producing state and a given city, but that rail predominated.

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[6] It is worth expanding briefly upon what is meant by a market for transportation in the present context. A market represents a possible shipment of a given commodity between a particular state and a particular city. In other words, there is an exact correspondence between the number of markets for transporting a good and the size of the shipments matrix. Since our data pertain to forty-eight states and nineteen cities, each matrix has 912 possible shipments or transportation markets for each commodity. There being twenty commodities, the total number of markets for transportation is 18,240.

4 Indicates that rail and truck transported some tonnage of a commodity between a given producing state and a given city, but that truck predominated.

5 Indicates the carriers were of approximately equal importance in a particular market for transportation.

There is one point concerning the tables that warrants brief mention. Some readers may question their significance because the largest entry in every case is found to be the zero category. For example, according to Table 1, 690 of the 912 possible shipments of apples were of zero order. The large number of zeros in each shipments matrix is to be expected, reflecting as it does the operation of an optimization process. Thus, even if twenty states ship apples, we should not be disturbed to find that the column pertaining to Seattle has only one positive entry, Washington to Seattle. If this column had many positive entries we would suspect that our commodity and/or geographic classifications were too gross.

With minor adjustment, the simple theoretical scheme developed earlier can be used to interpret the material in any one of the tables. Thus, looking at the column labeled 1 in the first table we note that there were 932 markets that were served exclusively by rail. These are the markets in which exempt carriers were unable or unwilling to compete because of alternative and more profitable opportunities. In other words, the competitive supply function may be above the regulated rate associated with a particular transport market either because its direct costs are higher or because the opportunity cost (profit) obtained serving other markets is higher. The unregulated carriers are free to pursue profit maximization. Since capacity is limited in the short run, they may fail to serve certain markets even though their costs of doing so are below the regulated rate. The introduction of opportunity cost is an adjustment that must be made if the reasoning of the simple two-point model is to be used to interpret data for many commodities, supply places, and consumption points.

The information in each of the tables is of some interest. However, changes over time are considerably more interesting since they may indicate the long-run outcome of competition between sectors that function under quite different rules. An examination of the tables reveals that the number of markets held exclusively by rail fell from 932 to 511 over the eight-year period. The number of markets served exclusively by the unregulated or competitive sector increased from 1,100 to 1,461. The number of markets in which rail predominated fell from 493 to 418. On the other hand, the number of markets in which trucks were predominant increased between 1953 and 1956, but decreased between the latter year and 1961. The picture that emerges from the tables is that of steady decline in the position of the regulated carrier.

The reader may, at this stage, object that the main empirical finding of the paper is unrelated to its theoretical argument. The objection is well taken. Whether as a one-commodity-two-point or a multicommodity-multiregion spatial competition model, the price ceiling schema is static and descriptive of a point in time. However, certain changes have taken place in transportation which, when incorporated into the price ceiling model as shifts in functions, strongly suggest the rails long-run decline in agricultural exempt tonnage. The changes referred to are improvements in highways and motor vehicles, more efficient methods of vehicle dispatch and control, improved handling and refrigeration to reduce damage, etc. All of these changes have tended to increase the efficiency and supply of competitive transportation. Until recently, on the other hand, regulated rates on agricultural commodities gradually increased. Within the context of the price ceiling model, an increase in the regulated rate is equivalent to a reduction in the supply of regulated transport. Such shifts in functions lead to the expectation of a declining common carrier role in the competition with unregulated carriers for exempt tonnage.

Conclusion

Congress will shortly debate extension of the agricultural exemption to railroads. Economic theory and evidence on the declining role of (regulated) railroads in the competition for exempt tonnage argue for the extension.

The issue with which we have been concerned is part of a more general problem: competition between transport sectors subject to uneven and often quite different regulations. A thorough review of these inequalities, the distortions and inequities to which they give rise, and the administrative and legislative imperfections that lie behind them is in order.

PRACTICAL APPLICATIONS OF MARGINAL COST PRICING IN THE PUBLIC UTILITY FIELD

By James R. Nelson
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The word "Applications" in my title has shrunk all the way from plural to singular. For, so far as I know, the only public utility enterprise in the world to proceed from the theory of marginal cost pricing to both a schedule of rates and a series of rules for investment policy is Electricité de France (or E.D.F., hereafter)—a public corporation which is the dominant factor in the generation, transmission, and distribution of electricity in France.

My discussion of this singular price policy will be in three parts. The first will sketch in certain special features of the French electricity industry and its physical and economic environment; the second will present an outline of marginal cost pricing as applied by E.D.F.; and the third will offer a brief critique of the French approach.

I

What specific features of the French scene most affect the application of marginal cost pricing to the supply of electricity?

The first is geographical. French electricity production is about 50 per cent hydro. This hydro production is concentrated in the Alps, the Pyrenees, and the Massif Central—or just about as far as possible from Paris and most other important consuming centers, which cluster in the north. Finally, the cheapest and most abundant hydro production comes from the Alps, is derived from melting snow and ice, and therefore reaches its peak in summer. However, consumption of electricity remains concentrated in winter.

The second feature until the Pinay reforms of early 1959 was monetary. The franc lost 99 per cent of its value relative to the dollar between 1914 and 1959. The governmental response to the first bout of this inflation, after World War I, was an "economic index" for use in adjusting prices of electricity; but this index was blocked in 1935 and has never since been permitted complete freedom. Moreover, fixed charges were frozen from 1935; after World War II, industrial rates placed almost no penalty on short-hour users. So E.D.F. was confronted, from its origin in 1946, with serious problems of both price level and price structure.

Third, most of the principal public utility and transportation enterprises in France were nationalized soon after World War II. The reasons for this wave of nationalization certainly did not include any passionate public support for marginal cost pricing. But a whole series of representative public utility industries faced the same problem at the same time: how, in the Brave New World of post-1945, to find a basis for pricing which would both benefit the consuming public as much as possible and at the same time offer maximum protection against political pressure for low price levels and special price structures?

In the particular case of E.D.F., nationalization may have created an economy of scale by bringing together a group of exceptional ability which could never have been assembled by any one predecessor firm. Pierre Massé, who joined the E.D.F. in 1946 and became Assistant Director General in 1948, was a dominant figure in E.D.F. studies of investment policy and an active supporter of marginal cost studies before he left the organization to become General Commissioner of the National Equipment and Productivity Plan in 1959. Gabriel Dessus, the original Commercial Manager of the E.D.F., played a central role in developing the case for marginal cost pricing until his departure to become a banking official. Marcel Boiteux, who was first brought to E.D.F. as an assistant to Gabriel Dessus, is now both a member of the governing board and Director of Economic Studies. His contribution to the development of a marginal cost rate structure has been in all respects exceptional. Finally, Paul Stasi was coauthor with Marcel Boiteux of a classic analysis of the factors entering into the marginal cost of electricity.[1] He is still engaged in analysis of the tariff and commercial position of E.D.F.

Finally, a word about general intellectual stimuli. The idea of applying the principles of marginal cost pricing did not emerge full-blown from some E.D.F. staff meeting. Professor Harold Hotelling revived the name of Jules Dupuit as a writer on marginal costs for the English-speaking world in 1938; but the influence of Dupuit had never died in France. Moreover, the marginal cost controversy started by Hotelling in 1938 was close to its peak just after World War II, and the French reimport of marginal cost principles was an important element in the development of tariff policy by E.D.F. Professor Maurice Allais, of the Ecole des Mines, has been and is an influential academic writer on marginal cost pricing.

II

What are the marginal cost principles which have already been mentioned so often? In brief:

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[1] "Sur la détermination des prix de revient de développement dans un système interconnecté de production-distribution," Union Internationale des Producteurs et Distributeurs d'Energie Electrique, Paris, 1952 (paper delivered at Rome Meetings, 1952).

1. The first step in the short-run cost analysis proceeds from the fact that Paris is the heaviest center of electricity consumption—and at a great distance from major sources of hydro supply. Therefore, at any given hour of any day, the short-run marginal cost of electricity delivered in the Paris area is the cost of fuel for an extra unit of output from the least efficient thermal plant which must be in service to enable the total demand at that hour to be met. A price which covers the marginal cost of this least efficient plant will, of course, produce a surplus over the fuel cost of all other plants in service.

2. The exception to this general rule that the marginal price should just cover marginal fuel cost occurs at the peak. The peak kilowatt-hour is responsible for the installation of new generating capacity. Here we begin to pass from short-run to long-run marginal cost analysis. But note that the long-run marginal cost is not the total cost of a new base-load generating plant. It is the lesser of two costs, neither of which is as high as the base-load cost: The unit cost of a plant specially designed for peak loads, or the net cost of the base-load plant after subtracting fuel economies because installation of the new base-load plant permits a reduction of the output required from older, less efficient plants.

3. These thermal conclusions must be modified in two ways to allow for French hydro production:

a) Since Paris is the residual deficit market, always ready to soak up hydro production when it is available, Paris is the key to the geographical price structure appropriate for the industry. Consuming areas closer to hydro surpluses than Paris should enjoy a price equal to marginal thermal cost at Paris minus transmission cost. In the limit, the value of the electricity at the site of the hydro plant itself is determined by the cost of a kilowatt-hour from the marginal thermal source at Paris minus transmission costs from the hydro site to the capital.

b) Since hydro supplies depend on variable water conditions, they involve more uncertainty than thermal supplies. Moreover, since hydro supplies reach their peak in France in the summertime, eking out the uncertain winter supply requires substantial investment in reservoir capacity. Thus the familiar problem of supplying peak use is complicated by the further problem of assuring winter supplies of electricity. The hours of heavy, but off-peak, use during the winter months are known as the "guarantee period." Mere ability to meet the peak does not, of itself, assure supplies during this entire period, since hydro sources that may yield substantial rates of output for short periods of time may be untrustworthy for longer periods. Planning is frankly based on the assumption that supplies for any forthcoming year stand three chances in a hundred of being inadequate due to inadequate water reserves. But provision of supplies during the guarantee period in ninety-seven years out of a hundred still involves reservoir costs. Otherwise the guarantee period might become a worse bottleneck than the peak.

4. The peak responsibility theory must be modified for two further reasons.

The first of these emerged when Marcel Boiteux, of E.D.F., finally laid to rest the pseudo problem of the wandering peak. The fallacious argument for the supposed wandering peak went as follows: Suppose, after adoption of the peak responsibility theory of charging for electricity, that possible higher prices for previous peak customers plus lower prices for previous non-peak customers cause the peak to shift to a time which was previously off-peak. This will require the whole pricing system to be reversed, which will cause the peak to revert to its original position. This is not so much circular reasoning as reciprocating reasoning. M. Boiteux halted it by pointing out that the object of "peak load pricing" was to create the widest possible plateau of use. Capacity costs could then be recouped in greater or less degree from all users at the time of the plateau, in proportions determined by their demand characteristics.

The second reason for modifying the peak responsibility theory stems from the stochastic character of diversity factor. If available capacity serves only one customer, then he should pay for all of it. If the plant is used by a large number of customers whose demands are not mutually interdependent, then the fixed charges of installations common to these customers should be entirely assessed on the energy charge in the period of highest use—not the peak charge.

The first of these modifications changes the whole character of the peak responsibility theory, and the second leads to a conversion of some of the previous peak charge from a charge per kilowatt to a charge per kilowatt-hour.

E.D.F. has assembled these theoretical ingredients into a new tariff for larger industrial users, the *tarif vert,* which was introduced during 1957.

The structure of the *tarif vert* in a mainly hydro area may be illustrated by actual rates charged in the departments of the lower Rhone valley. The original annual fixed charge was 4,000 francs per kilowatt, or the equivalent of just under $10 at the exchange rate then prevailing.

The price per kilowatt-hour was varied through five standard periods: peak hours in winter, heavy-use hours in winter, slack hours in winter, heavy-use hours in summer, and slack hours in summer. The kilowatt-hour price from the 150-kilovolt transmission system ranged from 7.50 francs, or just under 2 cents, at the winter peak, down to 2.13 francs, or less than 4/10ths of a cent, in the slack hours of summer. If the slack-hour rate in summer is arbitrarily equated to 100, then the other four rates, three winter and one summer, are: 352, 253, 130, and 157. For the smallest customer eligible for the *tarif vert*—one supplied by the 5-kilovolt distribution system—the range of kilowatt-hour rates was from 13.41 francs to 2.31 francs. With the lowest rate as 100, the other rates were 581, 323, 129, and 160. The 5-kilovolt consumer was asked to pay only 15 per cent more than the 120-kilovolt consumer in summer, and only 7½ per cent more in the slack hours of winter; but the smaller user had to pay 70 per cent more in the heavy-use hours in winter and almost 80 per cent more at the winter peak.

E.D.F. has estimated that improvement in load factors due to the *tarif vert* permitted an initial saving in capacity equal to six months' investment program.

5. To give a complete picture of the scope and coherence of E.D.F. thinking on tariff matters, this section should conclude with the investment criteria—and coefficients—used in E.D.F. planning. But this paper is too short to penetrate so far into the long run. Moreover, in the critical period during which the *tarif vert* was being developed, E.D.F. was subject to extraeconomic investment constraints; it was permitted to raise only a small and controlled fraction of the funds it required from the open market and was subject to rigid government budgeting on the rest. These constraints combined with the need to expand capacity by certain minimum amounts to stimulate some ingenious exercises in linear programming. But the relationship between short-run and long-run marginal cost was still affected by the hiatus between internal and external rates of return produced by rationing the inward flow of capital. Also, with respect to the relationship of investment planning to depreciation policy, Marcel Boiteux has written extensively about the general theory of depreciation as well as about its application to E.D.F. However, his views on depreciation have not yet been incorporated into E.D.F. accounting. So the full range of marginal cost pricing from short-run fuel cost to long-run investment programming has not yet been exhaustively covered, even in France.

III

To close with a few critical observations:

1. The idea of a multidimensional rate structure to recoup the multiple dimensions of the costs of electricity supply is practically as old as the industry. The distinctive technical feature of the French *tarif vert* is that it represents some retreat from the straight two-part demand charge plus energy charge as favored by Dr. John Hopkinson,

toward the incorporation of demand components in the energy charge à la Arthur Wright. The *tarif vert* is available to larger users only; so the customer dimension introduced by Henry L. Doherty is scarcely relevant. But with half, or more, of all electrical investment embodied in transmission and distribution systems, the attempt to grapple with distance which is embedded in the new French structure represents a distinct advance. Stated only in terms of cost dimensions (i.e., neglecting cost coefficients), the basic electrical cost structure is:

U for kilowatt-hours:

plus

P for generating capacity (subject to a minimum because load factor may not exceed 100%);

plus

C for customers;

plus

$(P'D)$ for transmission and distribution of capacity to ultimate consumers (subject to the 100 per cent load factor constraint)

The French system explicitly recognizes U and P; it at least hints at $(P'D)$.

2. The French system only hints at $(P'D)$ because it is precisely in transmission and distribution that the "public utility" character of the electricity supply industry is most evident—if "public utility" is defined in terms of decreasing average total unit costs in the distribution cost dimension. Large generating plants are more efficient than small; in fact, decreasing costs in this dimension are a necessary condition for a public utility industry rather than enormous numbers of home Delco sets. But if decreasing costs in generation were not accompanied by decreasing costs in distribution, the public utility status of electricity supply would be no different from the public utility status of ice cream manufacturing. So decreasing costs in distribution provide the sufficient condition for the "monopoly in the public interest" required by the public utility concept.

The trouble is that this very condition for allowing monopoly status in the first place also produces deficits from marginal cost pricing—for, as every school boy knows, marginal cost must be less than average cost if average cost is declining.

The French system has adopted the only possible economic rule for marginal cost pricing: in an industry in which costs decrease in any dimension, the economic case for making any sales at less than marginal cost is especially weak. The consistent emphasis in the E.D.F. analysis on at least covering marginal cost provides not only a powerful reason for knowing the exact magnitude of these costs but also a long-run protection against certain types of political pressure.

But no method of economic analysis can determine, scientifically, what to do about the gap between average and marginal cost. One E.D.F. answer to this, in the era of French inflation, was admittedly cynical: if most of the capital account takes the form of fixed-interest obligations, then the rate of inflation may exactly close the spread between average and marginal cost and permit marginal cost pricing to coexist with a balanced budget. This answer not only requires acceptance of the idea of an optimum rate of inflation, as Marcel Boiteux has pointed out; it also neglects the fact that different customers are likely to demand services which mingle cost dimensions in different proportions. Since decreasing costs are more significant in some cost dimensions than in others, no averaging-out process can produce a marginal cost rate structure. In practice, the E.D.F. policy in preparing the *tarif vert* has been to add a similar percentage surcharge at all margins in order to cover total costs.

3. When the distribution cost problem involves whole areas with different densities of use of electricity, the decreasing cost problem may turn into an issue in economc development. On the one hand, why should a low-density area be charged high prices to cover high average costs—and possibly have its growth further stunted as a result? But why should a supplier of electricity have to assume a burden on behalf of areas which may have other economic advantages associated with low densities? The E.D.F. answer to all this has been ingenious: to permit high densities of use to be reflected in lower rates for the one-third of all French departments with the highest densities; but to disregard density, as a cost factor, for the substandard two-thirds.

The two previous criticisms amount to saying that E.D.F. has failed to solve an insoluble problem. Can any criticisms be made which do not simply restate the obvious?

4. Yes, the emphasis on the actual costs and technical characteristics of new facilities completely bypasses the sterility of discussions based on "long-run marginal costs" which assume no improvements in technique, and are open to the abundant fallacies of the "reproduction cost" approach. If the emphasis of E.D.F. on costs of expansion or development could be combined with the Marcel Boiteux analysis of depreciation, E.D.F. might be the only industrial enterprise in the world to have achieved consistency between the logic behind its valuation of net assets and its calculation of depreciation charges.

5. Moreover, the stress on statistics, from the pioneering work of Pierre Massé on optimum reservoir policy to the contributions of Marcel Boiteux and Paul Stasi on the uncertainties of diversity factor, opens up new vistas for analysis of the relationship of rate levels and

structures to possible economic conditions in the only period that ever matters in economics—the future.

6. Although the French electrical rate structure is a product of a special relationship between hydro and thermal resources, the idea of combining peak rates below peak costs and relatively high rates on the plateau of heavy winter use is doubly useful; it provides partial safeguards against future demand shifts and it establishes principles on which the rate structure itself may be altered if need be. The *tarif vert* might still be hovering in the wings if it had not been for the unsatisfactory rate level and structure produced by the interplay of specific freezes and general inflation. But, once on the scene, it offers more prospect for flexibility than the familiar rigid "on peak-off peak" structure of rates.

The public utility industries in the United States certainly do not lack for statistics—for their own edification or for the public's. They can be excused for a certain nervousness about encouraging the use of these statistics in public debates about the cost basis of their charges. But there is one project, suggested by French experience, in which all interests in this country might join with profit to themselves and to the consuming and tax-paying public: What public utility services are now being rendered at less than marginal cost? Why are these services being rendered? What, if anything, can be done about it, and what further consequences would any action have?

DISCUSSION

Eli W. Clemens: We owe a debt of gratitude to Professor Nelson for this well-balanced and revealing discussion of the new developments in rate-making theory in France. Whatever disagreements I might have with Nelson are minor, particularly in comparison with the disagreements I have with the E.D.F. in respect to the *tarif vert*. My comments will then be directed toward the tariff as outlined by Nelson rather than towards his paper.

I am rather skeptical of marginal cost pricing proposals as commonly put forth. In my mind, they are oversimplified. If the assumptions are granted, one cannot quarrel with the theory, but the assumptions are such as to make the applicability in practice extremely questionable. The *tarif vert* is a more sophisticated version of marginal cost pricing but one which is nevertheless subject to some of the usual infirmities. A few of these infirmities may be listed as follows: (1) The tacit assumption that utility is measurable and that interpersonal comparisons of utility are feasible. (2) The failure to recognize that the value of service and ability to pay are as applicable to utility rate making as they are to taxation. (3) The easy reliance on the tax system to adjust divergences between average cost and marginal cost despite the fact that the tax system itself is the greatest example of nonmarginal cost pricing. (4) The difficulty of identifying and isolating the marginal unit of service. (5) The difficulty of calculating marginal cost. (6) The failure to consider the relevance of conditions of monopoly and competition in related segments of the economy. (7) The failure to make any allowance for the possibilities of differential rate making. (8) The failure to consider adequately the great body of rate-making research, experience, and practice. I do not say that there may not be others. The E.D.F. and the French economists who have turned their attention to the subject have recognized these limitations of theory, but in my mind, not adequately. These criticisms apply more accurately to marginal cost pricing theories as put forth by some economists, such as Hotelling, than to those of the E.D.F.

As I gather from Nelson, the two significant ways in which the *tarif vert* differs from power rates commonly found in this country are: (1) The *tarif vert* is built on the distinction between off-peak and on-peak consumption and recognizes for this purpose five rate-making periods. American practice does not recognize the difference in such a pointed and obvious manner. (2) The *tarif vert* offers only a flat price per kilowatt of demand and a flat price per kilowatt-hour of energy. There are no blocks of differential rates as are universal in American practice.

Lacking specific knowledge of French conditions I will frame my comments around the question as to whether the *tarif vert* offers anything to American utility practice. I will turn first to the peak pricing problem.

Both E.D.F. and the American industry recognize the value of a flat peak, or of a 100 per cent load factor. To obtain it the E.D.F. is willing to charge on a bare-bones marginal cost basis for what otherwise would be off-peak consumption. The American industry clings to the concept that all customer groups should make some contribution, if even a minumum one, to profit and overhead costs. I might add that off-peak pricing on a bare-bones incremental cost basis would probably be regarded as unduly discriminatory in this country in many instances and would come under regulatory ban. This, of course, does not affect the economics of the problem.

Off-peak capacity may be seasonal or daily. The proposition that off-peak customers should be charged with none of the capacity costs but only the marginal energy costs as long as they do not contribute to the peak (or peak plateau) is not a new one. Davidson, Vickrey, Steiner, Hirshleifer, and others have elaborated on it. Utility rate men have been mindful of the marginal aspects of off-peak pricing although they have not been willing to accept it as a controlling basis for pricing. Speaking generally, I would say that if American utility companies believe that there is a substantial off-peak load that can be developed, a special rate will be put into effect to develop it. Examples are special off-peak rates for controlled water heating, street lighting, the so-called "objective" rates of the thirties, and off-peak discounts for industrial customers, and other special rates. Such rates are not likely to be marginal cost rates but will be expected to make a small contribution towards overhead or capacity costs. Two notable exceptions occur to me: low rates designed to forestall the development of rural co-operatives or municipal ownership, and special "loss leader" rates designed to entice the customer to use electricity rather than gas for all possible services.

I am most skeptical as to whether low daily off-peak rates would encourage consumption except in the case of special circumstances. The demand for electricity is a derived one and constitutes a very small element in the cost of production. No manufacturer would put on an extra shift just to use cheap electricity. Even if we only partly accept the adage that overtime involves 50 per cent more pay for 50 per cent less work, the costs would be too great. Nor is it likely to encourage any conscious shift of usage from winter to summer months. Seasonal rates, however, might encourage conservation in the winter time and waste in the summer. And again, in special circumstances, the rates might encourage the seasonal use of some appliances. In United States, the problem is academic since the development of the air-conditioning load has resulted in a summer peak for about half of the utilities and a practicable peak plateau for virtually all utilities.

Even more significant as far as the daily peak is concerned is the fact that any customer already using power can get off-peak power at a lower rate by stepping up consumption on the so-called "trailing block." A quick survey of power rates in ten representative eastern and southern cities indicated that a normal power customer taking power at a 27.4 per cent load factor would pay an average of 2.20 cents per kilowatt-hour, but additional usage off-peak would be billed at rates ranging sharply downward to 1.20 cents and 1.00 cent per kilowatt-hour. This is not as great a drop as indicated by Nelson's figures for the E.D.F., but it is substantial and in my mind closer to real marginal costs than those of the French tariff.

I would certainly be hesitant in accepting the fuel cost as a complete measure of marginal cost. There would be a substantial increase in metering and billing costs, and a considerable amount of depreciation and maintenance expense as well as a smaller proportion of operating expense would be proportional to use. The development of a flat peak would make both routine and emergency maintenance work more difficult and probably increase the need for stand-by capacity. A wide differential between off-peak and on-peak rates might have some troublesome effects on competition between users of service and create problems of public acceptance. In this country, the effect might well be to shift the unfavorable load characteristics from the electric utility industry to the gas utility industry with a consequent diminution of the social gains involved.

On the other hand, if I read Nelson right, the *tarif vert* is subject to a rather serious criticism in that it fails to recognize the decreasing cost of service per customer whether measured by demand or energy. A kilowatt charge graduated according to transmission voltage seems to me to be an entirely inadequate substitute for either of these rate parts. In this country, transmission and distribution expense including fixed charges and a return account for approximately 38 per cent of the total costs of electricity. The per unit costs of transmission and distribution obviously decline with the increase in the customer's use of either energy or demand. No consideration is given to the power factor. Insofar as these considerations are ignored, the *tarif vert* cannot be said to conform either to marginalist or to full cost rate-making principles.

In my mind, the E.D.F. is the victim of the same fallacy that characterizes Hotelling's thesis; namely, the failure to allow for differential pricing. Optimization of social welfare does not require that all output be priced at marginal cost; all that is required is that the marginal unit be priced at marginal cost. The requirement may be met satisfactorily and without government subsidy by a well-designed rate system such as the common residential block rate or the Hopkinson or Wright industrial rates. Under these rate forms, the charge per kilowatt-hour is graduated downward with the marginal unit being priced at marginal cost. These rate systems conform to what is substantially true marginal cost regardless of the amount taken. A flat per unit rate, as assumed by Hotelling, and as incorporated in the *tarif vert,* does not. In addition, a government subsidy is made unnecessary in the cases where marginal cost is less than average cost by charging higher prices, which conform to costs, for supermarginal consumption.

I am not at all certain that Boiteux has solved the problem of the so-called "wandering" peak. It is true, as Nelson points out, that if the *tarif vert* achieves a broad flat peak, the problem of capacity cost allocation disappears. But it disappears only as a static consideration. Under dynamic conditions and with the uneven growth of loads substantial changes in rates may be necessary. The practical problems involved in changing rates may be just as important as the theoretical problems of cost allocation. For example, today many utilities have built up their off-peak load by the use of low rates for air-conditioning, only to find that air-conditioning is now responsible for the peak. To raise rates after a customer has committed himself to a heavy expenditure on equipment may be regarded as a fraud on the consumer. At least many consumers would regard it as such. I do not think we can easily shrug off the benefits of stability.

Finally, I would raise some question whether the full utilization of capacity should be an all-exclusive objective of policy. Much can be said for the social value of excess capacity. Thus, we met the production demands of World War II by moving to second and third shift operation, a process that was made easy by off-peak capacity of the electric utility industry. Similarly, much can be said for the social desirability of multiple sources of supply, regardless of the costs involved. A multidimensional pricing system for the electric utility industry should not have the effect of crippling the gas industry.

In conclusion, I would say that the *tarif vert* seems to be subject to some severe limitations both in form and in theory. It might, however, be more applicable to French conditions than to American. On the other hand, American rate-making practice might be criticized also in that it does not give enough attention to marginal principles.

JULIAN S. DUNCAN: I shall confine my comments to the urban and suburban highway aspects of Professor Vickrey's paper. The reason is that this, in my judgment, is its most novel, possibly its most controversial and probably its most important, contribution. Its significance lies in the steadily mounting costs of bringing automotive commuters into our cities. And there is no end in sight unless a solution such as Vickrey recommends is adopted. The number of cities with population densities needing to consider the types of solutions recommended by this paper is constantly increasing. For example, the number of cities of 250,000 to 500,000 population increased from eleven in 1910 to thirty in 1960—almost triple. And cities of one million and over increased in the same period from three to five, whereas cities from a half million to one million grew from five to sixteen. As the size of the city increases, there is a rise in land acquisition costs which is more than arithmetic. The discourse faces squarely the question of whether in view of defense, space exploration, and other governmental outlays the American economic system, affluent though it may be, can afford the luxury of providing more highways into urban centers for the commuting motorist. There is general agreement among transport economists with a basic assumption made by Professor Vickrey; namely, that the highway user, be he passenger, automobile, or truck, does not pay his share of the costs of highway right-of-way acquisition, construction, and maintenance. His paper conservatively estimates that the user of city streets pays only a third of the "real economic costs" of the facilities he uses.

The magnitudes of the costs involved are by no means insignificant, but what is still more important is that the probable lower costs of alternative means of transporting the citizen into the center of town cannot easily be put on a comparable basis. There is the feverish pace of new urban and suburban highway construction.

The writer believes that Professor Vickrey is proposing a theoretically sound and technologically workable solution to the problem. Simply stated, he proposes that the users of the facilities be charged the full costs of facilities used. The social utility, furthermore, of providing a right-of-way for each vehicle does not justify subsidization. The factor which is causing the pressure for the added lane or lanes is the peak load traffic. Obviously the concept of average costs will not do. What should be considered is marginal cost. Most simply the question is: what is the added cost of providing for the peak load traffic? It follows then that the motorist who uses the highway during the peak load hour should pay this added cost.

Developments in electronics make it possible to record the time the individual motorist is on the highway at the peak load period, and digital computers make it economical to send a monthly statement to him at a very moderate cost. Cars using the highway would be equipped with response blocks. Cars not equipped with the response blocks would actuate a camera which would record its license number. Before completing these comments, I discussed Professor Vickrey's paper with a specialist in urban land economics, Professor James Gilies, of the University of California at Los Angeles. He stated, "Vickrey is right." I next discussed technological potentialities with the Chairman of the Department of Electrical Engineering at the University of New Mexico, Professor A. H. Koschman, who confirmed Professor Vickrey's findings on the technological possibilities.

Granting theoretical soundness and operating feasibility, what are the chances that such a proposal will be put into operation? It has not been possible, as is well known, to put into effect marginal pricing in allocating intercity highway user payments among different types of highway users. A neglected criterion for allocating intercity highway user payments is, of course, gross ton-miles of use. Because of subsidies, hidden or overt (and regulatory conditions), intercity freight traffic is not now being allocated on the basis of economy and fitness. Furthermore, the distorting effects of the exemption of agricultural products from highway transport regulation and the commodities clause exemption in water transportation remain in force.

Judging by the past, the chances for legislation looking toward a consistent policy of transport regulation and promotion along the lines recently recommended by President Kennedy would appear slim. One explanation, it seems to me, lies in the distribution of powers between the executive and the legislative. The Congress is notoriously susceptible to pressure groups. The manufacturers of automotive equipment as well as highway users, both urban and intercity, are well organized for the exertion of pressure. The amounts of money they can spend are by no means insignificant. And the farm block should not be forgotten. The committee chairmanship system of the Congress based solely on seniority is not conducive to needed institutional adjustments through legislation to technological changes. A recent addition, by the way, to the list of important political leaders calling for a revision of the seniority committee chairman system in the Congress is former President Eisenhower. Parliaments, we are reminded, are not necessarily immortal, and George Galloway has recently pointed out that U.S. Congress is no exception to this rule. Charles de Gaulle has been ruling, in practical effect, without the French National Assembly. You are really making a very serious criticism of the Congress when you estimate that the chances for an adequate long-deferred overhaul of transport regulation and promotion has little chance of passage. Taking a closer look at

Professor Vickrey's carefully worked out proposal, we find federal urban highway subventions so pervasive that it often becomes difficult to convince the urban voter that the added highway construction is costing him anything. The approach of the Bureau of Public Roads seems to be that questions of highway construction are primarily engineering in character. Its concept of costs, in cost-benefit analysis, is a narrow one. The idea of "true economic costs" seems to be a stranger to it. The BPR, furthermore, lagged behind the state of Kansas in the use of geological science in highway route decisions. A monument to this unwillingness to consider the views of geologists is the continuing series of landslides on the Pan-American Highway in Guatemala. The Bureau of Public Roads not only has vast sums for urban highway construction but also large sums for research in urban transportation. I have not noted any tendency on the part of the BPR to allocate its research funds to organizations or individuals who, like Professor Vickrey, raise "embarrassing" questions.

This does not imply that Professor Vickrey's "monster" cannot be slain. The effort, it seems to me, is decidedly worth making. More research is required if such policies are to have a chance for adoption. The cost magnitudes involved, not to speak of the contemporary chaos in patterns of urban land use, cry out for remedial action.

ROY J. SAMPSON: The Alexander-Moses model, evidence, and conclusions are straightforward and plausible. I have no serious quarrel with their presentation as such. I do have reservations concerning the broader concepts underlying this and similar works, however. No matter how well designed and constructed, a building cannot be more firm that its foundations.

The main stream of classical economic theory had its Keynesian revolution almost a generation ago. Concepts useful in underindustrialized economies striving to industrialize were no longer sufficient once a high level of industrialization had been reached. We have not yet had our revolution in classical transportation theory, however. This is especially true in the area of pricing and resource allocation.

No topic in transportation today is more important or more discussed than pricing. Much of this discussion, in one way or another, centers around alternate railroad pricing methods. But most of our discussions still are based upon nineteenth-century conditions. We still rely heavily upon concepts originated by Von Thunen, father of location theory and an early marginalist, early in that century; by Lardner, an important influencer of Jevons, at mid-century; and by Hadley during the latter quarter of the century. These concepts, useful and perhaps sufficient in their time and place environment, may now need some modifications.

Railroad pricing has leaned heavily upon the twin pillars of value-of-service (or "what the traffic will bear") and out-of-pocket costs. This may have been profitable to railroads—and not particularly harmful to the over-all economy—during an era in which the economy as well as the railroad industry was expanding rapidly and in which railroads had a virtual monopoly of long-haul inland transport. Today, however, the gales of intermodal competition, during an era in which transportation capacity has increased much more rapidly than the traffic using that capacity, have created a different environment. The traditional pillars have become bending reeds rather than sturdy oaks.

Value-of-service pricing cuts two ways. It means high rates—and potentially high profits in monopolistic situations—for high-value services. It also means low rates—and consequent low profits or no profits—for low-value services. Under present highly competitive conditions, high railroad rates will divert much of the most desirable traffic to other modes. Railroads, therefore, are stuck with the less profitable edge of the value-of-service concept. This makes it essential that costs be considered.

Out-of-pocket cost pricing for a firm with unutilized capacity makes sense in the short run. If applied indefinitely to a substantial volume of traffic, however, such pricing leads toward bankruptcy. It is not sensible to replace full-cost capacity which will be used at out-of-pocket cost prices. Yet this is what railroads do.

A railroad plant has a long life. Its "short run" covers a long period of calendar years—usually much longer than the tenure of any given management. As management is responsible to stockholders annually rather than in some distant long run, it is not unnatural that policies are more likely to be based on short-run rather than long-run pay-offs.

Even more important, perhaps, are the common carrier obligations under which railroads operate. After accepting a given type of traffic at rates only slightly above out-of-pocket costs, railroads find themselves legally limited in increasing these rates when their present capacity does have to be replaced or when costs otherwise rise. The common carriers' duty to serve requires not only that existing traffic be handled but even that additional capacity must be installed if necessary to handle increased traffic.

According to Interstate Commerce Commission calculations, which may be questioned in detail but which appear generally indicative of the existing situation, a substantial portion of rail traffic moves at rates lower than its full costs and a considerable portion at rates even below out-of-pocket costs. Under such circumstances, it is not surprising that many railroads are in financial difficulties. Short-run policies of the past have finally taken their toll. The long run has arrived!

Different pricing policies are necessary. We cannot undo the past, and even future pricing changes must be made with care to avoid unduly upsetting a delicately balanced economy. If railroads are to remain privately owned and unsubsidized, however, their over-all prices must cover full costs. This means that a much larger portion of traffic must pay completely for its movement.

As a start, few if any new rates should be established at levels significantly below full costs. Then existing rate levels could be brought upward, over a period of several years, to full-cost levels. Meantime, capital expenditures for handling traffic which does not cover its full costs could be curtailed. The alternative almost certainly is government ownership or large subsidies.

Such pricing would bring changes in locational patterns, interregional commodity flows, and intermodal traffic allocations. Some would be hurt, and vested interests would protest. A gradual approach would be politically necessary as well as economically desirable. But the end result would be a transportation system and capacity better fitted to our economic needs and performing more efficiently. Thus, over-all real costs of production and distribution would be reduced. This is a worthy long-run objective.

Critics of full-cost pricing like to point out the difficulties of determining actual full costs for a given commodity or movement. Many costs cannot be allocated by any kind of economic logic. To this I can only reply that practical cost determination is an accounting problem and is outside any special competence claimed by economists. We have enough to do in setting our own house in order. It is likely, though, that accountants will do a reasonably good, even if not a perfectly satisfactory, job if economists give them the proper concepts to work with. Computers make feasible much that would have been impossible a decade ago.

As little as five years ago, those who suggested that more attention be given to full-cost pricing could at best expect to be dismissed by a statement such as, "That's all very well in theory, but. . . ." Some critics, both practitioners and theoreticians, were and are much less charitable. But ideas have changed a little. For example, the Doyle Report, while not openly advocating full-cost pricing, did give full cost some moral support by tending to place more emphasis on cost of service than had been fashionable in many earlier studies. The minimum rate deregulation and extended exemption controversies have swollen full-cost ranks although, unfortunately, many of the recruits have economic axes to grind and thus are not completely unbiased. I will not say the tide has changed. But it at least appears to be slackening, and eddies are forming. The full-cost advocate is no longer buffeting such a strong one-way current.

POSTWAR GROWTH IN THE UNITED STATES IN THE LIGHT OF THE LONG-SWING HYPOTHESIS

THE POSTWAR RETARDATION: ANOTHER LONG SWING IN THE RATE OF GROWTH?

By BERT G. HICKMAN
Brookings Institution

The purpose of this paper is to examine the recent slowdown of economic activity against the background of the previous history of long swings in the rate of United States economic growth. Are we in the midst of the same kind of prolonged deceleration that occurred at least five times in the nineteenth century and twice more before World War II? If so, will it culminate in a severe depression, as it did so regularly in the past? Are there self-corrective forces at work to make for an automatic reversal? If not, can discretionary policies do the job?

These questions could not have been framed, let alone answered, were if not for the voluminous literature on the long swings which has accumulated in recent years, together with closely related writings in the field of economic growth and business cycles.[1] What I have done is to abstract from this research stockpile those findings which are significant for the purpose at hand. Space limitations prevent me from doing more than scant justice to the richness of the literature or the presentation of empirical evidence. I can only hope that the result is not unnecessarily dogmatic.

I will follow the custom of referring to the long swings as Kuznets cycles, in recognition of the pioneering work of Simon Kuznets in the field.

Salient Features of the Long Swings

The Kuznets cycles in aggregate production consist of alternating phases of rapid and slow growth rather than of fluctuations in the absolute level of output (Chart I). Since the swings are not readily discernible until the annual data have been smoothed to remove the shorter fluctuations due to business cycles, their identification and

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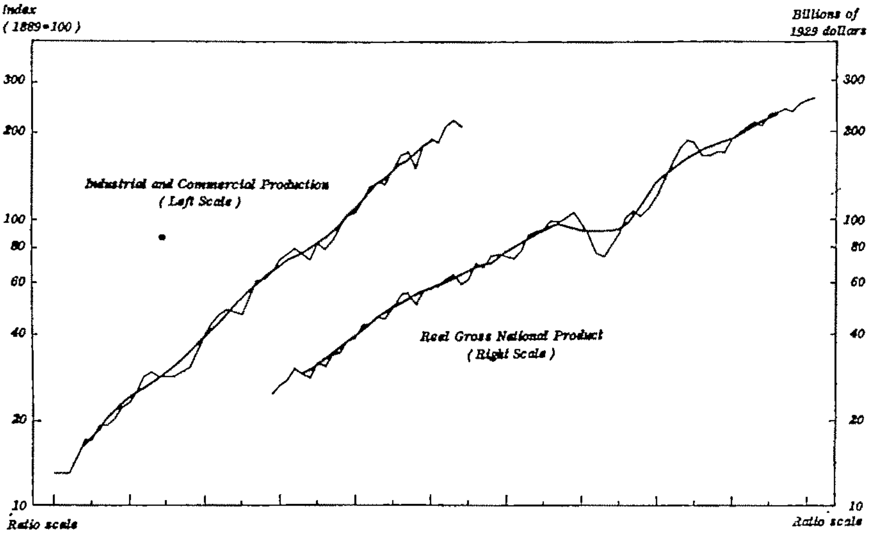
[1] The best summary discussion of the long swings is presented by Abramovitz in [2], together with citations to the major studies in the field. Reference [10] also contains an extensive bibliography, with special emphasis on demographic factors.

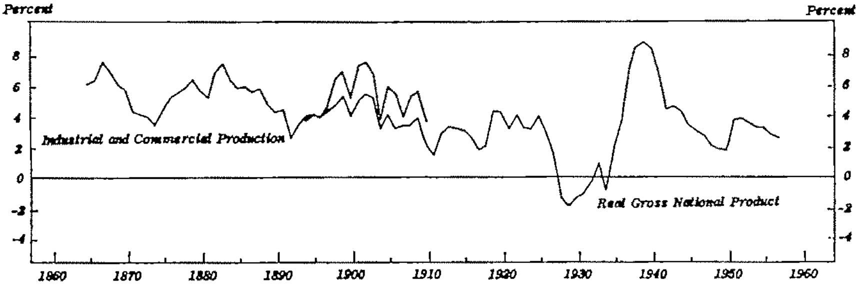
CHART I

Long Swings in Aggregate Production, 1860-1961

Annual Estimates and Nine-Year Moving Averages



Annual Change in Nine-Year Moving Average



Source: Industrial and Commercial Production: E. Frickey, *Production in the United States* (1947), Table 20. Real Gross National Product: J. Kendrick, *Productivity Trends in the United States* (1961), Table A-IIa.

dating depend partly on the choice of smoothing device.[3] It is generally agreed, however, that their average period is about eighteen or twenty years, as compared with four years for business cycles.[3]

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[3] Kuznets places long-swing troughs in 1873, 1893, 1916, and 1934, and peaks in 1882, 1908, and 1926. The corresponding dates from Abramovitz are 1874, 1892, 1911, and 1930 for the troughs and 1881, 1899, and 1923 for the peaks. These disparities are due partly to different smoothing techniques and partly to the fact that Abramovitz depends explicitly on the peaks and troughs in the smoothed rate-of-change of output, whereas Kuznets relies on "the rule of regarding a phase as terminated only when the beginning of a new phase is clearly indicated" and bases his dates on "a close inspection of the charts of decadal averages and on calculation, if necessary, of successive changes" [19, p. 351].

The amplitude of the long swings is substantial. As measured by Abramovitz, the annual rate of growth of real GNP (corrected for business cycles) exceeded 5 per cent at the long-swing peaks of 1881, 1899, 1923, and 1938, whereas it averaged about 2 per cent at the troughs of 1892 and 1911 and fell nearly to −3 per cent in 1930.[4]

The latest long-swing peak was centered in 1938 and marks the midpoint of the interval of rapid growth between the depression cycle of 1932-38 and the war cycle of 1938-46. The growth rate dropped from the 1938 peak of 7.3 per cent to 2 per cent in the early postwar years, rebounded to 4.3 per cent during the Korean war, and has averaged about 2.3 per cent in recent years (Chart II).

Viewed against the backdrop of history, then, the deceleration of recent years is not unusual. But the slowdown is not necessarily due to the same causes as before. Indeed, it is an open question whether the past swings resulted from systematic forces. It can even be argued that they are illusory.

Since moving averages can produce an appearance of cycles in a randomly generated series, it is possible that the long swings are merely a statistical illusion resulting from a randomly generated series of business cycles in which an abnormally severe depression or vigorous expansion occasionally occurs.

Now, it is quite true that the long swings are only partly independent of business cycles. Not only do major depressions leave a clear imprint on long-term moving averages (Chart I), but the troughs of the long swings consistently fall during unusually severe depressions

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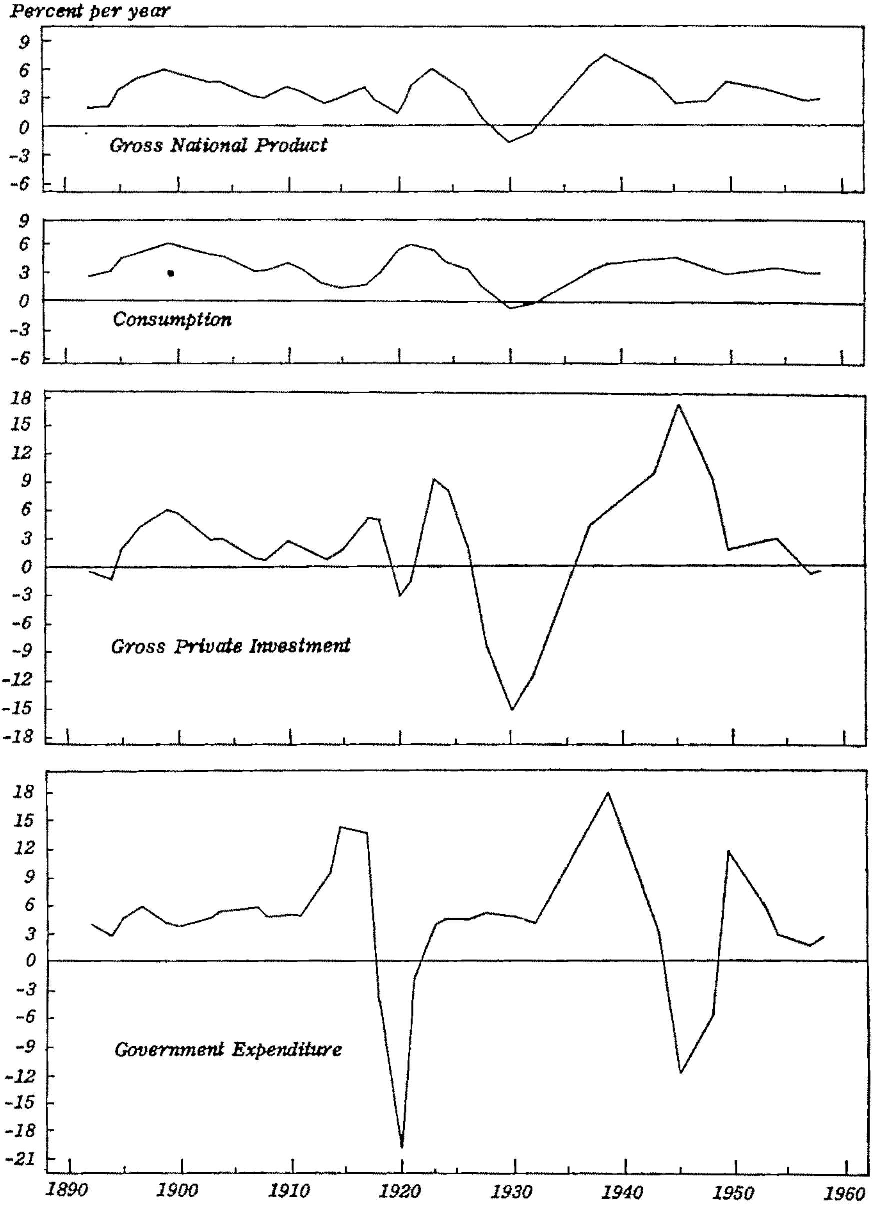
[3] Abramovitz distinguishes two additional shorter swings, one between 1886 and 1892 and the other between 1911 and 1920, although not without expressing considerable skepticism about including them in the long-swing chronology [2, pp. 232-33]. These extra movements are eliminated by the more radical smoothing techniques of Kuznets, and rightly so in my opinion. They are too short to be distinguished from major business cycles. The building cycle is an important component of the long-swing mechanism, and there was no building cycle trough during the eighties: the business contraction of 1882-85 occurred despite the continued rise of building activity. Similarly, the extra movement of 1911-20 occurred during a period when war disrupted the usual long-swing mechanism, and among other things, depressed building while accelerating total output. These observations do not beg the question, since more radical smoothing does eliminate the short "extra" movements, but without removing the longer ones or altering their one-to-one correspondence with the long cycle of building construction. If the "extra" movements are included, the average duration of the long swings as dated by Abramovitz is reduced from eighteen to fourteen years.

[4] These growth rates are based on the changes between the average levels of GNP during successive business cycles and the dates refer to the midpoints of the intervals between the centered cycle averages. The computations are made for business cycles marked off on both a trough-to-trough and peak-to-peak basis to form an overlapping series similar to a moving average but with a variable period. This device was used to compute the rate-of-change series shown in Chart II and the lowest panel of Chart III. The other estimates in Chart III are simply the average levels of the given series during successive business cycles, again with the trough-to-trough and peak-to-peak averages intermixed to form a time series.

CHART II

LONG SWINGS IN THE GROWTH RATES OF GNP AND MAJOR COMPONENTS, 1892-1958



SOURCE: Annual estimates from J. Kendrick (see Chart I). For smoothing method, see footnote 4.

or during periods in which contractions of lesser severity are interrupted only by abortive recoveries [1] [2]. But this correlation need not mean that the swings are illusory. It is equally consistent and much more fruitful to infer, as does Abramovitz, that prolonged periods of underemployment of the stock of labor and capital must form an integral part of the long-swing mechanism.

The long swings are plainly evident, moreover, in the unsmoothed data on a number of significant aspects of economic activity.[5] Even more noteworthy, the long waves in basic economic variables are systematically related in a manner implying the existence of some endogenous mechanism (Charts II and III). Among the important regularities are:

1. The long waves of output growth are accompanied by lagged swings in the volume of immigration and the rates of increase of population and labor force.
2. A long swing in the unemployment rate is a prominent feature of the Kuznets cycle.[6] The (inverted) unemployment swings also lag output growth.
3. Pronounced long waves appear in total building construction [20] [21] and in the fixed investment of railroads and public utilities [22]. Aggregate capital formation also participates in the swings, although the downphases sometimes appear as mere retardations instead of the absolute declines typical of building, railroad, and utility investment. The waves in the growth rates of aggregate capital formation and real GNP are roughly synchronous, but the peaks and troughs of the absolute swings in building regularly lag those of output growth [1, Tables 1, 7-A, 7-B].
4. A roughly coincident wave in the growth rate of the money stock accompanies that in output growth, except for substantial deviations during the World Wars.

This bare recital of characteristics is sufficient to show that the Kuznets cycles involve waves in the rate of growth of potential output due to the uneven growth of labor supply and capital stock. It is evident from the wide swings of unemployment, however, that they also involve variations of effective demand relative to potential out-

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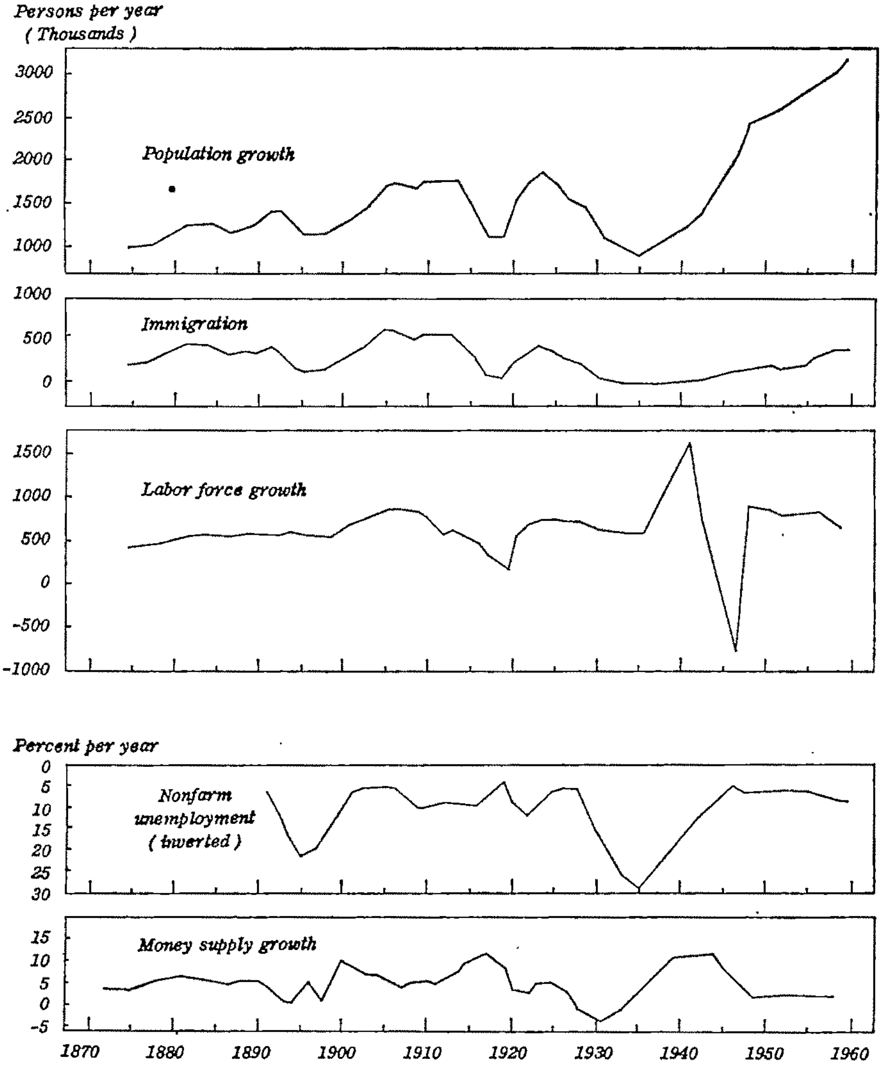
[5] This was pointed out by Kuznets [19, pp. 362-63]. On the plane of formal argument, he also observes: "The responsiveness of economic processes to short-term disturbances, which bars conceiving them as purely random changes (whether such randomness be applied to annual or cycle-average values), means a significant negative correlation between the successive values. Given such negative correlation, the probability that moving averages will generate long swings as statistical illusions becomes much less than in cases where we deal with truly random changes" (p. 365).

[6] There is direct evidence of this fact in the data on unemployment which are available since the nineties, and it may be confidently inferred for earlier years from the regular association of Kuznets cycle troughs and severe business depressions and from internal characteristics of the long swings [2, Section II].

CHART III

LONG SWINGS IN IMMIGRATION AND UNEMPLOYMENT AND IN THE ANNUAL GROWTH OF POPULATION, LABOR FORCE AND MONEY SUPPLY, 1874-1960



SOURCE: S. Kuznets (population and immigration), R. Easterlin (labor force), and M. Friedman and A. Schwartz (money supply) for National Bureau of Economic Research. Unemployment rate by Kuznets (1890-99) and Lebergott (see Chart IV). For smoothing method, see footnote 4.

put.[7] In view of the fact that the swings in the rate of resource growth were themselves largely induced by the waves of capacity utilization, moreover, it is doubtful whether Kuznets cycles would be discernible at all if effective demand had always restored the economy quickly to full employment following business downturns.[8]

Elements of the Long-swing Mechanism

An Endogenous Capital-stock Adjustment Process? Multiplier-accelerator or capital-stock adjustment models are usually viewed as explanations of a presumed Juglar or major business cycle with a duration of eight to eleven years. They may nonetheless offer some insight into the mechanism of the long swings, since the latter also involve waves of capacity utilization.

At first thought, the ceiling theories of Goodwin and Hicks appear to offer a promising lead [12] [15]. The acceleration phase of the Kuznets cycle is associated with the re-employment of idle resources during the recovery from severe depression or stagnation. Clearly, the subsequent deceleration is due partly to resource limitations, and it is tempting to conclude that the deceleration culminates in a new depression because the capital stock grows faster than output and eventually becomes excessive, inducing a reduction of investment demand

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[7] Kuznets appears to hold a contrary view with regard to the long swings prior to World War I, since he sketches a self-perpetuating mechanism for that period which makes no use of the theory of effective demand and no reference to the effects of underutilization on either investment demand or the growth rates of factor inputs and apparent productivity [19, Chap. 7]. Although containing suggestive elements, his explanation is unconvincing as a model of an endogenous long cycle. An assumed feedback of per capita consumption growth on population growth is the only element of instability in the model, and it is necessary to introduce an extraordinarily long lag (on the order of ten years, to judge from Chart 10) in the relationship to account for a twenty-year cycle. Immigration is strongly affected by domestic economic opportunities, but the appropriate index of such opportunities is the unemployment rate, which is nearly synchronous with immigration during the only pre-1914 Kuznets cycle for which unemployment estimates are available [*ibid.*, Charts 4 and 11]. Moreover, Jerome's pioneering work established that variations in employment opportunities affect immigration even during short business cycles; i.e., almost immediately [17]. Certainly there is no presumption that immigration should lag economic opportunities, however measured, by eight or ten years.

The other major defect of the model is Kuznets' assumption that the inverse movements of population-sensitive and other capital formation during the long swings were due exclusively to a continuously operating capacity constraint. Although it is true that other capital formation increased rapidly while population-sensitive investment was declining during the nineties, it is also true that unemployment was high and excess capacity substantial during the major business depression of 1893-96 and for several years thereafter [*ibid.*, Charts 7 and 11]. Similarly, Lebergott's estimates indicate substantial unemployment during the 1910-16 period of inverse investment swings. Clearly, aggregate capital formation was not constrained by capacity limitations during those periods.

[8] The swings of population and labor force growth were largely induced by those of unemployment. The fluctuations of capital-stock growth would also have been narrower in the absence of long swings of effective demand, both because excess capacity must have deterred investment in some industrial branches during the Kuznets troughs and because competition between industrial and housing demands for investment resources would have been steadier were it not for population swings. Finally, the long waves of productivity growth would have been confined to fluctuations, if any, resulting from an uneven rate of innovation.

and starting a cumulative deflationary process of the multiplier-accelerator type.[9]

This line of thought cannot be accepted, however, as an endogenous explanation of the periodic occurrence of prolonged depressions. Much has been written about the automatic fiscal stabilizers which are so prominent a feature of the postwar economy. What needs emphasis is that powerful private stabilizers existed long before the government stabilizers became important, so that the economy proved strongly resistant to deflationary forces during two out of three of the business cycle contractions of the past century.[10]

The principal private consumption stabilizer is the high marginal response of corporate saving to GNP. With regard to investment, we have the fact that a large fraction of fixed capital formation is inelastic to moderate declines of national income.[11] That is, fixed investment has a large income-autonomous component which sets a high floor to income contraction as long as other investment determinants remain favorable.

Given such a high floor under conditions favoring a large volume of autonomous investment, a check to the rate of growth from a real or monetary ceiling will result at most in a brief, though perhaps sharp, contraction.[12] The stability of final demand will ensure a quick recovery from an accelerator-induced reversal of inventory investment if the upturn is not prompted even earlier by induced declines of interest rates or construction costs or by a rise of strictly autonomous expenditure. Thus the rate of growth which can be achieved at a given time is limited by the availability of real resources and may also be checked by monetary constraints, but the economy is capable of moving along its potential output trend with comparatively minor interruptions, given favorable conditions of autonomous demand. Lags in the adjustment of actual to desired stock and the tendency for desired stock to increase along with GNP will act to prolong a new upsurge of growth once the latter is under way, but the relapse into severe depression must be attributed to a downshift of income-autonomous demand.[13]

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[9] The use of long-term moving averages could introduce a spurious lead of output deceleration over severe depression, because the depression itself will reduce the average centered some years back. However, rates of growth calculated between successive NBER business cycle peaks also indicate that output decelerates before reaching the peak preceding a major depression.

[10] Burns and Mitchell distinguish only six major contractions between 1870 and 1938, although one other (1882-85) is a borderline case [5, pp. 455-64]. There were nineteen contractions in all between the Civil War and 1938. For a comparison of prewar and postwar income stabilizers, see [14, Chap. 9].

[11] This may be because of the necessity of building far ahead of product demand (e.g., railroads, public utilities); or because product demand itself is inelastic to short-term reductions of national income (utilities; trade; services; housing); or because long-term investment expectations will be unimpaired by current, and presumptively short-lived, demand reductions in industries experiencing rapid secular growth; or because of the long lag of some kinds of investment expenditure behind investment decisions.

[12] I have discussed these matters more extensively in [14, pp. 276-85]. See also the similar analyses of Gordon [13] and Duesenberry [8, Chap. 12].

An Autonomous Building Cycle? Notice has already been taken of the wide swings of building during the Kuznets cycles. The crucial question is whether an independent building cycle sets the basic pattern of the swings in aggregate growth, or whether the building waves are themselves induced by the income variations.

An autonomous building cycle could result from an autonomous cycle of population growth and household formation, but there are compelling reasons to reject this hypothesis. The period between generations is too long to account for an eighteen-year building cycle based on population echo effects involving cycles of natural increase [14, pages 309-10]. Nor can the immigration waves between the Civil War and World War I be attributed to external causes, for Kuznets has shown that they were composed of synchronous inflows from many lands, strongly suggesting that immigrants were responding to a pull from here rather than a push from their country of origin [18, see also 9]. The pull was exerted by the ebb and flow of employment opportunities during the long swings; so the population cycle itself had a large induced component.

Another possibility is a building cycle of the cobweb type, with long lags in the adjustment of supply to demand causing recurring phases of over- and underbuilding in response to an initial disturbance. Derksen has shown, however, that the building cycle of 1918-33 was too long and severe to be explained solely by factors endogenous to the industry and that the endogenous mechanism was so heavily damped that more than one cycle was unlikely in response to a single disturbance [7]. Furthermore, Abramovitz has noted that building cycles in specific localities could arise from a cobweb process, but the existence of a national wave implies some synchronizing influence from outside the industry. The national waves of population growth not only provide the synchronizing force but also prolong the building waves by augmenting or diminishing the demand for buildings. It may be added that the swings of per capita income which accompany those of population growth also influence the rate of household formation and the extent to which housing needs are satisfied by conversions of existing structures. The concept of an independent building cycle as a driver of the long swings must be rejected.

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[13] In an interesting simulation experiment, Adelman and Adelman showed that the Klein-Goldberger econometric model of the U.S. economy produced stable growth unless subjected to recurrent random shocks, especially of autonomous investment [3]. (The investment equation of the K-G model includes capital stock and real non-wage income as explanatory variables, and hence is similar to a capital-stock adjustment formulation.) The shocked model did produce business cycles with realistic features of timing, amplitude and duration. More recently, Irma Adelman demonstrated that damped Kuznets cycles are also generated by the K-G model under the same conditions, suggesting that both autonomous disturbances and a capital-stock adjustment process are involved in the long swings [4]. However, population growth is exogenous and also does not enter the investment equation of the K-G model, nor were endogenous monetary effects admitted in the simulation experiment. A definitive simulation test of the long-swing mechanism cannot be made until these elements are incorporated in the model, as will become apparent below.

A more promising approach is a model incorporating population and labor force growth as endogenous variables along with national income and capital stock. Thus the adaptive mechanism of the economy operates to augment income-autonomous demand and to relieve supply shortages when high-level employment is reached following recovery from a depression. The advent of high employment augments population growth by encouraging immigration, marriages, and births [10] [18] [19]. One result is to speed labor force growth, which may also be accelerated by induced increases of participation rates [10]. Another is to raise housing and other population-sensitive investment demands at the very time that income deceleration is reducing some other categories of investment [2]. During the past long swings, however, these favorable population effects were insufficient to prevent a gradual deceleration of aggregate investment and output even before the cyclical downturn into severe depression.[14]

Innovational Investment. The introduction of population as a determinant of desired capital stock does not overcome the inherent inability of a capital-stock adjustment model to explain the recovery from severe depression in the face of generalized excess capacity. On the contrary, the typical sequence is for production and employment to increase and induce an upturn of population growth and building construction [14, Chart 37].

It is therefore necessary to introduce an increase of investment demand which is autonomous with respect both to income and to population. Innovational investment is the obvious candidate on both theoretical and empirical grounds. Thus far growth itself has been left unexplained. Whereas it is possible to construct a model of cyclical growth without an uptrend of autonomous investment, this requires the unreasonable assumption that innovation, and hence productivity growth, occurs without investment [12]. It is a matter of common observation that both process and product innovations ordinarily require investment outlays for their implementation, although in the latter case, the major stimulus to aggregate investment may come some years after the initial introduction of the product, when the new industry is still growing rapidly and has become quantitatively important.

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[14] Moreover, building construction led both population growth and general business on the 1873 and 1893 downturns [14, Chart 37]. Thus the induced rise of population growth postponed the building downturn by raising the desired stock of buildings, but it did not prevent actual stock from eventually increasing enough relative to desired stock to cause new building to decline before population growth.

Several writers have built theories of the major business cycle around the innovational process—Schumpeter and Hansen are leading examples—and Isard has made innovation the driving force in his model of an eighteen-year transport-building cycle [16]. The empirical support for these theories is strongest for the period between the Civil War and World War I, when the transportation and utility industries were growing rapidly. Innovations in these industries are especially likely to promote long expansions. Because of technical indivisibilities and long gestation periods, their own investment booms are apt to be long lived. More important, the growth of transportation, power, and communications facilities creates investment opportunities elsewhere, not only through the usual multiplier-accelerator mechanism, but also by stimulating internal migration of population and industry.[15]

Long expansions driven by innovational investment could end in several ways. A mere deceleration of innovational investment could precipitate a major contraction if the economy were subject to an explosive multiplier-accelerator process involving fixed investment, but this possibility was ruled out above. Second, autonomous investment could decline because innovational opportunities were temporarily exhausted. However, it is hard to see why even major innovations should not overlap sufficiently to prevent prolonged depression except possibly as an occasional, unperiodic accident. Third, a deceleration or decline of innovational investment might coincide with an endogenous decline of building construction—an hypothesis consistent with observed relationships during past long swings.[16] Fourth, there is Schumpeter's explanation that speculative excesses may develop during the boom and lead to a financial crisis and secondary deflation which plunges the economy into depression. This brings us to the last important element in the long-swing mechanism.

Monetary Factors. The growth rate of the money stock has decelerated along with output during each of the long swings since the

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[15] The expansion of the production and use of the automobile was also a powerful stimulus to investment demand during the twenties, as was the continued growth of the electric power and telephone industries. However, the boom of the twenties and depression of the thirties were so heavily conditioned by other autonomous factors—including the aftereffects of World War I on investment backlogs and population growth, the impact of restrictive immigration legislation after 1924, and the monetary difficulties associated with the reimposition and ultimate collapse of the gold standard—that it is questionable to single out innovation for a dominant role. This problem is even more acute for the years during and after World War II.

[16] The decline of building construction during the late twenties was partly exogenous, since restrictive immigration legislation helped curtail household formation.

Civil War, but the very regularity of the association bespeaks its endogenous character and renders implausible a ceiling explanation based on autonomous variations in money supply.[17] An endogenous interaction between real and monetary variables is another matter, however.

The principal possibility is the occurrence of a financial crisis arising from an overextension of money and credit during a prolonged period of high-employment growth.[18] During such periods, profit expectations are favorable and both financial institutions and the general public are prone to stretch their liquidity to the limit, especially after expansion has been under way for some years. The result of a cyclical downturn under these circumstances, whether precipitated by real or monetary factors, may be a drastic upsurge of liquidity preference and a sharp downshift of demand for goods at the given level of income.

This sort of financial crisis occurred near the outset of the depressions of the mid-seventies and mid-nineties and was largely responsible for the sharp business contraction with which the 1907-14 interval of stagnation began. The panics of 1893 and 1907 did not develop into cumulative deflationary spirals, but their aftereffects conceivably may have depressed investment demand enough to prevent the achievement of full employment during the ensuing business recoveries, because of excess capacity engendered in cyclically-sensitive industries or because income-autonomous investment was depressed by feedback effects on population growth or investor confidence. The banking panics of 1931-33 clearly deepened and prolonged the Great Depression, moreover, and the silver controversy was an important depressant in the mid-nineties.[19]

The Current Swing Compared

Apart from its exceptional length, the current long swing exhibits most of the surface characteristics of its predecessors, including waves in the unemployment ratio and in the rates of increase of population, labor force, capital formation, and money supply. This surface agreement conceals several important changes in the underlying mechanism, however, which may have permanently altered the amplitude and duration of the long swings even apart from future efforts to control them by discretionary means.

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[17] Moreover, during at least one Kuznets cycle for which data on money stock are available—that of 1892-1911—output decelerated as usual despite a rate of growth of money stock which remained persistently double that of output even as it decelerated along with output. Autonomous accelerations of money stock growth due to gold discoveries or wars have given a distinctly inflationary cast to some of the long swings without altering their physical characteristics much in comparison with the others.

[18] For a formal model of an endogenous monetary cycle, see [15, Chap. XI].

[19] With regard to the nineties, Fels concludes that "the real villain of the piece was the silver problem, without which the contraction of 1896 would have been mild and the contraction of 1893-94 little worse than 1882-85" [11, p. 218].

1. Prior to the restrictive legislation of 1924, immigration accounted for most of the variation in population and labor force growth during the long swings. Since the supply response to the swings of labor demand took the form primarily of immigration, the impact of changing employment opportunities on urban fertility and labor force participation rates was blunted [10]. The reverse has been true since the immigration curbs were enacted.

With respect to the current swing, the principal implication of the foregoing shift is that the postwar housing boom was less dependent on population increase than its predecessors. A high birth rate has little influence on the concurrent demand for housing as compared with an influx of adult immigrants. As a result of the low birth rates of the thirties, the age distribution shifted in a direction unfavorable to housing demand during the fifties. As Campbell has shown, this shift would have caused a substantial decline of new housing demand despite the rising population increments of the fifties, were it not for an unprecedented increase in the rate of household formation in each age bracket [6].

As for the future, it appears that population echo effects will have a longer period and be considerably more pronounced than in the past, owing to the wide swing of births since the last Kuznets cycle trough of the early thirties. The demographically predetermined components of population and labor force growth will assume heightened importance as compared with current endogenous determinants—a fact which has far-reaching implications for long-term projections and econometric growth models based on past experience. Neither the predetermined nor endogenous determinants of population and labor force growth can be safely neglected in such analyses, especially since they interact in complex ways.[20]

2. The monetary environment has changed significantly as compared with earlier long swings. The development of federal insurance of bank deposits and of the insured and amortized home mortgage has provided automatic safeguards against the liquidity panics and foreclosure waves that aggravated past depressions. Because of the basic modification of international monetary arrangements since the thirties, the Federal Reserve authorities have greater freedom to pursue domestic policy objectives, which have themselves been upgraded in importance. This freedom is by no means absolute, as our current problems with the balance of payments attest, but monetary deflation

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[20] See the highly suggestive analysis by Easterlin [10, pp. 890-900].

is clearly no longer an acceptable method for dealing with gold outflows.

3. Residential construction amounted to 4.5 per cent of real GNP (1929 prices) in the 1900's and twenties but to only 3 per cent in the fifties. A similar decline in the relative importance of other private construction reduced its share from 9 per cent in the 1900's to 5 per cent in the twenties and 3 per cent in the fifties. Thus the threat posed to stable growth by income-autonomous variations of construction activity, whether due to population or cobweb effects or to unsound financial practices, is much diminished. The increase of defense spending from virtually zero in the twenties to 8 per cent of real GNP in the fifties works in the opposite direction, but with one significant difference insofar as systematic growth swings are concerned. Major fluctuations of defense spending are sporadic and unpredictable, whereas the long swings of private construction have large induced and predetermined components in an augmented model containing population and financial variables in addition to income and capital stock.

4. Both the aggregate capital-output ratio and the marginal propensity to consume GNP have fallen substantially in real terms as compared with the first quarter of this century [19] [14]. *Ceterus paribus,* therefore, a given change of real national income will induce less investment or disinvestment and a smaller change of consumption demand than formerly. This is a favorable change insofar as downward stability is concerned, but it also places a greater burden on autonomous demand to provide the rising expenditures to sustain full employment growth. Fortunately, neither the capital-output ratio nor the propensity to consume is an immutable datum beyond the reach of monetary and fiscal policy, nor, in my opinion, is there any shortage of worth-while investment opportunities in the public sector.

5. The most significant structural change of all is the growth in the share of the federal government from 1.5 per cent of real GNP in the twenties to 9 per cent in the fifties. The tax increases to finance this enlarged share are chiefly responsible for the aforementioned reduction in the propensity to consume GNP and they conceivably may have contributed to the fall in the investment-output and capital-output ratios by diminishing the *ex ante* supply of saving. The central point that I want to emphasize, however, is that the growth in the federal share has enormously enhanced the leverage of discretionary fiscal actions as compared with the prewar economy.

6. Autonomous factors have always played a prominent role in the long swings, but the influence of war is especially marked in the cur-

rent one. The extended duration of its deceleration phase is due in large part to the aftereffects of the World War II demand backlogs and to the upsurge of military spending during the Korean war. Furthermore, the absolute decline of about 25 per cent in the level of real defense expenditure after 1953 is the principal cause of the deceleration of total government spending since that time. Since tax rates were reduced much less than expenditures, the defense cutback exerted a restraining influence on the growth of GNP not previously experienced except for a few years following each of our major wars, when its impact was offset by private backlog demands. The 1961 federal budget on income and product account would have yielded a surplus of roughly $9.5 billion at a potential output employing 96 per cent of the labor force [23, Chart 7], whereas in 1953 there was an actual deficit of $7.5 billion accompanying an employment rate of 97 per cent.

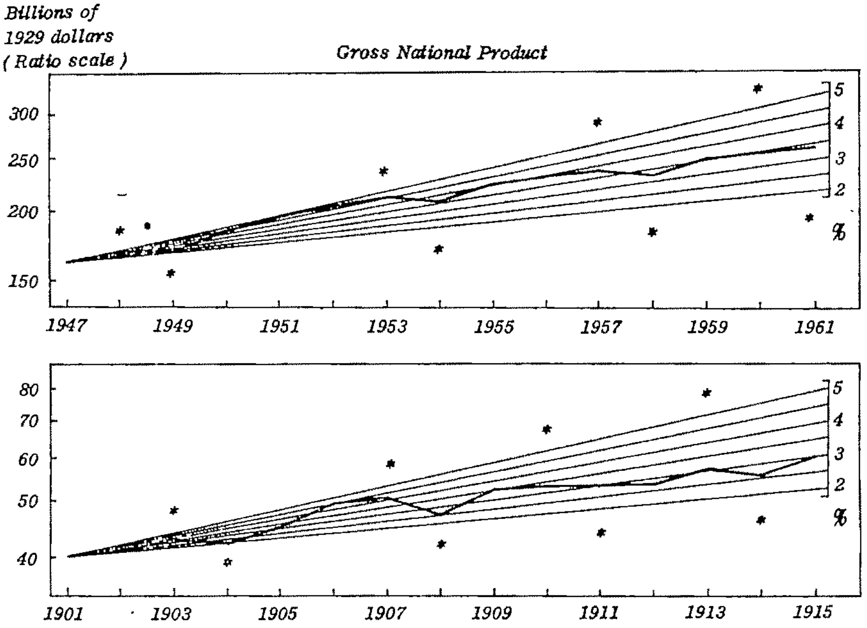
Important as these war influences are, however, they should not be allowed to divert attention from the systematic forces we have been considering. Both the war backlogs and the autonomous waves of defense spending had peacetime analogies during previous long swings—the one in the demand backlogs accumulated during prolonged depressions because of deferred capital replacements and the continuing growth of population and technical knowledge, and the other in the waves of private capital formation associated with the growth and maturation of great industries. Nothing in a century of experience with the long swings suggests an economy capable of quick enough automatic adjustment to the elimination of backlogs or the deceleration of autonomous expenditure to prevent prolonged departures from a full employment growth path.

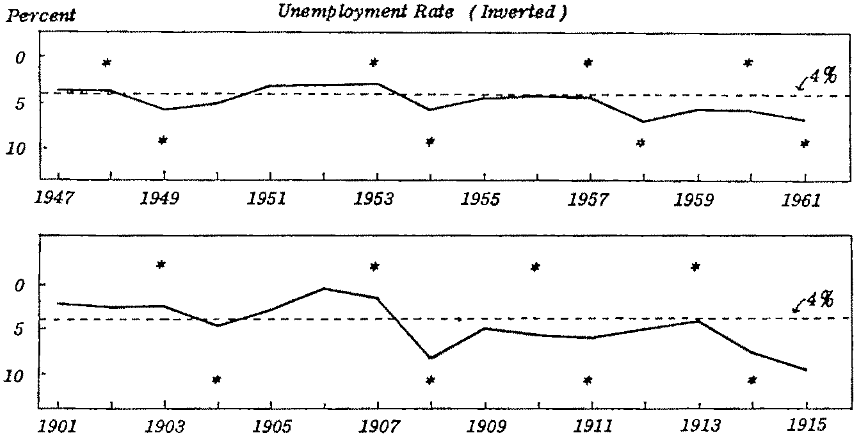
Conclusions

The long swings of the past resulted from the interaction of endogenous and exogenous forces. There was neither a purely endogenous cycle of the multiplier-accelerator type nor a regular long cycle of autonomous investment. Such periodicity as the long swings exhibited —and it was substantial—was due primarily to a combination of long lags in the adjustment of the actual stock of buildings to the desired stock and the tendency for changes in the desired stock to become amplified and prolonged by induced changes in population and income. Another apparently systematic element was the tendency for financial excesses to develop from the buoyant expectations generated by prolonged booms, so that the depression phases of the long swings were aggravated to varying degrees by liquidity crises, bank failures, and political-monetary disturbances. The long swings of experience re-

CHART IV

A COMPARISON OF GROSS NATIONAL PRODUCT AND UNEMPLOYMENT, 1901-15 AND 1947-61


Gross National Product


Unemployment Rate (Inverted)

NOTE: Asterisks indicate business cycle peaks and troughs.

SOURCE: GNP: 1947-61, Dept. of Commerce, 1900-14, S. Kuznets, *Capital in the American Economy* (1961), Table R-22, Variant 1. Unemployment: 1947-61, Dept. of Labor, 1901-15, S. Lebergott, "Annual Estimates of Unemployment in the United States, 1900-1954," *The Measurement and Behavior of Unemployment* (1957), Table 1.

flect the operation of these systematic forces as modified by wars, major innovations, autonomous changes in money supply and the financial and monetary structure, immigration legislation, and so on.

If these judgments are correct, it follows that severe depression was not an inevitable consequence of growth deceleration even before World War II. It is a matter of history, moreover, that the prolonged deceleration during the 1900's culminated between 1907 and 1914 in a period of stagnant growth similar to that of recent years rather than in a superdepression on the order of the nineties or thirties (Chart IV). But this episode also shows that full employment growth has not always been sustained even when severe cumulative contraction was avoided, and its timing is so consistent with developments during other long swings that it cannot plausibly be attributed to accidental factors. Evidently the growth equilibrium of the American economy is neither as unstable as some neo-Keynesian models would have it nor as stable as some neoclassical models appear to imply.

Most of the elements that contributed to past long swings are still part of the economic structure, but in new combinations and with altered weights that make mechanical extrapolation of past experience even less warranted than usual. The economy may continue to experience long swings, but if so, they will almost certainly be milder even without discretionary control, because of stabilizing changes in the financial structure and the diminished importance of private construction. If left alone, the present slowdown will probably be reversed in time by some new acceleration of autonomous demand resulting from innovational activity, a political-military disturbance, or a predetermined population upsurge. Since stagnant growth rather than deep depression is the more likely prospect at worst, the slowdown might even be reversed endogenously as the gap between desired and actual capital stock is narrowed by rising income and a diminished rate of capital formation. This last is a slow and uncertain process, however, as compared with the immediate results which could be achieved by a substantial tax cut to augment private demand or by an acceleration of public expenditure.

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LONG SWINGS IN RESIDENTIAL CONSTRUCTION: THE POSTWAR EXPERIENCE

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I

There is little doubt that residential construction moved through a series of long swings prior to World War II. These long movements appear with regularity in the available estimates of housing starts and residential capital formation.[1]

There is less agreement about the nature and causes of these long swings than there is about their existence. However, it can be shown that whatever other factors may have influenced the timing and relative amplitude of the long swings in residential construction, we would have experienced essentially the same fluctuations as recorded if demographic changes alone determined the level of residential construction. In other words, the prewar fluctuations in residential construction apparently constitute a family of long swings resulting from similar causes and very likely inherent in the growth process in the United States.[2]

Our particular task is to see if events in housing markets in the last fifteen years involve an exception to this prewar experience or simply continue the long swings of the past. To do so we must first take a more detailed look at the nature and probable causes of long swings in residential construction prior to World War II. This past experience provides the necessary framework for analyzing and evaluating recent history.

II

Our formulation of the relation between housing demand and population differs significantly from the usual approach. We recognize that housing demand not only varies with the size of the population but varies with the age composition of the population as well.

The age composition of the population influences housing demand because of the large and systematic differences that exist in headship rates (i.e., in the ratio of household heads to the total number of people in an age class) between age classes. As we have shown elsewhere, the inclusion of age composition as a determinant of housing demand leads to a much better explanation of household formations and so of residential construction than is possible on the basis of changes in the size of the population alone.[3]

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[1] This paper draws heavily on my doctoral dissertation, "The Housing Life Cycle and Long Swings in Residential Construction: A Statistical and Theoretical Analysis," Stanford University, 1961.

[2] See M. Abramovitz, "Nature and Significance of Kuznets Cycles," *Economic Development and Cultural Change*, Apr., 1961, for an interesting interpretation of the role of population change in the generation of long swings in economic growth.

Our procedure for estimating the change in the housing stock required by changes in both the size and the age composition of the population (which we call "required additions") follows. Assuming headship rates unchanged during the period, we first estimated required additions for past (or future) five- or ten-year periods in each of several age classes by multiplying the population change in the age class by the headship rate for the age class.[4] Then we summed the increases or decreases in the housing stock required by population changes in each age class.

It follows from our procedure that any difference between required additions and actual additions to the occupied housing stock (or household formations) must result from changes in headship rates. The difference between required additions and housing starts in any period will depend on conversions, withdrawals, and on the change in vacancies as well as on what happens to headship rates.

With this brief description of our procedures, we can now turn to a discussion of the prewar relation between population changes and residential construction. Our estimates of required additions are shown in Chart I along with estimates of household formations and housing starts. Evidently there was a regular pattern of long swings in population pressure for additions to the housing stock from the nineties through the sixties. Further, household formations followed the long swing in required additions quite closely through the forties. Prior to World War II, changes in headship rates were clearly of minor importance in explaining the number of households formed.

While housing starts (and residential construction) swung much more widely than either required additions or household formations, they followed the increases and decreases in required additions through the forties. The wider amplitude of long swings in housing starts relative to required additions would be expected because of the self-reinforcing effect of changes in residential construction on income and of changes in income on housing demand and on rural-urban migra-

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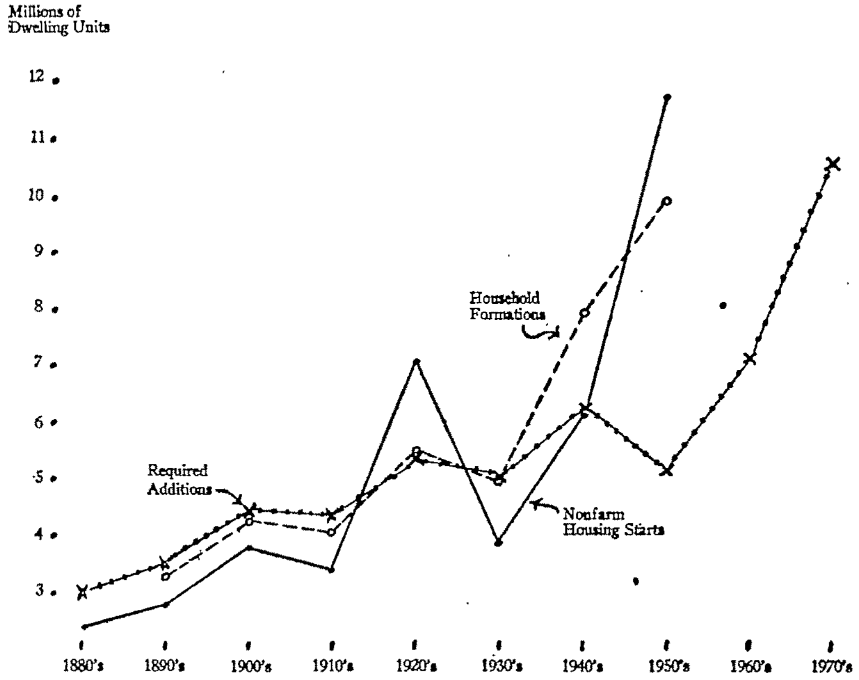
[3] See Campbell, *op. cit.,* Chap. VI.

[4] The headship rates used were as follows: 1890 headship rates from the eighties to the twenties, beginning of the period headship rates from the thirties to the fifties, 1959 headship rates for the estimates of future required additions. See my dissertation, *op. cit.,* Chap. IV, for a complete discussion of the methods used to estimate required additions.

CHART I



SOURCES:

Required Additions—Campbell, *op. cit.*, Table IX-1, page 338.

Household Formations—U.S. Bureau of the Census, *Statistical Abstract of the United States, 1961*, Table 1066, page 764.

Nonfarm Housing Starts—Eighties derived from estimates of residential construction in 1929 dollars, see S. Kuznets, *Capital in the American Economy*, Table R-30, page 576; nineties to forties from L. Grebler, D. Blank, and L. Winnick, *Capital Formation in Residential Real Estate*, Table B-1, page 322; fifties from the *Economic Report of the President, 1961*, and the *Federal Reserve Bulletin*, August, 1962.

tion.[5] The working off of wartime shortages during the twenties and the often discussed tendency for the supply of new dwelling units to overadjust to excess demand might also explain the wider amplitude of long swings in housing starts in the decades before World War II.

Of course, none of the relations shown in Chart I prove that population change was the cause of the long swing in housing starts and residential construction between the eighties and the forties. Rather, they are simply consistent with this hypothesis. However, it is reasonably clear that changes in the age composition and size of the population did account for the long swing in household formations, and

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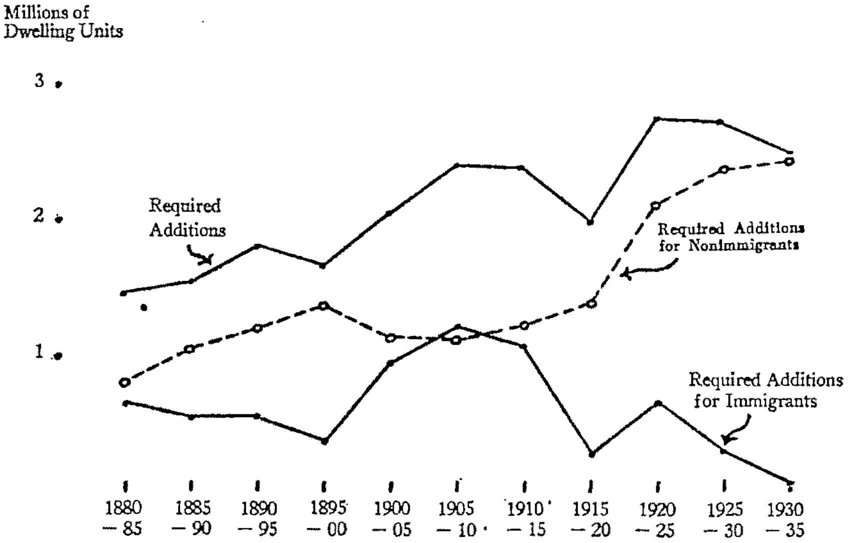
[5] Because data limitations made it necessary to compare nonfarm housing starts with national household formations and required additions, the differences between housing starts and both household formations and required additions shown in Chart I depend in part on the level of farm to nonfarm migration.

CHART II



Source: Campbell, *op. cit.*, Table VI-2, page 224.

there are strong a priori grounds for expecting to find residential construction and housing starts varying with household formations.

III

In the time and space alloted we can go no further in documenting our contention that changes in the size and age composition of the population accounted for the long swing in residential construction during the seventy years prior to 1950. Instead, we now turn to a brief analysis of the sources of the long waves in required additions during this period.

From the eighties to the fifties, the long swing in required additions can be explained by either the direct or the lagged impact of immigration on the size and age composition of population increments. Until the early thirties, required additions for current immigrants not only moved with total required additions, but in most periods they determined the sign of the change in total required additions (see Chart II).

The fact that immigration regularly determined the direction of change in required additions is particularly interesting. It means that immigration tended to raise total required additions (and so to raise household formations and housing starts) at the same time that the number of additional units required by population changes net of immigration was falling (and vice versa).

One explanation of this relation (suggested by Easterlin's recent work) is that immigration responded to swings in the growth of labor demand relative to the rate of growth of the domestic labor supply.[6] If so, then the resulting dampening effect of immigration on fluctuations in the rate of growth of per capita real income may partly explain the relative stability of headship rates from 1890 to 1940.

From the early thirties until the present time, the effect of current immigration on required additions was slight. However, the timing of the continued long swing in required additions can be traced to past swings in immigration. Thus, the increase in required additions in the forties (the largest on record) is explained by the increase in immigration immediately after the turn of the century, and the decline in required additions in the fifties can be accounted for by the decline in immigration after 1925. On the other hand, beginning with the fifties, the amplitude of changes in required additions can only be explained by the wide swings in birth rates that started in the late twenties and continued through the early fifties.

In sum, from the late nineteenth century through the forties, we have evidence of a family of long swings in residential construction largely explained by concurrent long swings in required additions, which, in turn, were largely the result of long swings in current immigration until the early thirties and of the lagged impact of immigration after that.

With this background we are now prepared to analyze and evaluate the experience of the last decade.

IV

Required additions reached a long-swing peak in the forties, and then in the fifties required additions fell by the largest amount ever. As the population pressure for additions to the housing stock moved toward a long-swing trough, housing starts and household formations increased to record levels, with housing starts exceeding household formations (see Chart I). The extraordinary nature of these developments becomes clear when one recognizes that prior to the fifties required additions and household formations always increased and decreased together and that only once before—in the twenties—have housing starts exceeded household formations!

To determine whether the experience of the postwar years heralds a complete and continuing break with the past or is just an episodic divergence from the long-run relation between population change and residential construction, one must look more closely at the reasons for the increase in household formations and for the excess of housing starts over household formations during the fifties.

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[6] R. Easterlin, "Influences in European Overseas Emigration Before World War I," *Economic Development and Cultural Change,* Apr., 1961.

Apparently the excess of housing starts over household formations was primarily the result of the huge population redistribution that occurred during and after World War II.[7] The fact that housing starts only slightly exceeded nonfarm household formations in the last decade implies that farm to nonfarm migration was particularly important in explaining the relatively large excess of housing starts over total household formations during the fifties.

However, population redistribution does not tell the entire story. For example, it seems likely that part of the excess of housing starts over household formations in the fifties can be explained by the existence of a quality backlog at the end of the forties.

Unlike the first World War, which came at the trough of the long swing in required additions, World War II coincided with the peak of the long swing in population pressure for additions to the housing stock. Production controls and materials shortages, along with the absence of any significant technological advances in the home building industry, prevented a rapid adjustment of output to this increased demand during the forties and led to a large excess of household formations over housing starts in that decade. With this difference largely filled by conversions, it seems probable that a quality backlog was established.

Finally, the unparalleled increase in demand for sales units in the postwar period helps to explain the excess of housing starts over household formations. Rising real incomes, increased birth rates, and the once-and-for-all impact of the federal mortgage insurance programs were probably most important among the many factors that contributed to this shift toward owner occupancy.

Whatever the cause of the excess of housing starts over household formations in the last decade, it was clearly much less important than the increase in household formations in explaining the break in the historical relation between population change and residential construction. In fact, housing starts exceeded household formations in the fifties by less than they did in the twenties. The most significant development during the postwar years and the major source of the divergent movement between housing starts and required additions was the first important increase in headship rates in over sixty years. Without this change in headship rates, housing starts would have followed the long swing in required additions in the fifties; with this change, the number of dwelling units started increased to over two times the number required by population change alone.

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[7] In general, changes in the geographic, quality, or tenure composition of housing demand will not change the number of dwelling units required unless they lead to an increase in the number of households. However, dwellings cannot be easily moved, cannot all be shifted between sales and rental markets in response to changes in demand, and are far from similar in quality. Thus, changes in the geographic or tenure composition of demand are likely to result in an increase in housing starts (matched by an increase in vacancies or withdrawals), even if the number of households remains unchanged. The relative downward inflexibility of rents in multi-unit structures enhances this possibility when demand shifts toward sales units (as it did in the postwar period). As long as a shift in the quality composition of demand cannot be met by converting existing structures, it will have a similar impact on housing starts.

Although an increase occurred in every age class, most of this extraordinary change in headship rates took place at the early and late life-cycle stages (see Table 1). An important result of the age composition of headship rate changes during this period was a large increase in the number of households headed by primary individuals.

TABLE 1

HEADSHIP RATES

Age Class	1950	1959	Change 1950–59
15–19..........	1.2%	1.2%	0.0%
20–24..........	16.4	21.5	5.1
25–29..........	33.2	38.6	5.4
30–34..........	40.1	44.9	4.8
35–44..........	44.9	47.8	2.9
45–54..........	49.2	51.9	2.7
55–64..........	52.5	54.3	1.8
65–74..........	54.2	60.7	6.5
75-up..........	47.8	53.7	5.9

SOURCES: 1950—U.S. Bureau of the Census, *United States Census of Population: 1950*, Vol. II, Tables 38 and 107.

1951—U.S. Bureau of the Census, *Current Population Reports*, Series P-20, No. 75; Series P-25, No. 212.

This increase in "non-normal" families suggests that one possible explanation of the change in headship rates was the break up of customary family groupings brought about by the impact of the war and cold war on tastes, mobility, and employment opportunities. The almost universal service experience helped young people acquire a taste for independence, and the existence of sufficient job opportunities (at least in the first half of the fifties) made it possible to make this taste effective.

Also contributing to the increase in headship rates after World War II were the increase in survival rates at the late life-cycle stages and the factors (such as the quality backlog and the shift to sales housing) tending to increase housing starts at the expense of increased vacancies.[8] The latter encouraged household formation by providing ready availability of separate quarters as real incomes rose.

Finally, it is problematic whether any of the foregoing developments would have resulted in increased headship rates without the large increase in per capita income in the last twenty years. In this context, the fact that growing real output could raise per capita real incomes without leading to a burst of immigration may help explain why headship rates increased so much more in the fifties than in earlier boom periods.

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[8] The increasing number of people receiving (Social Security) retirement benefits also may have been important in increasing headship rates at the late life-cycle stages.

VI

The question that comes to mind after studying the experience of the postwar years is: Can housing starts and household formations continue to move independently of the underlying population pressure for new dwelling units? Of course, a definite answer to this question cannot be given, but we can suggest some of the more important possibilities.

For example, birth rates have been decreasing in recent years and, even on the basis of the most favorable estimates, the war-induced quality backlog is about gone. Similarly, it seems likely that the once-and-for-all impact of past social legislation on housing demand has about run its course. Only the probability of a continued high level of internal mobility and the possibility of increased real income seems favorable to a continued excess of housing starts over household formations.

More important, it is very unlikely that headship rates can continue to increase in the future as they have in the recent past even given continued internal mobility and increasing real income.

With vacant units increasingly available and real incomes remaining at high levels for some time now, it seems reasonable to suggest that most of the increase in headship rates likely to result from improved economic conditions has already taken place. The demand for separate quarters is almost certainly discontinuous with respect to both income and price, and, until incomes reach the two dwelling unit level, no further increase in demand seems likely from this source. In fact, in several age classes the headship rate was near the maximum level possible at the end of the fifties. If any potential for significant increases in headship rates exists, it is probably to be found among the minority groups currently crowded into urban and rural slums.[9] Presumably, only government action can tap this source of demand for separate quarters.

If the very rapid growth in the number of people at the early life-cycle stages in the sixties runs into less rapidly growing job opportunities and incomes, it is even possible that headship rates may fall rather than increase in the near future. These are the life-cycle stages where headship rates increased most rapidly between 1950 and 1959 and where a high proportion of non-normal families make wide shifts in headship rates possible.

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[9] The relatively high level of unemployment in recent years may have held back some household formations. However, there is a considerable overlap between the unemployed and the minority groups discussed in the text. Thus, reducing unemployment will not raise headship rates as much as one might expect unless the social problem of minority housing is also solved.

There is some inconclusive evidence that the past relation between housing starts and required additions is returning. Briefly, the growth rate of both housing starts and household formations declined in the last few years, and housing starts declined relative to household formations. Further, the number of normal households formed dropped in the last five years and the increase in headship rates between 1955 and 1960 was less than between 1950 and 1955. Perhaps even more important, the annual rate of increase in the occupied housing stock not only fell between 1950-56 and 1956-60, but this drop was accompanied by a significant increase in vacancies.

VII

Together, the foregoing considerations lead us to the conclusion that housing starts and household formations will follow required additions again in the near future.

However, if the evidence of a current slow down in residential construction is accepted, it is clear that the slow down is not part of the prewar pattern of long swings. Rather it signifies a move toward the limits established on residential building by basic long-run forces. If housing starts and residential construction were following the long swing in required additions, they would now be increasing more rapidly than they did in the fifties. It seems clear that episodic factors helped us to at least defer one entire contraction phase of the residential building cycle.

The fact that the experience of the fifties constitutes a divergence from the past family of long swings does not mean the residential building cycle will cease to exist, nor does it mean that the prewar pattern of long swings will reassert itself in the future. While there is evidence that residential construction and household formations are moving back toward required additions, the long swing in required additions in the future will be different than it was in the past. By their impact on birth rates, depression and war have permanently altered the population base for the long swing in required additions.

In the future there will be three main elements to the swing in population growth and thus in required additions. First, there will be the vestiges of past waves of immigration (affecting demand in the over-45 age class in the sixties). Second, there will be the large decrease (to negative levels) in required additions resulting from the decline in birth rates between 1925 and 1940 (reaching the 30-45 age class in the sixties). Third, and dominating the change in total required additions, there will be the effect of the increase in birth rates in the forties and early fifties (reaching the 15-29 age class in the sixties).

Under the impetus of these diverse sources of change, required additions will increase by increasing amounts through the first half of the seventies. Most of this increase will take place at the early life-cycle stages. More additional dwelling units will be needed for the 15-34 age class alone in the seventies than were required for all age classes in any prior decade except the forties. However, required additions will increase at a slower pace in the last half of the seventies and then stop increasing or even decline in the eighties.

Taking this chronology any further would involve us in the thankless task of estimating future birth rates. However, it is not necessary to project birth rates to outline the main characteristics of the long swing of the next twenty years. Moreover, if birth rates do not vary much, then this pattern will be repeated.

The new long wave in required additions will reflect the swing in birth rates beginning in the late twenties rather than the shorter swing in immigration as in the past. The result will be a long swing of longer duration (20 to 30 years rather than 15 to 20 years), characterized by extremely wide shifts in age composition, and showing up primarily in the rate of change of required additions rather than in the level of required additions.

VIII

In summary, for the seventy years prior to the fifties, long swings in residential construction and household formations followed the path traced by changes in the size and age composition of the population. Then in the last decade this well-established pattern was broken. Residential construction and household formations increased to record levels as required additions fell.

Our investigation of the sources of this divergence led to the tentative conclusion that the experience of the fifties was largely the result of special factors most of which are not likely to be repeated in the future. Instead we expect that long swings in required additions will once again be reflected in similar swings in household formations and housing starts.

However, we found that the same conditions that led to the residential construction boom in the postwar period also changed the nature of the long swing in population pressure for additions to the housing stock. Thus, depression and war not only had an episodic effect on housing markets, but through their impact on birth rates, they will have a long-run effect.

Given the importance of population change and household formations for all sectors of the economy, it is reasonable to suppose that the emergence of the new long swing in required additions may mark a break in the historical pattern of long swings in economic growth (as well as in residential construction)—with effects not yet foreseen.

In closing, we might note that, if we are correct and the past relation between required additions and housing starts is restored in the sixties, the result will be a considerable decline in residential construction. This decline would occur despite the increase in required additions to record levels in the sixties (as a quick glance at Chart I reveals). Certainly, there is little room for optimism about an increase in housing starts in the present decade.

DOLLAR SCARCITY AND SURPLUS IN HISTORICAL PERSPECTIVE

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Introduction

The theme of this paper is that current problems with our balance of payments can be more intelligently understood when compared with past American experience with the long swing. The suggestion that conditions in our balance of payments since about 1950 and over periods longer than the business cycle have been very closely related to the general pace of American postwar development is certainly not original to me. Nevertheless, there has not been, to my satisfaction, a serious attempt to review our recent balance-of-payments history in the light of our knowledge of U.S. growth variations during the ten or fifteen years since the war.

Perhaps it would be wise at the outset to make clear just what part of our postwar experience we shall attempt to explain. First, this paper does not consider the effects of the short-run business cycle on the balance of payments. But at the same time, this brief excursion into our balance-of-payments history does not consider the possibility of this country's involvement in a fundamental secular disequilibrium. Between the years 1946 and 1961, the U.S. lost something in the neighborhood of $2.7 billion worth of gold; from 1950 to 1961 the figure is close to $7.2 billion. The size of the secular deficit is even more impressive, of course, when it is measured in terms of gold flow and increase in liquid dollar holdings abroad. What we are primarily concerned with here is the sharp, long-term movement which has occurred around that average deficit level since the late forties, but especially since 1950.

As the title of this paper suggests, it is my own feeling that American postwar experience with surplus and deficit may be much less unique than most economists imply. I would be the first to admit, however, that the most available portion of our history—the two interwar decades—does not appear to be very helpful in offering historical precedent for an understanding of contemporary dollar problems. One has to glance further back in history, into the pre-1913 era, to find significant similarities between past and present.

Pre-1913 Experience

Abramovitz, Burns, Easterlin, Kuznets, and others have shown us that United States growth did not occur without serious interruption. It seems rather that our experience is best described as occurring in waves, each dynamic crest of which was followed by an equally stagnant, but shorter, period of depression, unemployment, and price stability or deflation. These Kuznets cycles or long swings in American development, though different in character and milder in nature, seem to have persisted even through the last fifteen years of our history.

Investigation into the long swing has thus far been limited chiefly to its domestic aspects. (There have been two notable exceptions: the work of Douglass North and Brinley Thomas.) Yet at least until World War I, the long swing had a reasonably consistent and impressive impact upon our balance of payments. Prior to the first World War and during a period of so-called nineteenth-century "normalcy," decades of rapid domestic growth and accompanying inflationary pressures were generally ones of increasing balance-of-payments surplus. Because of the excessive variations in the rate of net capital movements,[1] which we will discuss below, rapid growth and accompanying relative price inflation generated dollar shortage; periods of secular stagnation and sluggish growth tended to generate dollar surplus and severe gold drains. The public reaction to these periods of severe dollar crisis strikes a familiar contemporary chord. In the middle of the nineties, as in the late fifties, another great debate on our balance of payments began. This was the era in which Bryan made his eloquent reference to the "cross of gold" and the Private Exchange Pool Proposal was outlined to deal with the hot money movement (Simon, *Journal of Economic History*, March, 1959, pages 31-50). If you will recall, most of that decade, following the rapid growth of the eighties, was also characterized by relative stagnation, high unemployment, and excess capacity; as Abramovitz dates it, it included the trough of a long swing. It was also, to repeat, a period of serious deficit and gold outflow.

What were the underlying component movements in the balance of payments which tended to produce alternations between dollar surplus and deficit? Over the nineteenth and early twentieth centuries, long swings in merchandise imports are apparent in the raw data; these long-run variations reveal, not surprisingly, high positive correlations with the levels of income, output, and investment but with a small lag. Leaving aside exports for the moment, the long swing in import demands created similar patterns in our trade balance and current account. During periods of rapid growth on the upswing of a Kuznets cycle in internal development, the trade balance became progressively worse, reflecting a tendency towards increasing excess demands in the domestic market. Generally the trade balance and current account lagged considerably behind domestic growth rates: peak deficits in the trade balance did not occur on the average until five years after retardation had set in. These periods of progressive deterioration in the trade balance alternated with periods of progressive improvement, lagging by about four years, when our growth was sluggish and the economy was seriously involved with periods of severe recessions, unimpressive recovery, and factor unemployment. The trade balance improved, as the simplest trade models predict, simply because income and prices were relatively low or growing at a low rate.

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[1] As asserted below, rapid growth tended to generate dollar shortage or payments surpluses due to the enormous response of capital movements to the growth. Rapid growth did not generate surplus via the trade balance and thus not by, as Kindleberger calls it, Abramovitz-Solow growth of the technological sort (Kindleberger, *Lloyds Bank Review*, July, 1962). If it exists, much of our present long-run dollar problem may be due to low Abramovitz-Solow growth. Nevertheless, it is argued here that the decade variations in our postwar payments position, apart from its longer run level, is due primarily to the relationship between capital flows and economic growth.

What about the role of exports in our nineteenth-century balance of payments? Mintz has shown us that exports always have had a very complex relationship with our business cycle. This has been true, too, of the long swing. In no portion of our pre-1913 history has the pattern of aggregate exports interfered with the dominance of aggregate imports so as to make increasing trade balance deficits typical of periods of rapid growth and surplus of slow growth. During some stages of our development (approximately 1820-60), exports moved in a positive relation with our growth over the long swing; during other periods (approximately 1860-80), exports moved independently of the domestic long swing; but during the four decades prior to the twenties, the relationship was, if anything, inverse, with periods of laggard growth being ones of rapid export expansion. In spite of their inconsistent pattern prior to World War I, however, any unco-operative movement in aggregate exports over the long swing was swamped by the pattern of imports.

If an increasingly large trade balance surplus was coincident with domestic retardation and eventually temporary stagnation, how do we explain the tendency towards increasing payments deficits during these same periods? The answer, of course, is that alternations in the trade balance were actually overfinanced by similar but more striking fluctuations in net capital imports. Nurkse and others have pointed out that the nineteenth century did not know of a chronic sterling shortage due to the British willingness to export capital in increasing amounts. But this condition did not eliminate the tendency towards recurring dollar surplus and sterling shortage in the U.S. Extensive periods of rapid development were accompanied by increasing relative rates of return on American securities; thus, increasing trade balance deficits were accompanied (not always with synchronism) by sharply rising net capital imports (or sharply declining net capital exports). Furthermore, the fluctuations in net capital imports were only negligibly offset by changes in the flow of payments on that accumulating debt. In this way, potential balance-of-payments deficits generated by periods of rapid growth were transformed into actual payments surpluses by an excessive outflow of securities which were earning increasingly higher relative returns.

TABLE 1

COMPARATIVE DATING OF ECONOMIC ACTIVITY AND SOME BALANCE-OF-PAYMENTS COMPONENTS

Economic Activity or GNP in % Change (1)		Deflated Imports in % Change (2)	Net Capital Imports in Absolute Levels (3)	Trade Balance in Absolute Levels (4)	
Peak	1834	1833	1837	Trough	1837
Trough	1840	1842	1842	Peak	1842
Peak	1846	1846	1852	Trough	1855
Trough	1858	1860	1858	Peak	1860
Peak	1864.25	1865	1871	Trough	1871
Trough	1874.25	1876	1879	Peak	1879
Peak	1881	1881	—	Trough	—
Trough	1886.5	1885	—	Peak	—
Peak	1889.75	1888	1888	Trough	1888
Trough	1892.25	1896	1900	Peak	1900
Peak	1899	1901	1911	Trough	1908
Trough	1911	1913			

SOURCE: Column (1), M. Abramovitz, *Statement in Hearings before the Joint Economic Committee of the Congress of the United States*, 1959, 86th Cong., 1st Sess., Part 2, Tables 9 and 16, pages 411 and 466; Columns (2)–(4) from the author's own research. Abramovitz's series is adjusted for business cycle movements; the balance-of-payments figures are not.

So much for a brief description and interpretation of past payments experience. The question immediately arises as to the relevance of our pre-1913 experience to contemporary problems. Do these distinctive long swings persist in the recent two decades? Perhaps it would be wise to discard immediately such emotive terms as long swings and Kuznets cycles and talk less pretentiously in terms of variations in growth rates however explained; surely significant long-run variations in growth rates have occurred in the postwar era though certainly milder than those of the past. But there are other more serious questions. Has the period since 1950 been one of "normalcy" similar to the placid atmosphere of the nineteenth-century trade? Can we appeal to the all-evasive term normalcy to explain away the messy balance-of-payments history of the interwar period? Are imports of a composition today that make them respond differently to contemporary domestic conditions? Is the trade balance now tied to capital movements? This is just a sample of the questions which come to mind, but the issue is clearly whether there is enough similarity between past and present environments to expect similar patterns in our contemporary balance of payments.

Postwar Development in Our Balance of Payments

Beginning with 1950, the U.S. deficit declined steadily. This trend of improvement in our payments position, although it was not great and suffered interruption, persisted until the mid-fifties. In the autumn of 1956 and the greater part of 1957, the U.S. actually found itself enjoying a surplus. At this point there is a sharp line of demarcation: towards the end of 1957 we moved back into a deficit position which became worse and has remained at alarming levels until the present. If we regard the period from late 1956 to 1958 as an extraordinary combination of circumstances, perhaps it may be more accurate to say that long-run pressures to increase the size of the U.S. deficit were apparent as early as 1956. The abrupt transition from surplus in 1957 to tremendous deficit in 1958 would seem to support this view.

Thus far the description of the movements in our payments position has covered the period beginning 1950. Is this an arbitrary delineation? Suppose we set our task to include an explanation of the changes in our payments position since 1946 or 1947? At first blush our conclusions would seem to be somewhat different: the payments position moves secularly—despite the interruptions of 1950 and 1957—from a surplus position in the late forties to an increasingly large deficit in the late fifties and early sixties. It is my own opinion that the pattern from 1945 to 1950 is explained by the exogenous effects of reconstruction and the overwhelming importance of government transactions in the balance of payments. The more systematic and endogenous pressures, which this paper attempts to identify, existed during this period in the private account but were overshadowed by the movements in the public account. It is only with the beginning of the fifties that systematic effects make themselves felt. Thus the discussion of the effects of American growth on the net payments position begins with 1950, while investigation of the payments components may cover the earlier period.

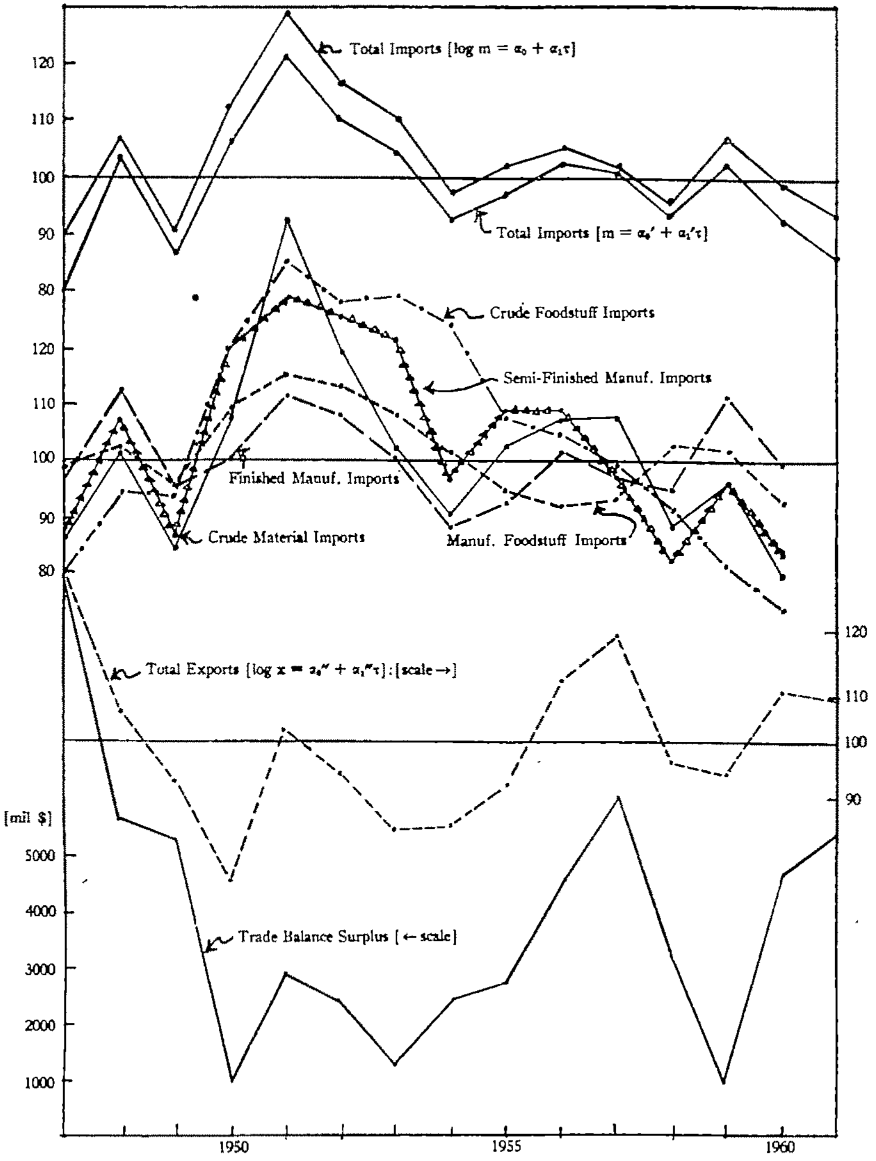
To idealize and generalize the patterns then, the period of rapid U.S. growth from 1950 to the mid-fifties was a period of improvement in the U.S. balance of payments (although it was not one of consistent surplus). Serious balance-of-payments problems only begin to arise in the late fifties with the inauguration of an extended period of sluggish development in the American economy; this relationship seems to be interrupted only temporarily by the unusual international circumstances of late 1956 and 1957.

Thus far we have only the loose correlation between the net payments position and American growth performance. The trade balance, however, also seems to have a pattern similar to those of the pre-1913 era. During a period of impressive American growth, the trade balance surplus shrank from a high level in 1947 to a very small surplus in 1953. Now surely a good part of the severe deterioration in our export surplus during the late forties was due to the abrupt contraction of American assistance in postwar reconstruction. (One may contest more strongly that the deterioration in our export surplus reflects the devaluation of 1949.) Nonetheless, the trade balance continued to shrink until 1954 and remained at low levels up to 1955. If we again disregard the unusual nature of 1957 and 1958 (inflationary boom, material shortages abroad and the Suez crisis in 1957, bad harvests in Europe in 1956, and a reversal of these conditions in 1958) as well as 1959 (the steel strike and the invasion of the "compacts"), then the trend has been towards significant improvement in our trade balance during the seven years, 1956-62. This again is consistent with American history and with our elementary notions of the effect of domestically generated income movements (of the Harrod-Domar rather than Abramovitz-Solow type) on the trade balance; rapid growth of money income was coincident with a reduction in the trade balance surplus and relative stagnation with an improvement in our trade balance position.

But is it simple as that? Are the causes of the trade balance fluctuations in the postwar period common to those of the pre-1913 era? As we have seen, in the nineteenth century, exports never seriously contributed to the pattern of long swings in the trade balance and current account. Variations in import demands precipitated by income and price movements were only negligibly affected by exogenous shocks and by the complex effects of systematic productivity changes on import substitutes and exports. Is it primarily the dominating pattern of imports which produces this configuration in the contemporary trade balance surplus?

Although the nature of the current domestic long swing has been much milder than those of the past, it still seems clear that aggregate imports have not revealed very striking fluctuations over recent patterns in our growth (Chart I). It is true that aggregate imports rise from low immediate postwar levels to a significant peak in 1951 and then fall consistently into the sixties. But surely a large portion of the boom culminating in 1951 is due to the inflation of primary product

CHART I



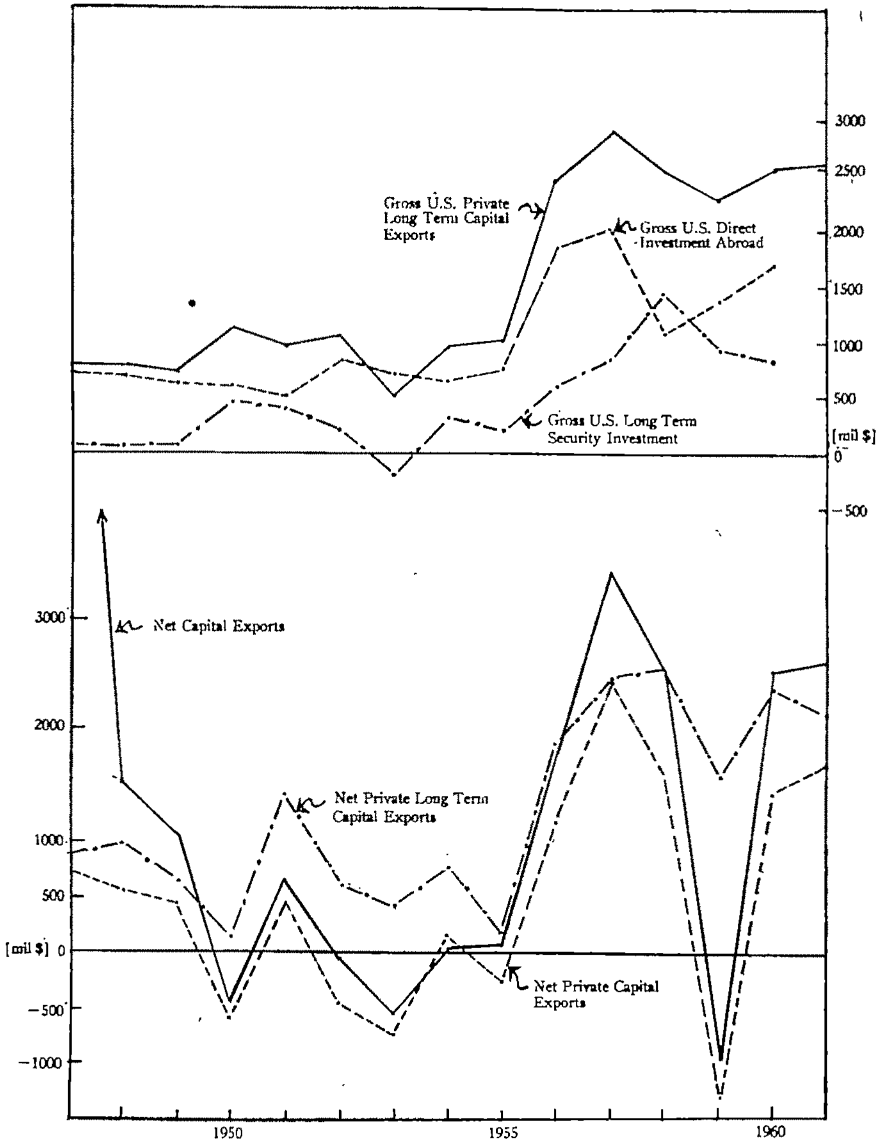
prices during the Korean conflict (and the 1949 devaluation)? Indeed, when aggregate imports are deflated by the Department of Commerce import price index, then very little of the long swing in imports remains even in the trendless data. Does this suggest that imports reveal a relationship with postwar American growth performance inconsistent with the past? Perhaps so, but an interesting complication arises when imports are disaggregated into five commodity groupings. Chart I reveals that four of these groupings reflect long-swing patterns. Crude materials, crude foodstuffs, semimanufactures, and, to a lesser extent, manufactured foodstuffs—all show a pattern of increasing imports up to 1951, remaining at high levels until 1953, and then declining continuously up to 1960. The nonconformist is finished manufactures, which apparently has been an important enough bundle to minimize the long-swing pattern in aggregate import values. In the pre-1913 era, these five components tended to move together rather consistently.

Although import values (trendless) reveal a positive correlation with the trade balance surplus, aggregate export patterns seem to play a far greater role in contributing to the long-run pattern in the trade balance than historically. With their trend removed, exports decline from high levels in 1947 to 1953-55. Following that low, they increase to, and remain at, high levels in the late fifties and early sixties (except for the interruption of 1958-59).

At this point, let me summarize by indicating that it is not the export pattern which lacks historical precedents (e.g., the period 1880-1915), but that the apparent importance of export movements in helping produce a trade balance configuration consistent with history is unusual. As far as the export pattern is concerned, we might be able to fit that experience, *ex post,* into the framework of American postwar growth. First, it is quite possible indeed that domestic price inflation has had a significant effect upon the pattern of exports during the postwar period; these price movements have not been entirely independent of the rate of U.S. growth. Second, the export pattern may simply reflect our "capacity" to export; periods of excess capacity and sluggish growth in domestic demand may be ones in which foreign demand can compete successfully for the U.S. products. It is my own feeling that the long-term movement in U.S. exports during the three or four decades prior to the first World War can in good measure be explained by the combination of these two factors (prior to 1880, exports may have been the initiating factor of the long swing, especially pre-Civil War). Third, the pattern of exports may simply reflect American willingness to export private and public capital abroad. The prime historical example of such a balance-of-payments mechanism is nineteenth-century Great Britain: to grossly overgeneralize, nineteenth-century America facilitated the real transfer over the long swing by variations in import demands; England facilitated the real transfer by variations in aggregate exports.

Given that the trade balance deteriorates during periods of rapid growth and improves during periods of poor growth performance, how do we explain the opposite pattern in our net payments position? In

CHART II



my own view, the answer to this question clearly lies primarily in the private capital account. Net capital exports fell sharply and with few interruptions from extremely high levels in 1947 to 1953-55 (Chart II). From the mid-fifties to 1961, net capital exports increased sharply and fairly consistently except for the interruption in 1959 (again with a lag behind retardation in domestic growth). But public capital movements, although they may explain a large part of our export patterns especially in the late forties and early fifties, do not explain the pattern of total U.S. capital exports. Net private capital exports fell from a high in 1946 to a net import position in 1953; they rose abruptly after 1955 and remained at those high levels until 1961 (again, with the exception of 1959). Except for the years 1946 and 1947, it is private capital which dominates the pattern in net capital account. Years of rapid U.S. growth are ones which apparently produce successively smaller rates of net capital export; periods of sluggish growth and apparent low relative returns on capital are ones of increasing capital export. So significant are these fluctuations that they overshadow changes in the trade balance and tend to produce payments deficits during years of generally unimpressive growth.

Furthermore, this time pattern in our net private capital account is not explained simply by short-term capital movements. The net long-term private capital position shows stability or even a slight but steady decline in the rate of export up to 1955; after that point it reveals the same pattern as the net capital position but that 1959 is still a year of significant long-term capital export. My own position would be that the long-run pattern in our net payments position during the fifties and sixties can be explained by relative profit opportunities and returns on securities which, though difficult to quantify, should have a significant positive correlation with the domestic income-output movements. Furthermore, one must agree that the portion of short-term capital, a part of the deficit itself, which represents speculation against the dollar is small compared with the bulk of American capital that went and is going abroad in search of higher rates of return.

Direct investment did not play an important role in the American capital position prior to World War I. But the fact that it does today does not seem to obliterate the tendency for net private capital movements to be "sensitive" to long-run domestic conditions. Certainly part of the extremely sharp movements in net capital exports from 1953-55 to 1957 can be explained by the tremendous increase in direct investment in petroleum. Nevertheless, the striking increase in net capital exports persists even after discounting for the abrupt rise in direct petroleum investment during these years. As a matter of fact, long-term investment in securities exhibits a pattern even more suggestive of a close correlation with American growth. Beginning with 1950, net long-term investment in securities gradually declines to a net capital import position in 1953; then expands continuously to a high level of net capital export in the years 1958-60.

It seems to me that the pattern of our net capital account cannot be dismissed by an appeal to such explanations as the gradual process of returning to the normalcy of the twenties and earlier. I find it difficult to attribute the sharp rise of net capital exports from the mid-fifties to the early sixties as simply a lagged readjustment of the international community to a renewed period of relative international stability. Nor can I find a great deal of sympathy for the view that the pattern of net long-term private investment is simply the reflection of such exogenous accidents as the creation of a Common Market in Europe. The pattern of American long-term investment abroad applies not only to European but to Canadian and Latin-American investment as well.

Conclusion

In a paper as brief as this one and one which attempts to treat a problem as large and as complicated as our contemporary balance of payments, the discussion is doomed to be cursory. My comments have been, therefore, directed in the spirit of offering suggestive hypothesis rather than substantive proof. I do feel a necessary compulsion to make a projection into the future. Not only is more rapid growth a domestic policy goal, but if we should begin to achieve it in the near future and if our interpretation of contemporary payments problems is approximately correct, our net payments problem is certainly likely to diminish. Whether or not our payments deficit will disappear is another question entirely, but it is conceivable that once again in the sixties we will find the literature flooded with articles on causes and cures of the new dollar shortage.

DISCUSSION

EDWARD F. DENISON: Use of earlier long-swing patterns to illuminate the present requires extraordinary ingenuity and perspicacity because the current long swing in GNP is so different. According to Abramovitz it has already lasted far longer than previous swings—almost thirty-three years and the downward phase alone twenty-three years. Moreover, it is difficult even to think in a framework that considers all of the varied events since January, 1939, as having occurred within any kind of downswing.

Further, changes in intensity of resource utilization since 1930 have been much greater, relative to changes in the growth rate of productive potential, than in past long swings, but the difference has not been measured. This makes analogies with past swings hazardous. Long-swing analysis stresses interrelationships between utilization and growth of productive potential, but the two have not been statistically disentangled because data and technique to construct series for both actual and potential GNP for the past century are lacking.

Finally, recent cataclysmic events and institutional changes have fundamentally altered the timing and amplitude of long swings in most variables.

These differences confine applicability to the present of research into past long swings to lines of causation disclosed among output, demographic variables, capital formation, and so on.

Williamson rejects the Abramovitz twenty-three year downswing and characterizes the first half of the fifties as a rapid growth period and the subsequent years as a slow growth period. He treats these as comparable, for his purpose, to similar phases in pre-1914 long cycles. This classification is crucial to the force of his argument, since, without postwar confirmation, patterns in evidence before 1914 might seem to have little present relevance. His timing seems to me legitimate, but this is clearly debatable.

Williamson finds that before World War I and after World War II rapid growth of domestic output brought an improving balance of payments because capital movements more than offset deterioration in the trade balance. He concludes that more rapid domestic growth in the future would probably improve the balance of payments for the same reason.

Williamson's postwar analysis rests on somewhat delicate abstraction from the effects of exogenous factors on the balance of payments. Relevance for the present of the nineteenth century may be questioned for reasons the author notes, and so may omission of the interwar period. And historical analogy is only one way of appraising the future effects of rapid growth. Still, Williamson raises a substantial doubt of the common view that more rapid domestic growth would harm the balance of payments because deterioration in current transactions would exceed any favorable effect on capital flows. If, as Williamson believes, this dilemma does not exist, it is obviously a discovery of first-rate importance for domestic economic policy.

It is true that most discussions stress movements over a single business cycle, whereas Williamson is concerned with longer periods; but surely Williamson's longer period focus is correct. The purposes of reserves surely include cushioning strictly cyclical changes.

Since the view that with faster growth current account deterioration will exceed capital account improvement is not impressively documented, intensive research on the question is clearly indicated. Rarely is a single point of analytical disagreement both so clearly formulated and so important for policy.

The difficulty with Campbell's application of long-swing analysis to prospective housing starts is that "required additions," from which past long swings stemmed, have not moved with starts since World War II or, if one considers amplitude of fluctuations, even since 1920. Thus the long-wave background gets Campbell no farther than others who have based forecasts of housing starts on the normal increase in households that would result from population changes in relevant age-sex groups, which Campbell formalizes in his "required additions."

Starting at least with S. Morris Livingston's fine article in March, 1950, efforts have been made to obtain a starting point for projections by statistical reconciliation of housing starts with normal household formation. Enormous data problems hamper such reconciliations, and projection of the reconciliation items themselves has not yet been very successful. Livingston, for example, concluded there was a basis for starts to continue high only through 1952, unless a large replacement demand could be created. By now it seems clear that comparison of starts with normal household formation tells little about when starts may be expected to drop, and Campbell gives little reason to pick the remaining sixties rather than 1955 or 1975. Moreover, doubts are growing that any previous relationship with required additions will ever be restored, and Campbell does not indicate what would be a sustainable relationship in future.

This general type of investigation, though incomplete, is nonetheless useful. But this paper does not seem to me a step forward because, unlike Livingston and some other investigators, Campbell gives no estimates of the individual reconciliation items that would permit quantitative thinking about the future based on their individual size.

Hickman's admirable contribution compresses a remarkable amount of information and analysis. Despite his rather disconcerting demonstration that available explanations of the basic long-swing relationships are incomplete or otherwise unsatisfactory, he appears convinced that the relationships themselves, as modified by recent events, continue important. I shall make four observations:

1. Hickman, like Abramovitz, dates the last peak around 1939, although his Charts I and II show an apparent peak around 1950-51. If this is rejected as an aberration caused by the Korean war, one wonders why the two World Wars were not also discarded. It is of interest that Hickman's conclusions about the present do not seem to depend at all on whether the last peak was twelve or twenty-four years ago.

2. In evaluating changes in economic structure to determine how they have altered the importance for the economy as a whole of long swings in particular sectors, Hickman relies on constant dollar estimates with a fixed base. Data in current dollars appear more appropriate for this evaluation. Expenditures in the prices of each period measure the distribution of resources among types of activity in that period. The impact of a drop in real GNP in the twenties is indicated by its percentage drop in prices of the twenties; in the sixties by the percentage drop in prices of the sixties. Suppose a 10 per cent drop occurs now in a component that accounts for 6 per cent of GNP in current prices in both 1929 and 1962 but, because its relative price has doubled, for only 3 per cent in 1962 when measured in 1929 prices. The effect on employment, the welfare cost of the lost output, and the multiplier effect are all the same as a 10 per cent drop in 1929, not half as large. For Hickman's purpose, not only GNP components as such but also the aggregate capital-output ratio and the marginal propensity to consume must be stated in the prices of each period, not in the same prices in all periods.

Use of current prices would cut the drops shown in the ratio of investment components to GNP and in the capital-output ratio, and swell the increases in the ratios of government expenditure components to GNP.

3. Hickman's message emerges clearly despite his cautious wording. The downphase of the long cycle culminates in major depression or stagnant growth because it creates an insufficiency of aggregate demand and hence underutilization of resources. But there is no need to wait for endogenous forces to reverse this phase of the long swing. Federal tax cuts or expenditure increases offer an appropriate mechanism to correct the situation. It is reassuring that careful review of long-swing relationships raises no new doubts about the efficacy of fiscal policy.

4. Suppose fiscal policy fully prevented long swings in the ratio of actual to potential output. Changing rates of growth of capital stock would still introduce long-wave-type fluctuations in the growth rate of potential itself, for the share of investment in GNP would not be stabilized. But long waves from this source would be minor. Changes in the growth rate of the labor force have a much larger effect on potential output. But if these had long-wave characteristics they would not be those of the past because of immigration restrictions and new age patterns. Long-wave analysis tells little about future labor force behavior not shown directly by projections based on the age distribution of the population, school attendance, the retirement age, and so on. Whether long waves have important implications for the growth of potential output over the next two decades depends on whether we use economic policy to keep actual output near potential and, if we do not, the effect of underutilization on potential.

Since, according to Hickman, endogenous forces creating long waves themselves result from past fluctuations in the ratio of actual to potential output, elimination of these fluctuations might eventually eliminate long waves. But this is indeed looking far ahead.

account outweighs the loss on current account to be expected from soaring demand for imports and reduced incentive to export. This hypothesis, with its pleasant policy implications, is supported by Professor Williamson's original and stimulating paper which suggests that the balance-of-payments deficit can be reduced, if not eliminated, by encouraging the growth of the home economy. During the nineteenth century and until World War I, Williamson finds "rapid growth and accompanying price inflation generated dollar shortage; periods of secular stagnation and sluggish growth tended to generate dollar surplus." This experience, he believes, has been repeated during ten or fifteen years since World War II, though not in the "messy" interwar period.

Professor Williamson regards his study as offering suggestive hypothesis rather than substantive proof. Yet others will build on his findings, which therefore deserve careful examination. For brevity's sake, however, I shall have to omit comments on many issues raised by the paper, such as the long-swing chronology for the fifties; the assumption that profits and long-term capital flows move with growth cycles; or the roles of prices, costs, and interest rates.

My first question concerns the general plausibility of swings in the balance of payments being caused by variations in growth rates. Net current transactions move inversely to net capital flows during long cycles, as Williamson affirms. Hence improvement or deterioration of the balance of payments depends on whether the change in the trade and services balance is larger or smaller than the one in capital accounts. This points to the great difficulties involved in predicting the movements of a balance of payments. One deals, in MacDougall's words, with "the marginal part of a marginal part." The components of the balance are subject to numerous influences and also depend partly on one another, as when a rise in merchandise exports is caused by a rise in capital exports. Thus one would hardly suppose that the net change in all goods and service transactions produced by variations in growth rates would turn out to be in a consistent relation (i.e., always smaller) to the change in net capital flows.

If we do find correlation between growth rates and the balance of payments, it may appear more plausible to interpret it in a different fashion. Does it not indicate the effect of the balance on the growth rate rather than the other way around? The smallness of the balance of payments does not prevent it from affecting the course of the economy, as we know. If loss of reserves hampers business expansion, as it did under the gold standard and does today, this may explain coincidence of low growth rates and unfavorable balances. Causation may, of course, run both ways. An unfavorable balance may restrict growth, and this in turn may cause the balance to deteriorate still further. But the presence of such a cumulative process (the opposite of the tendency toward equilibrium assumed in traditional theory) would have to be established, and this requires much further study. The present paper does not deal with this problem.

The actual behavior of the balance casts further doubt on Williamson's interpretation. Neither in the nineties nor in the late fifties was there a progressive decline which would agree with the notion of cycles. Instead, and in contrast to its components, the balance exhibits a sudden shift from one level to another. This suggests an explanation through similar shifts rather than gradual change in causal forces.

The next question I want to raise concerns the role of swings in the economies of other countries. In the present paper Professor Williamson does not refer to his earlier essay on the relation between British and American long cycles, but treats the outside world as an exogenous factor of minor importance. It is not clear, however, why swings abroad should not affect international transactions at least as much as domestic ones. When foreign expansion coincides with domestic contraction, it will reinforce the latter's effects. It is difficult in such cases to distinguish to what extent an improvement in the trade balance or a rise in capital outflow is due to the domestic downswing rather than to the foreign upswing. When, however, foreign and domestic cycles are synchronous, the two forces oppose each other, and a close relation between U.S. growth rates and balance changes is unlikely. My own investigations reveal the dominant influence of foreign cycles on U.S. exports, the largest component of the U.S. balance of payments. The rise and fall of exports always follows the course of world trade, while the inverse effect of cycles in the domestic economy on exports is in general of decidedly second-rate importance. This is different for imports, of course; yet the positive correlation of the trade balance to world imports is higher than its inverse correlation to domestic business. I have not studied capital flows but would expect a similar situation. Therefore, I would interpret an event like the recent vigorous rise in U.S. exports of commodities and capital as due, apart from other factors, primarily to rapid growth abroad and only to a minor extent to slow growth at home.

Moving on to more specific points, let us inspect the evidence for the current period. Williamson compares the slow growth period, 1956-62, with the rapid growth period, 1950-55. He finds that in the latter the surplus on current account as well as capital exports are higher than in the former, which accords with his theory. The theory requires also, however, that the improvement on current account be less than the rise in the capital outflow, while inspection of the charts gives the opposite impression; namely, that the trade balance rises more, not less, than capital exports. A table in a forthcoming study by Hal B. Lary confirms this. It shows that the deficit on basic transactions—the sum of the current and long-term capital accounts—averaged about $1.8 billion from 1950 to 1955 and fell to about $1.5 billion on the average for 1956 to 1961 exclusive of 1959, a year that is also excluded by Williamson as an exception. The same result is obtained when the averages for 1950-55 are compared with 1960-61—years when the harmful effects of prolonged slow growth should be particularly pronounced. The improvement in the current account thus was larger than the offsetting increase in long-term capital flows, and it is only the inclusion of short-term capital export which raises the deficit of the later years above that of the earlier ones. But one may doubt that short-term capital went abroad "simply because of the relative improvement in profit opportunities"; i.e., due to slow growth. If the rise in the deficit is to be explained in this fashion, it needs to be shown that the outflow of short-term capital can really be attributed to the downswing.

As regards the period from 1879 to World War I, there is a single instance of a long downswing in the balance of payments (more correctly, a prolonged relatively low level). This is the period from 1888 to 1896, which also saw a rise in capital exports and in the current balance, in accordance with Williamson's thesis. The question is again whether all this can be explained by the slow growth of the American economy at that time. Williamson's table shows that the GNP growth rate fell in only two and a half of the eight years. Perhaps comparison to swings in growth rates is not appropriate here, but when I use, instead, Abramovitz' chronology of long cycles in the level of construction, I still find that activity rose in four of the eight years of unfavorable balance. Moreover, the balance decline precedes the fall in the GNP growth rate by two and that in construction by four years. In view of these worrisome timing discrepancies, one may be inclined to attribute capital outflow and deficit to the monetary uncertainty, financial crisis, and defaults of this period which deterred foreign lenders and caused capital flight. Williamson regards these disturbances as consequences rather than as causes of falling capital inflow, which he attributes to low profits due to slow growth. But, from the evidence presented, this interpretation is not entirely convincing.

My conclusion is that Professor Williamson's study has the great merit of challenging traditional views about balance-of-payments behavior. His evidence does not suffice to show, it seems to me, that slow growth causes a rising balance-of-payments deficit.

R. A. Gordon: There is an expanding body of evidence in favor of the view that growth in this country has gone through somewhat irregular swings of fifteen to twenty years in duration. This view is apparently shared by the authors of all three of these papers. They seem to be agreed (1) that it is useful to think of past economic growth (in a number of different dimensions) as having occurred in long swings and (2) that the disappointing growth of the American economy during the last half-dozen years or more can be interpreted as part of the downswing phase of such a long wave (although the postwar swing differs in significant respects from earlier ones). My ability to serve as a carping critic in this session is very much restricted by the fact that I agree with both of these propositions.

I am also inclined to accept the moderate and eclectic position that Hickman takes with respect to the nature of the dynamic process involved in these long swings. If I understand him correctly, his position can be summarized as follows. A dynamic process, subject to autonomous factors and characterized by long lags, is at work to create long swings in growth. The process, in very general terms, involves some such sequence as the following: a widening of innovational or other investment opportunities, accelerated growth in income and employment, the response of demographic factors (immigration, birth rate, household formation), the upswing of a building cycle, and eventual retardation—which in the past has usually been accompanied by a serious depression. The retardation in growth marking the declining phase of the long swing is likely to be due to some combination of the following: the results of a catching-up process for various types of investment leading to the substantial elimination of backlogs, a less rapid opening up of innovational investment opportunities, the downward phase of a building cycle (itself in part the result of a catching-up process operating with a long lag and in part a response to demographic reactions to the slowing-up process which has already begun elsewhere in the economy)—all this possibly magnified by monetary and financial reactions to the optimism bred by the previous rapid rate of growth. Wars and major changes in peacetime government expenditures can also play an important role.

I agree further that a process essentially of this sort has been in operation since World War II, although, as Hickman is careful to point out, the details of the dynamic mechanism have changed in significant ways since the twenties and particularly since the pre-1914 period. And as Campbell suggests, also, the mechanism of the building cycle has changed in significant respects.

It is not easy to date the long swing in which the United States presumably now finds itself. In terms of rates of growth in real GNP, Abramovitz marks a low point at the beginning of the thirties and a high at the end of that decade. It is possible to argue that we are still in the downswing of an unusually long wave, the previous low point of which was reached some thirty years ago. One might also perhaps argue that one complete long swing ended with the close of World War II, followed by another (very mild) swing in the rate of growth that reached its peak around 1950.

Without worrying too much about precise dating, what does seem to be clear is (1) that the rate of growth in output has been unsatisfactorily low since the short-cycle peak in 1953 and (2) that the expansion in private investment has been particularly unsatisfactory, especially during the last two short business cycles. Further, since the end of the Korean conflict, federal government expenditures have, except during the last couple of years, contributed significantly to the decline in the rate of growth of total output.

Let me comment briefly on some of the factors that seem to have been holding back the expansion of aggregate demand since the mid-fifties. Trouble really began after the short-cycle peak in 1953. Government expenditures (in real terms) ceased to rise and showed a net decline between 1953 and 1957. This slack was not fully taken up by the continued rise in private spending, despite the private investment boom of 1955-57. Real GNP increased at an annual rate of only 2.6 per cent between 1953 and 1957. After 1957, both private investment and government expenditures failed to provide the basis for a satisfactory rate of growth in aggregate demand.

In real terms, gross private investment exclusive of inventories failed to expand between 1957 and 1960—even between 1955 and 1960. Business expenditures on plant and equipment actually declined over the three-year period, and the net expansion in the last two years has been far from satisfactory. Even on a current-price basis, plant and equipment expenditures in

1962 are expected to be lower than in 1957 in manufacturing, mining, railroads, and public utilities. In the last case, a "catching-up process" had probably largely been completed. In manufacturing, the 1955-57 boom had clearly resulted in considerable excess capacity. Also, I suspect, a catching-up process was involved here in various ways.

Residential building has done little more than hold its own since 1955. I suggest that we have been in a very mild "downward phase" of a building cycle for half a decade or so—one which, fortunately, has been free of the extreme deflationary forces that frequently characterized long building downswings before World War II. Campbell provides a valuable analysis of the special factors that have been affecting residential building in the postwar years. He concludes that "episodic factors helped us to at least defer one entire contraction phase. . . ." I prefer to say that these factors lengthened out the swing in building and gave us (at least so far) a very mild downward phase.

Nonresidential construction has also moved along a more or less horizontal plateau since the mid-fifties. The biggest disappointment has been the behavior of producers' durables, which in real terms have not yet reattained the peak reached in 1956-57. Clearly, underlying forces have been tending to hold back private capital formation.

The effects of this relatively poor performance of private investment were aggravated by the behavior of government expenditures. Not until 1961 did total government expenditures, in real terms, reattain their 1953 level, the marked rise in state and local spending being offset by the sharp drop in federal expenditures from 1953 to 1955 and approximate stability thereafter. (Federal spending has, of course, risen substantially in the last two years.)

What all this adds up to is simply the proposition that nonconsumption expenditures have, since 1953 or so, been rising at a much slower rate than in the earlier postwar period—and at a rate slower than that needed to support what most of us would consider a satisfactory rate of expansion in aggregate demand. We have, in short, been experiencing a mild downswing of a Kuznets cycle. It is useful to think of it loosely in cyclical terms because the retardation is not independent of the previous more rapid rate of growth, and we can specify—at least in a general way—some of the lagged relationships that have been involved. Fortunately, it has been a downswing free of serious depression, without a collapse of building activity, and free also of serious deflationary forces stemming from monetary and financial difficulties. For this, in good part, we must thank the structural changes to which Hickman refers.

Can we look forward soon to an acceleration in growth marking the end of the current Kuznets downswing? Hickman places some hope in tax reductions and possibly increased federal spending. Campbell is pessimistic about prospects for residential building in the remainder of the sixties. Some express concern about the current boom in commercial—particularly office—building. Clearly a good deal will depend upon federal fiscal policy. A more expansionist monetary policy—if we can solve our balance-of-payments problems—would also help. Most, however, will probably depend on that complex of underlying forces about which we know so little—the state of private investment opportunities—which in turn reflect a wide range of factors from the degree of excess capacity through business psychology, wage-price relations, and monetary and fiscal factors, to technological change, demographic factors, and changes in the pattern of consumers' demand. Unfortunately, we do not know enough to be able to predict with any confidence the level and trend of private investment that will emerge from these influences during the decade ahead.

I shall end by venturing the guess that we are close to the end of a Kuznets swing—in the sense that the growth rate may cease declining—but that the modest acceleration that then will follow will leave a good deal to be desired. But this guess—as guarded as I have tried to make it—is based on such imponderables that it is hardly worth putting into words.

ECONOMIC TRENDS AND PROSPECTS IN THE U.S.S.R. AND EASTERN EUROPE

PROSPECTS FOR ECONOMIC GROWTH IN THE U.S.S.R.

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This is not an easy paper to write or to deliver. The only possible reason for asking me to deliver it is that I happen to be in America this year and you may want to see a new face. Among the participants at this conference are many who are eminently qualified to give a paper on this very subject. Worse still, some of them have in fact done so, and many of the participants, myself included, have had the benefit of reading their work.

The title of the paper might suggest that its author has special qualifications as a crystal gazer. Nothing is further from the truth. Fortunately he shares this defect with all his colleagues. Even if we ignore the possible direct impact of nuclear weapons on the productive capacity of industrial nations or the effect on output of the continuation or cessation of the arms race, there remain a large number of unforeseeable developments which could occur in the next decade in science and technology and also in economic and political organization, within the Soviet bloc and elsewhere, which defy prophecy. However, the Soviets have declared it to be their aim to overtake America in economic might within ten years or so and have indeed published a number of detailed targets for industry and agriculture through 1970 and even 1980. To some extent, therefore, it behooves us, who claim to possess some information on the Soviet economy, to become part-time reluctant crystal gazers. The interested nonspecialist is entitled to some kind of answer to such questions as: Are the declared aims of the Soviet leaders in the economic field within the realms of possibility? With what obstacles will they have to contend? By what criteria can one measure their progress toward their declared goals? How useful or misleading can such criteria be? Is it the case that the present organization of Soviet economic planning favors, or on the contrary restrains, the rapid growth envisaged by the plans? The observations that follow might be of interest, if only as an attempt to analyze some of the unknown factors relevant in considering answers to the above questions.

A few basic statistics related to Soviet performance and plans are given in the table below. These are deliberately confined to the present decade. Figures for so remote a year as 1980 seem to me hardly worth detailed discussion.

GROWTH OF U.S.S.R. ECONOMY
SOME OFFICIAL SOVIET DATA
(Growth per Annum, Per Cent)

	1954–61	1957–61	1959–65 (Plan)	1961–70 (Plan)
National income *	9.7	8.3	7.2	9.2
Gross agricultural production	6.0	3.1	8.0	9.6
Gross industrial production	10.1	10.2	8.6†	9.6
Steel	8.0	7.8	8.3‡	8.2
Oil	15.4	14.7	11.2‡	10.1
Electricity	11.7	11.3	12.3‡	12.8
Cement	15.6	14.9	12.9	10.2
Cotton fabrics	3.3	3.8	4.4	—
Wool fabrics	6.9	5.8	7.8	—
All fabrics	—	—	—	7.5

* Soviet definition ("material production").

† Implicitly revised upwards, though no new figure available.

‡ Upward revision of original targets for 1965 (steel from 88.5 to 96 m. tons, oil from 235 to upwards of 240 m. tons, electricity from 510 to upwards of 520 billion kilowatt-hour; some of these and other figures are midpoints of ranges).

All figures on past and present performance in the above table represent official claims. There is a wide consensus in favor of the proposition that they are a reliable guide to growth rates of industrial commodities in physical terms, but that agricultural statistics have been or may have been inflated on an increasing scale in the last few years. For familiar reasons, aggregate percentage indices of national income and industrial production should be regarded with varying degrees of suspicion, although it is accepted by most critics that the amount of exaggeration has somewhat declined compared with the period before 1950. This is not the place for a detailed discussion of statistical reliability; nonetheless, it is relevant to any real or implied extrapolations which might be involved in thinking about the future. For example, in the above table it appears that the growth plan through 1970 for Soviet industry implies a slight decline in tempo compared with that achieved in the last five years. However, if, as is widely believed, output indices for past years are somewhat exaggerated and if indices reflecting output plans for the future do not suffer from this defect, what looks like a slowdown may in fact represent acceleration. This seems a reasonable conclusion, on the admittedly imperfect evidence available, because the principal causes of exaggeration—essentially the effects on methods of aggregation, statistical reporting and certain production choices of the desire to show success in terms of high growth rates—ought not to be operating when planners make forward projections. But, of course, we will never be in a position to prove our point with official statistics when the future becomes the past.

It is tempting to substitute for the official figures a recomputation of the relevant magnitudes by Western scholars. However, it is not to a discussion of the intricacies of interpreting statistics of the past that this paper is to be devoted. I will confine myself to the following generalizations, which most specialists would probably accept. Plans for the rest of this decade call for increases in industrial output at a rate slightly exceeding that genuinely achieved in the recent past; agricultural production is to advance at a spectacular rate never hitherto achieved in Soviet (or, one suspects, anyone else's) history; at the same time, the trade and miscellaneous services sectors are to grow relatively more rapidly than ever before. It is common ground between Soviet planners and Western scholars that this extremely ambitious program cannot be achieved without much greater efficiency in the use of resources.

It is to this question of efficiency, and the related questions of organization and growth measurement, that much of the present paper will be devoted. If most of the space will be given over to industry rather than, for example, agriculture, the reason lies in the fact that some particularly interesting problems arise in assessing the ability of the U.S.S.R. to reach its industrial goals, whereas there is an understandable and well-founded unanimity among all Western commentators concerning the unrealistic nature of the agricultural plans for 1965 and 1970.

To sustain an annual growth rate of 9.6 per cent to the end of the present decade, Soviet industry will require a vast increase in the flow of materials and fuels. A very rapid rise in labor productivity is envisaged by the plans, for otherwise labor will be a bottleneck, the more so as the intake of young workers is now adversely affected by the low birth and survival rates of the war and early postwar years. This will require substantial investments. These facts are, of course, well known to Soviet planners, who seem justified in their confidence that the necessary minerals and fuels can be provided, while synthetics could provide alternatives to materials of agricultural origin. It is true that new areas may have to be developed, with consequent expenditures on social overhead capital. However, this may well increase the capital output ratio only in the short term, as some of these new sources o minerals, notably in the East, should prove economical to work. On recalls that the Kuzbas, though costly to bring into operation, has fo many years been providing some of the cheapest coal and steel in th Soviet Union.

However, organizational inadequacies and irrational investmen choices would quickly lead to shortages. They would express themselves by the emergence of bottlenecks, in shortages of labor, of capital, of this or that material.

Soviet economists and planners are well aware of this, also. Never has there been such widespread and frank discussion of the defects of the planning system. To an ever greater extent, Soviet economists express the view that a new stage has been reached, that the old methods of planning cannot any more cope with the problems of an increasingly mature and sophisticated industrial system. The pages of *Pravda* and of the specialized press are filled with debates on radical reforms. It is clear that these are needed if the economy is to cope with the demands made upon it and that drastic changes cannot be long delayed. In the words of Academician Berg, "We cannot permit ourselves the luxury of wasting time, if we soberly consider the tasks that face us and also the difficulties which we must overcome as quickly as possible."[1] The drastic changes in party and state organization announced in the November, 1962, plenum—and indeed the fact that so fundamental a discussion of the very bases of Soviet planning has been openly raging in the party's own press—clearly show that the political leadership is deeply concerned with remedying existing defects. It also follows that these matters are in the highest degree relevant to our assessment of the growth prospects of the U.S.S.R.

A number of economists in the East and West have at various times in the past few years echoed similar sentiments to those cited above. Oscar Lange, in his lectures in Belgrade in 1957, contrasted the "highly politicalized" system of the crash industrialization period, which he likened to a war economy, with the needs of a more developed and settled economic and political order, in which efficiency considerations become vitally important. With increasingly diversified needs, with more complex and less easily defined priorities, new methods are being sought, a fashioning of new planning instruments, new thories. There is a search for efficiency in the use of resources scarce in relation to needs, including the need to increase living standards.

All this is familiar enough in the form of generalizations. It is necessary to examine the nature of the actual defects from which their economy suffers and relate them, and possible means of correcting them, to problems of growth.

The present difficulties of Soviet planning relate both to the position of the planners and to the behavior pattern of enterprises.

There is ample evidence that the planners are overwhelmed by the complexity and burdens of planning both output and inputs of many thousands of enterprises. As the size of the econom number of planned interconnections increases much key problem in operational planning is what in *uvyazka,* or ensuring consistency of different portio the sheer volume of work requires it to be divided b stitutions, or different offices within the same instit plans often fail, at micro level, to match with anoth duction plan, creating headaches and holdups at e which the planners are responsible. Plans for costs, profits, calculated in yet other offices, may not matc plans, or one another. Investment plans, a vital aspe growth plan, are not properly geared in to the curr material allocation plans, so that machinery is not tion, building materials fail to arrive, and there is unfinished construction. All this is complicated by leadership is pursuing several objectives at once; so have become blurred or at least difficult to enforce. it remarkably difficult to give priority treatment t cially important investment projects" which the drawn up. For example, in the Russian republic in t entire investment plan has been 89 per cent fulfille age for the "especially important" projects was 83 complaints from below concerning "the endless mu peated changes in the plan through the year to whi apply, a consequence of the desperate attempts b with an impossible task. Thus the investment plan f was amended "almost 500 times" during 1961.[8]

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[1] *Pravda*, Oct. 24, 1962.

While centralized planning overburdens the or carrying it out, decentralization—the obvious re pletely unworkable so long as planners' instruction criterion for local decisions. The modest attempt t to territorial economic organs, in 1957, was inevita newed centralization. Within the system as it is, on position to know the needs of industry and of so these are not transmitted by any economic mech torial authority. The latter is therefore unable to f its decisions on the economy of other areas, and, in decentralized decision making must lead to intole The consequent recentralization went so far tha Estonian (republican) regional economic council

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[7] *Ekonomicheskaya gazeta,* Nov. 17, 1962, p. 5 (the figures rela of 1962).

[8] *Ibid.,* p. 6.

that the 17 commodities planned by Estonian officials represented 0.2 per cent of the industrial output of that republic, and that "all the rest is decided by the USSR Gosplan," which now plans production and distribution of close on 19,000 commodity designations.[4] Thus decentralization is both indispensable and impossible. Recentralization led to errors, confusion, overwork.[5] As a result of the reforms at present being implemented, the work may be redistributed, but without curing the diseases necessarily associated with centralization.

Planners are also handicapped by inadequate criteria for decision making. This applies most clearly to the much discussed problem of choice between investment variants. It applies also to the placing by the planners of production orders with different enterprises; as a Soviet critic pointed out, the officials in charge of this task do not even conceive it as part of their duty to know the relative costs in different enterprises; they are busy enough simply distributing the various production tasks to be performed.[6] These are direct consequences of quantitative administrative planning.

Obviously, if planners are to perform their tasks effectively, all this must be changed radically. Two approaches to reform may be observed. They have in common the belief that the central planners, expressing the will of the Communist party leadership, must decide the basic pattern of investment and production, as well as the basic "proportions" of the economy (i.e., allocation of resources between consumption and investment, industry and agriculture, and so on). However, one group of would-be reformers envisages a high degree of centralization, hoping that new planning techniques, and especially the electronic computer, will make it work effectively. The other lays much greater stress on enterprise initiative, on freely negotiated inter-enterprise contracts and supply arrangements, which would free the central planners of much of the detailed operational work which at present threatens to overwhelm them. Neither species of reform would be practicable without a major overhaul of the price system. Thus Berg, in his already cited article, points out that "the task of optimal and continuous planning cannot be carried out satisfactorily without a reform of price fixing. Can one speak of any optimum if the criterion of its determination is unclear at existing prices?" He advocated the creation of "a mathematical economic model of the economy" in which with the aid of input-output balances, it will be possible to "correct the sins of pricing." Nor do the advocates of decentralization deny the need to use mathematical techniques in central planning. One of the best-known of the "mathematical" reformers, Novozhilov, has emphasized that the proper use of computers and the efficiency prices which would emerge would make possible effective decentralization by providing proper price criteria at enterprise level. Nonetheless despite some similarities and overlaps of ideas, one can discern two different kinds of outlook about the matters with which central planners, as distinct from productive units, ought to be concerned. One could perhaps add a third view, apparently held by Khrushchev, to the effect that proper party leadership can solve all problems. In itsel such a view is naïve and irrelevant: "party leadership" cannot provid rational choices between investment alternatives, or match suppl plans with production programs. In sanctioning or tolerating open debate of basic planning problems, and indeed in his own not unfriendl comments on some of the radical reform proposals, Khrushchev ha shown himself aware of this. Yet his own predisposition towards party enforced priorities and administrative solutions leads him to lea towards the centralizers.

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[4] *Ibid.*, Nov. 10, 1962, p. 8.

[5] See A. Nove, "The industrial planning system—reforms in prospect," *Soviet Studies*, July, 1962.

[6] I. Borovitski, *Pravda*, Oct. 5, 1962.

We have so far been concentrating on planners' problems. Howeve as recently published debates in the Soviet press have underlined, muc that goes wrong does so in the microworld of enterprises. It is onl in the imagination of model builders, not in reality, that Soviet ente prises are passive and obedient executants of plans passed down fro above. In varying degrees, enterprise management is able to adapt i behavior, and the product mix, to the often ambiguous process measuring the fulfillment of plans. Since, for reasons already give the various plans it receives are frequently inconsistent with one a other, it can and indeed must maneuver to fulfill some plans at th expense of others—and its principal guideline, despite efforts change this deeply ingrained habit, remains the fulfillment of outp goals in tons or in rubles. Since rewards are related to the fulfillme of plans, managements are encouraged to conceal production possibi ities, to hoard, to refrain from doing too well in case the plan for t next period is made too "tight." New production methods, new design impose disproportionate risks upon management, with few compens tory rewards. "Plans are fulfilled at any price, to the detriment quality, reliability, durability."[7] The orders placed by the trading n work, which do on occasion reflect consumer demand, are too fr quently ignored by industrial enterprises, partly because the requir product mix does not fit plan fulfillment indicators, partly because the inflexibility of plans for material supplies, costs, prices. This a plies in at least equal measure to the problem of adapting producti to the requirements of other industrial enterprises. It is worth emph

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[7] *Ibid.*

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[8] *Pravda,* Sept. 9, 1

[9] *Pravda,* Sept. 21,

is noticeable that the bulk of the commodities listed
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Western countries. This suggests advantages of sca
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mine. It is surely not a coincidence that few of the
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num: "Steel production is like a much-used track
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It is worth noting that the top priorities of the S
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A few words are now needed concern
tion: the statistical reflection of irratio
decisions. It is far too easily assumed th
growth. But do they necessarily do so?

Of course, some do. Unfinished inves
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of "irrationalities" have no such effects
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argument.

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the Soviet case, which extends over a
the trouble lies precisely in an overcon
targets; i.e., on the adaptation of the
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may slow down statistical growth rates,
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sale is inconvenient from the standpoint

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[23] *Ekonomicheskaya gazeta*, Sept. 29, 1962, p. 39.

terms of either thousands of garments or rubles. It must not be forgotten that Soviet prices do not, as a rule, reflect degrees of scarcity, quality, or need, and tend to be changed by a very clumsy procedure at infrequent intervals—all of which tends to accentuate the extent to which aggregate statistics can mislead.

Irrational decisions on investments, location, and production may also be consistent with high growth rates. Of course, in these and in other respects there is an opportunity cost: resources cannot be devoted to other potential uses if they are wastefully deployed on irrational projects. Yet it does not necessarily follow that they could or would be used to greater statistical advantage. Let us take two different examples. Suppose a steelworks is located far from sources of materials, so that large additional transport costs are incurred, as has in fact happened in the case of Cherepovets. Or that agriculture is supplied with equipment so inefficient that its use actually raises the cost of production. The net output of the steelworks and the farms is adversely affected. But in both the examples, costs consist of unnecessary goods and services (freight transport, wagons, locomotives, rails, tractors, steel for all the above, fuel, etc.). These are part of the national product on any definition. True, the output of end products of consumers' goods is less than it otherwise would have been. The effect of the inefficiencies mentioned above is to increase unnecessarily the volume of intermediate products of "producers' goods." In fact, this may be one of the explanations for the striking contrast between growth of aggregate output in the U.S.S.R. and the much more modest growth in living standards.

Of course, it does not follow from all this that the vast bulk of Soviet output is either useless or defective. Wasteful or uneconomic practices are, in any event, common enough in Western economies. The point is that the Soviet economy was organized for rapid growth, particularly of heavy industry, and that the incentive system was deliberately related to measurable growth as a prime objective. But, since there is an unavoidable element of artificiality in measuring growth of varied assortments of products, this led to a sizable disparity between the growth of useful output and the growth of output as measured by statistics.[11] However, the Soviet political leadership and planners are increasingly aware that growth as such is a misleading criterion and that many highly desirable objectives of domestic and foreign policy require the elimination of waste, even if this waste does "count" statistically as output. Or, to put it another way, there is consciousness of the high opportunity cost, in terms of planners' preferences, of present inefficiencies.

[11] To this extent there is value in M. Polanyi's "Theory of conspicuous production," *Soviet Survey,* Oct.-Dec., 1960, though he seems to me to go much too far.

Let us now draw a few conclusions from the arguments set out above. Soviet industrial planning has run into heavy weather, and everyone knows it and is busily engaged in seeking remedies, suggesting new courses and reforms. It is by no means clear whether they will be able to find the desired balance between central planning of essentials and the local operational initiative which they are seeking. These are matters in the highest degree relevant to the assessment of Soviet performance in the next decade. We may see a period of experiment which could lead to increased confusion. In the short run, we may be seeing a victory of the "conservatives"—an attempt to maintain tight centralization, with the computor used to facilitate more speedy working out of consistent material balances and production plans. This may no more than enable the planners to keep pace with the constant increase in the volume of work which they have to perform as the economy grows. It is hard indeed to imagine how any form of centralization can eliminate the major inefficiencies of the present system, but we would still see impressive increases in industrial output in terms of percentages or tons or meters which can be used to substantiate the claim that America is being overtaken and the long-term plans fulfilled. In particular, the seven-year plan, due to end in 1965, may even be declared fulfilled in 1964. Yet these figures may be misleading, for reasons already discussed. Some of the output will be a consequence of inefficient resource utilization. In any case, the volume of production of basic commodities or aggregate indices expressing quantities are becoming increasingly irrelevant as a guide as to whether or when America is being overtaken. Soviet planners know this, too. Thus V. Dymshits, the rising star among industrial planners, expressed this thought when he said to the November, 1962, plenum: "It is time to take seriously the economy and rational utilization of timber. Of course the question is not one of overtaking America in the volume of timber-cutting, but of using the timber better than in America." These considerations apply, in even greater measure, to consumers' goods and services.

However, it seems hardly likely that major decentralizing changes in the planning system will be avoided for long. Let us suppose that the Liberman proposals or some variant of these are adopted, and that much greater flexibility is introduced into the economy through the use of incentives based on prices and profits. The net effect should be to make production much more responsive to demand and eliminate inefficiencies of many kinds. However, this could have two adverse effects on growth—one apparent and one real. In the first place, insofar as production geared to growth statistics is replaced by production for need, the statistics of growth may show a slowdown contrasting

with the increase in the relative usefulness of what i
hard, indeed, even to attempt to quantify this proj
point is surely not an insignificant one. The second a
cerns the likely drift of resources into nonpriority s
not promote rapid growth. This is the great fear of th
the source of much of their opposition to the Libern
nov proposals. On both counts the effect might well
consumer welfare and probably also a decline in t
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correct and also the strains and confusions which
them. However, statistical success is certainly possible

The national income targets through 1970 seen
realistic than the industrial output goals for the s
principal reason for skepticism relates to the nature
agriculture. A number of analysts, including the pres
written at length about the difficulties which beset S
and there is neither space nor the time to repeat thes
Even according to the official statistics, farm outp
very slowly since 1958 and is very far indeed behind
causes may be distinguished: unfavorable natural c
quate incentives, excessive centralization, and insuffic
One might add the complex of problems connected
relationship between the authorities and the peasant
excessive size of most farms. Unfortunately, cent
does so much harm in agriculture, has been somewha
ing 1962. However, one must expect vigorous efforts
ters. For example, there is to be a substantial rise c
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tial equipment, and for these and other reasons one r
the rise in agricultural production will be resumed,
ing on average to some 3 per cent per annum throug
this is only a small fraction of the planned rate. Si
culture was, according to Soviet official statistics, re
under 21 per cent of the national income, this in i
significant shortfall in the latter.

Other spheres of what is known in the Soviet U
production will hardly be in a position to make up th
volume of construction will of course increase, thoug
fast as industrial production. The relative contril

transportation to the national income ought to diminish somewhat with the elimination of unnecessary crosshauls resulting from an improvement of the planning system and economy in the transport of bulky, solid fuel by rail. On the other hand, the relative importance of trade is expected to increase, as the plans envisage substantial improvement in the still primitive system of distribution. The labor plans as published imply a substantial relative increase in the numbers engaged in trade, and also in so-called "nonmaterial" services, ranging from hairdressing and laundries to education and health. This presupposes considerable release of labor from agriculture, and a comparatively small addition to the industrial labor force, i.e., would be conditional upon a major improvement in productivity and efficiency. Failure in these respects would express itself in acute labor shortage in relation to the plan objectives, just as shortages of various materials are and will be indications of defects in planning.

This brings us yet again to the problems of the organization of the economy, price system, centralization and decentralization of decision making, investment criteria and the rest of the now familiar headaches of the Soviet planners. Here, in my view, lies the main bottleneck, the principal unknown, in the assessment of the Soviet economic performance in the immediate future. I do not think any confident forecast can be made either about the pattern of reforms or their consequences. In all probability we would be no better off even if we had before us all the information available to Soviet planners, as judging from the very frank discussions that have been published, they do not know the answer either.[13] It should be a very interesting decade for those who study the development of the practice and theory of the Soviet planning system.

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[13] Anyone who can read Russian is urged to read the debates in *Ekonomicheskaya gazeta*, Nov. 3 and 10, 1962.

YUGOSLAV ECONOMIC GROWTH AND ITS CONDITIONS

By JAROSLAV VANEK
Harvard University

Introduction

The purpose of this panel is to discuss economic growth in thr Communist countries, and the specific purpose of this paper is to so for the Yugoslav economy. More so perhaps than for the other tw cases considered, that of Yugoslavia requires particular attention be given not only to the purely statistical aspects of growth but al to the over-all economic conditions generating and conditioning th growth. Indeed, the Yugoslav experience is without precedent in number of respects; it resembles closely neither the Western nor t Eastern centrally planned prototype of economic organization. Rath —at least in the minds of the Yugoslav planners and lawmakers— is an attempt to combine the allocational efficiency of largely co petitive markets and highly decentralized decision making with t distributional efficiency of collective ownership.

Thus our task is not so much to present a systematic verbalizati of the very abundant statistical information presented in the Yug slav yearbooks and other statistical material, but rather, once t salient points about the growth have been made, to state the princip characteristics of the Yugoslav economy and to perform a few glob tests of its dynamic efficiency. Two sets of such tests can be conceiv of. On the one hand, because the economic system in Yugoslavia sin before the war has passed through three distinct stages, certain rou correlations can be established between the type of economic orga zation and the rate of growth of the economy. On the other hand, the most recent period when the economy assumed its specifica Yugoslav form, certain hypotheses bearing on efficiency can be form lated on the basis of a comparison with other Communist countr more closely adhering to the Russian type of allocational patterns.

I am confident about the accuracy of the data presented here have come to this conclusion through a summary study of the co sistency of different statistical materials. Moreover, many Weste economists who are acquainted with Yugoslav statistics as well as dependent Western institutions seem to share my opinion. Howev a thorough and independent appraisal of Yugoslav official statistics desirable and is yet to be undertaken.

Growth of the Economy Since before the War

Only very little precise information is available concerning the state of the Yugoslav economy preceding World War II. However, even from the little there is, it can be concluded that Yugoslavia as a whole was among the poorest countries of Europe in the prewar years and very little headway, if any, was made between the two wars towards changing this situation. In retrospect, the backwardness of Yugoslavia of those years will become apparent from the fact that in 1960, when the country could compare only with comparatively poorer industrialized countries with three to four hundred dollars of per capita income, industrial production was about five times higher than in 1939; in agriculture, on the other hand, the number of tractors rose about thirteen times and consumption of artificial fertilizers almost forty times in the same period.

An approximate link between the pre- and postwar periods can be established for 1947. In that year, the economy pulled itself out of the worst of the disastrous consequences of the war; industrial output stood at about 121 per cent of 1939 and agricutural production lagged by about 10 per cent behind the average of the thirties. Thus it can be concluded that in 1947 national product was roughly comparable to that of the years preceding the war.

Beginning with 1947, far superior and more abundant statistical information is available. For reasons that will become more apparent in the subsequent section, and in order to conform to the time limitation imposed by the scope of this panel, we prefer to carry out the discussion in reference to annual averages for certain specific periods rather than in reference to individual years. There are four such significant periods: the first covers the years 1947-52, the second 1952-56, the third 1956-60, and the fourth, covering the current Five Year Plan, extends to 1965. The overlapping years of the different periods are to be understood as reflecting the fact that our data mostly pertain to average growth rates. Thus, for example, the first period will reflect the average annual changes that took place between 1947 and 1952.

In the first period, gross national product grew at an average compound rate of 2.4 per cent per annum, in the second at 8.8 per cent, in the third at 12.6 per cent, and it is expected to increase at the rate of about 11 per cent in the current five-year period. While an important discontinuity will be observed in the first period, national product having actually declined in its latter part, over the second and third period the rate of growth was remarkably stable. In the second and third periods, the growth would actually have been even more regular were it not for a rather erratic behavior of the agricultural sector, whose growth still was heavily influenced by climatic conditions.

Industrial output was not only the most stable component of the Yugoslav growth pattern, but it was also the most progressive one. While increasing from about 33 to 46 per cent of gross national product between 1947 and 1960, it grew at a rate of about 6 per cent in the first period, about 12 per cent in the second, and 14 per cent in the third. Agriculture and construction barely stood above the 1947 level at the end of the second period; however, their increase was remarkable in the third period when the former grew at over 8 per cent and the latter at over 16 per cent.

As could be expected, the high rates of growth just pointed out for the whole economy were accompanied and largely generated by a relatively high rate of capital formation. Throughout the first three periods the share of national product allocated to gross investment was fairly stable, in the neighborhood of 30 per cent. In the first period it was somewhat lower than that figure, in the second slightly higher, and in the third just about at that percentage. In the terminal year of the current Five Year Plan, gross investment is expected to stand at about 33 per cent of national product.

In the first two periods, the most important recipient of investment funds was the industrial sector, its share in the total being close to 60 per cent. The second, absorbing almost two-thirds of the rest, was the transportation sector, while the residual of about 15 per cent of the total of gross capital formation was directed into construction, agriculture, and other sectors. In the third period, the share of total investment going into agriculture has increased to nearly 20 per cent of the total, largely at the expense of a declining share of investment in industry. A similar pattern, although somewhat less pronounced, is expected to prevail in the current Five Year Plan, with industrial investment falling just short of 50 per cent of the total.

Before ending this factual exposition, some remarks bearing on the most recent situation of the last two or three years are called for. Following 1959, a certain deceleration in the Yugoslav rate of growth is noticeable. While in the first year of that period it was fully imputable to a poor harvest, with industrial output growing faster than ever before, between 1960 and 1961 both major components of national product performed less satisfactorily, the former declining even further owing to another bad harvest, while the rate of growth of industrial output declined to about 7 per cent. In the current year 1962, the increase of industrial production seems to be comparable to the low rate of the preceding year rather than to the extraordinary growth rates of the second and third periods noted above. This then raises the question whether the targets set for industrial production in the current Five Year Plan are not too high. To answer it, we have to learn something about the possible causes of the abrupt deceleration between the third and fourth periods. We will return to this problem presently.

Efficiency of the Economy in the Light of Changing Systems and in the Light of International Comparisons

Following the second World War, Yugoslavia adopted an economic system much akin to that of Soviet Russia of that time, characterized by highly centralized administrative management of the economy, with very little reliance—if any—on the price mechanism, and only the roughest regard to an efficient resource allocation. Forced farm collectivization and compulsory state deliveries were the rule in the agricultural sector. The period corresponding roughly to this regime is defined in the preceding section as our first period.

The failure of the economy to grow at a satisfactory rate in those years, however, can be imputed only to a minor extent to the economic system. Without any doubt, political tensions that arose in the later part of the period between Yugoslavia and Russia with its satellites were the more important causes of the poor economic conditions. Trade with other Communist countries that was initially very important virtually disappeared in the years following 1948 and development credits extended by Soviet Russia were cut off.

As a result of these developments, Yugoslavia was brought to seek its trading partners and creditors outside of the Communist bloc and, perhaps more important, to make an attempt to find less rigid and more efficient forms of economic organization, better suited to its particular conditions. Abandonment of forced collectivization, the introduction of workers' councils with a high degree of economic autonomy, and a parallel political decentralization were the cornerstones of the reform.

The fundamental legislation establishing and defining the competence of the workers' councils was enacted in 1950, and the political reforms required by the new economic system followed in the subsequent period roughly extending into the second period, as defined in the preceding section. This period can be considered as one of gradual consolidation of the new conditions and also of recuperation from the disastrous situation caused by external Communist blockade of the country.

Although the significantly higher rates of growth can to some extent be imputed to the belated utilization of capacity accumulated in the preceding period, fruits of the reforms are already discernible in the second period. It must also be recognized that the results of those years would not have been so satisfactory were it not for the new trading outlets Yugoslavia found outside of the Communist bloc and, especially, for the economic aid it received primarily from the United States.

But the full impact of the new regime comes to light in the third period, when expectations of the Five Year Plan were realized in about four years, and when the national product grew at a rate of nearly 13 per cent, the highest in the world during that period. It will be recalled that in those years of the latter fifties gross capital formation was no higher, measured as a share of GNP, than in the preceding two.

As the success of the third period can largely be imputed to the new forms of economic organization, a somewhat more thorough and more abstract exposition of the principal factors of that efficiency is in order. First of all, it will be observed that the autonomy given to workers' councils in individual firms carries with it as one of its effects a high degree of market competition, individual firms being free to act in different markets to their greatest advantage; the natural consequence of such a situation is an efficient allocation of resources throughout the economy. Actually, it can be shown that a fully competitive system operating under workers' management and profit sharing, all other things being equal, will lead to the same type of Paretian optimum as is usually postulated for a competitive capitalistic system.

Moreover, at least some will agree that the incentive provided by profit sharing may yield better results than a system with a competitive capitalistic labor market. Also, it can be argued—figuratively—that a corporation's stockholders will be better informed and more concerned about the working of their enterprise if they are its workers than if they are not.

In regard to the allocation of investment funds, the Yugoslav system based on both external and internal financing of projects and the current practice with respect to the former type of financing guarantees—or at least is consistent with—an efficient allocation. Here we understand by "efficient allocation" approximate equalization of the internal rates of return throughout the economy. But the public management of an important portion of the supply side of the capital market carries with it another important factor of efficiency. It can be shown that the authorities managing the short- and long-term capital markets can make savings of the economy identically equal to investment, irrespective of the level of income. In other words, if properly managed, the capital market in Yugoslavia will closely reproduce the conditions of Say's law; actually, it can do so irrespective of whether prices are flexible downward or not. Price flexibility, in turn, considered as quite satisfactory in Yugoslavia—primarily because of the absence of monopolistic elements in the labor market—can only enhance the adjustment of the economy to full employment and full capacity, in cases where mismanagement of the capital market could otherwise tend to produce a Keynesian type of unemployment.

Let us now return briefly to the mainstream of our argument and attempt to explain, or at least hypothesize, about the abrupt declaration of the growth rate that has occurred in Yugoslavia in the last two years. In the first place, it can be argued that no economy could ever hope for a prolonged rate of growth of 13 per cent with a net capital formation of roughly 20 per cent of national income. Some gradual deceleration would be only the normal thing to be expected. However, a sudden decline from 13 per cent to around half of that rate cannot be explained on those grounds.

It appears that there were other factors, some of short-run, some of long-run character, some statistical, some economic, that have to be brought in to explain this decline. It has been suggested that part of the decline can simply be imputed to the statistical definition of national income: certain services of the order of about 10 per cent of national income are called unproductive and are not reckoned as components of national income. Nevertheless, it is very likely that the income elasticity of those services is greater than unity and thus part of the deceleration could be explained in this way. Another factor that ought not to be overlooked is a rather drastic reform of producers income tax, from strongly progressive to proportional, about two years ago. Such a tax, although probably desirable in the long run, may have acted as a strong disincentive in the short run especially with prosperous undertakings, where suddenly without any increases in productivity the return to be distributed among the members of the enterprise may have strongly increased.

Finally, it will be recalled that in both 1960 and 1961 agricultural output declined with respect to the preceding year. Not only did these crop failures have a direct impact on the growth of the total gross product of the economy, but, also, as certain manufacturing industries are linked in varying degrees to the farm output, value added by those industries was lagging behind what it would have been otherwise.

With these arguments in mind, it can be concluded that the long-run rate of growth of the Yugoslav economy to be expected for the fourth period (the current Five Year Plan) is not correctly indicated by the measured rate of the past two years; the true rate may well be 2 per cent above that measured. However, it also seems quite unlikely that the actual average increase for the current period would be as high as the planned 11 per cent.

In concluding this summary account of the performance of the Yugoslav economic system, let us draw at least one international comparison: that between Yugoslavia and Soviet Russia. Using Professor Bergson's data based on 1937 constant-price valuations (in *The Real National Income of Soviet Russia since 1928*, Chapter 14), we infer that the Russian rate of growth never exceeded 60 per cent of the peak rate of Yugoslavia, realized in the third period, and most of the time it was well below that percentage. But it also will be noted, again using Professor Bergson's computations, that the Russian share of GNP allocated to gross investment never stood above that of Yugoslavia, and only in the five-year period 1950-55 the share of gross investment was about comparable, even if still somewhat lower than the Yugoslav 30 per cent. It is also in this period that the peak rate of growth of 7.5 per cent was attained in Soviet Russia.

Considering the very rough evidence we have on the comparative levels of economic advancement of Yugoslavia and Soviet Russia, it can be argued, as a first approximation, that the state of economic development of Russia in the early fifties was about that of Yugoslavia in the latter part of that decade. If this hypothesis can be accepted as correct, then, with comparable shares allocated to gross investment, an average rate of growth of 12.6 per cent must be recognized as significantly higher than that of about 7.5 per cent; and all other things but the form of economic organization being approximately comparable, it appears, as could be expected on a priori grounds, that the Yugoslav form is more efficient than the other. On a per capita basis—a measure that some might prefer—the comparison comes out even more favorably for Yugoslavia; the rates of growth thus evaluated become 11.6 and 5.8, respectively.

ED GROWTH IN RUMANIA*

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the Rumanian Statistical Office has reported
vth which amount to the highest in the entire
ossible exception of Mongolia: gross indus-
es rose by 12 per cent in 1960 and by 16 per
ar Plan for 1959-65 calls for a sustained rate
bout 13 per cent per year. If we may judge
evidence suggests that the official index does
nt industry's rate of expansion. An independ-
on 106 published commodity series, weighted
ws that industry grew by 25 per cent a year
ost exactly the increase shown by the official
ear from 1952 to 1955 (against 11 per cent in
y 13 per cent a year from 1955 to 1960 (two
than the official index). Until a wider and
ple of commodity series is published, we can
tively accept the Statistical Office's calcula-

ve series to situate Rumania's importance as
umania is rich in petroleum, gas, and timber
tion of 18 million (slightly under 20 per cent
tion of the East European members of the
ation, excluding Albania), it accounts for 88
raction in this group, 81 per cent of the na-
ghly 20 per cent of the timber produced (all
are in timber output was much higher in the
ation measures have prompted retrenchments
respect to the materials and semifabricates
ntial ingredients of growth in these states,
eveloped and lags behind its allies, but it is

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paper, including sources and additional statistical informa-
d is available on request.

twelve sectors. Within sectors outputs were weighted by
to compute the value of gross output until 1955). Sectors
their estimated wage bills for 1958. Because of the unrep-
lished machinery and metal-goods series, I have substituted
for this sector, which grew at an appreciably slower rate

catching up: it produced nearly 7 per cent of the steel in the group in 1950, 9 per cent in 1960, and is slated to produce 3.3 million tons in 1965, or 11 per cent of the group's output. The respective figures for electric power output are approximately 4 per cent for 1950, 7 per cent for 1960, and 8 per cent for 1965, when 18.5 billion kilowatt-hours have been targeted. Rumania's plans for the production of iron ore are particularly ambitious: only 392,000 tons were produced in 1950; output was stepped up to 1.7 million tons in 1961 and is expected to reach 4 million tons by 1965, or 28 per cent of the group's combined output for that date. Of course, in comparison with the U.S.S.R., which plans to produce 160 million tons of iron ore by 1965, the East European total of 14.5 million tons is not impressive: the metallurgical industry in Rumania, as in every other satellite, will remain dependent on Soviet ore imports for a substantial proportion of its inputs.

The industrialization that has so far taken place in Rumania—starting from a very low prewar base—has not proceeded at an even pace. As the figures of industrial output quoted above indicate, there was a very rapid spurt of growth from 1948 to 1952, a period of consolidation from 1953 to 1955, and an accelerating trend since 1956, which has reached its apogee in the last two years. In some respects the first period is the most theoretically interesting: the question it raises is how the Rumanians could have developed their industry as fast as they did, and developed it along autarkic lines to boot, starting from extremely adverse conditions. Three years after the war ended, national income in constant prices was officially estimated to have reached only 67 per cent of its 1938 level. Most of the shortfall was due to the agricultural sector, whose net output was only 59 per cent of prewar. Three-quarters of the population were still engaged in agriculture at the time of the 1948 census. Up to 1953, the country was laboring under a heavy reparations burden, exacted by the Soviet Union, which in addition continued until the mid-fifties to syphon off a part of Rumanian oil, nonferrous metals, uranium, gas, and timber, at minimal cost to itself, through the joint Soviet-Rumanian companies which it had caused to be established in the area. Neither the Soviet Union nor any other country extended to Rumania any significant aid during its crucial years of development.

How could Rumanian industry be expanded so fast in face of these impediments? The answer lies partly in fuller utilization of capacity, which was still underemployed in 1948, due in part to shortages of imported raw materials. This fuller utilization was made possible by the increased provision of raw materials, financed by a rising level of exports, coupled with a rapid rise in nonagricultural employment, which went up from some 1.1 million persons in 1948 to 2.3 million in 1952. But the chief source of the growth in output, at least in the initial years of the first Five Year Plan (1951-55), seems to be traceable to an expanded capital stock. From 1950 to 1953, the share of accumulation—new investments plus increases in inventories minus depreciation—in the official estimates of national income exceeded 25 per cent, reaching a high of 29 per cent in 1953.

The low level of living that prevailed when the Communist government started its expansion program made it easier—paradoxically—to pursue an austerity policy in subsequent years and to accumulate resources for development. As agricultural production gradually recovered, at very little expense to the state in the way of machinery or fertilizers, it became possible to raise the food consumption of the population without stinting on the state's investment program. Rationing of consumer goods, incidentally, was maintained throughout this first industrialization period.

Many peasants, attracted by employment opportunities and higher incomes, moved into the cities as industrialization proceeded. This migration, by increasing the pressure on marketed food supplies, would, in all likelihood, have turned the terms of trade between industrial and agricultural prices in favor of the farm sector, if the state had not taken stringent countermeasures. The danger was particularly grave at a time when collectivization had barely started and private farmers held over three-fourths of the arable land. The government succeeded in extracting the peasant surpluses necessary to feed the city population in part through compulsory deliveries, for which it paid negligible prices, and in part by so-called "acquisitions" and "contracts," at higher prices, which gave the peasants, at least on the margin, some incentive to market their produce. Peasants were also allowed to sell a part of their surpluses on the free market, once they had met their obligations to the state. In 1950, these free sales, for which much higher prices were usually obtained than in transactions with the state, contributed close to two-thirds of peasants' cash incomes from the sale of farm products.

For these crucial years, no indexes of the relative prices of agricultural produce and of industrial goods purchased by farmers have been published, but we can still get some notion of the deterioration in the parity index—that is, of the shift of relative prices against farmers—from scattered price quotations. It turns out that, after the monetary reform of January, 1952, which in effect confiscated most of the peasants' cash holdings, the weighted average of prices peasants received per kilogram of grain, meat, and milk from all sources, including compulsory deliveries and free-market sales, bought only some 20 to 50 per cent of the amount of sugar, shoes, and cloth that might have been bought with a unit of each of these farm products in 1938.

These exactions from the peasants were especially heavy on the "kulaks," who though they owned only 8 per cent of the arable surface in 1952 still contributed, from compulsory deliveries alone, about 12 per cent of the fund of cereals and meat collected or purchased by the state. Although this policy, as we shall see, was not without adverse consequences in the long run, it still fulfilled immediate aims: the swelling urban population was fed, with enough rations and commercial sales at higher prices to supply an improving diet; by 1952-53, a small surplus of cereals was left over for exports and reserves, amounting to 20 per cent of the combined wheat and rye harvests for those two years.

Exports, during this period, were essential to finance imports of raw materials (cotton, iron ore, rubber), semifabricates (rolled steel products and nonferrous metals) as well as machinery and equipment, 40 to 50 per cent of the capital goods installed being purchased abroad. Besides cereals and other food products, which made up only 16.7 per cent of the value of exports in 1953 (versus 48.7 per cent in 1938), the principal source of foreign exchange consisted of raw materials and semifabricates (71 per cent of total in 1953). Petroleum products—chiefly crude oil in this earlier period—were of course the most important of these staples, at a time when around 60 per cent of the value of domestic production was exported. But other exports were also pushed as hard as possible to gain scarce currency; for example, it may be estimated that about half of cement output was exported in 1952 as well as a quarter of the volume of timber extracted from Rumania's depleted forests.

Stringent controls over urban and rural food consumption, paltry allocations of exchange for importing consumer goods, and a vigorous export drive help to explain how the state managed to press forward its investment program. Something remains to be said about the extraordinary concentration of investments in heavy industry. From 1950 to 1953, producer goods industries received a good half of all state investments in fixed assets. The jointly managed petroleum industry alone was assigned between a quarter and a fifth of this total, most of it going for well-drilling and exploration. After heavy industry, the largest claim on the investment fund was made by the transportation sector (12 to 17 per cent from 1950 to 1953). This went mainly to bring the heavily damaged railroad system back into operation; but no major new railroad lines were undertaken, and the surface-road network was left in its underdeveloped state. The only outstanding effort made to improve the country's infrastructure was the work done on the Danube-Black Sea Canal; but this project, which had proved exorbitantly expensive, was finally abandoned in 1953, when only seven kilometers of canal out of a total planned length of seventy-five kilometers had been excavated.

Once the priority needs of industry and transportation for investment funds had been met there was little left to develop agriculture and other low-priority sectors. Only about 10 per cent of state investments in fixed capital went to agriculture, which was of course concentrated on state farms. Housing was also neglected. In cities, where virtually all new housing was built directly by the state or with state credits, the expanding population had, for the most part, to be crowded into lodgings built before or during the war.

One wonders how long the first industrialization drive could have been kept up if the political climate had not changed in 1953 in the wake of Stalin's death. The backward state of agriculture would probably have stymied this headlong race in any event. By that time, the disincentive effects of compulsory measures were beginning to take their toll. Official estimates of milk and meat output show that prewar levels had nearly been recovered by 1950 but that little or no progress had been registered since that time. Indeed, if it had not been for the state farms, meat output would have fallen. There was a characteristic decline in the number of hogs kept by farmers for fattening. The investment did not pay, partly because of the high price of corn on the peasant market (the corn harvest was still at least 40 per cent below prewar). The problem of supplying the expanding urban population with enough meat and milk to come even close to the modest standards set before the war could not be solved without a radical improvement both in the volume and in the structure of farm output.

The Plenum of August 19-20, 1953, set itself the task of correcting the more flagrant excesses of the preceding period. It resolved to trim about 15 per cent from the total state investments in fixed capital scheduled for the next three years. At the same time, it raised by about a third the volume of investments assigned to agriculture, consumer goods industries, housing, and social-cultural services for the same period. Light industry alone was to receive nearly double the amount initially scheduled for those years in the Five Year Plan. The burden of the investment cut and of the reallocation toward sectors that had formerly been in a low-priority position fell on producer-goods industries and transportation.

The scope of this paper permits only a brief summary of developments during the next four years of consolidation and hesitant advance that followed the 1953 Plenum.[3]

[3] A more detailed version of the events of these years will be found in the expanded version of this paper referred to in note *.

Soon after the Plenum, compulsory deliveries from private farms and associations were reduced. A larger share of foodcrops and of meat production was paid for at contract prices—the highest tier of state purchase prices—nearly equivalent to free-market prices. The old policy of bludgeoning recalcitrant peasants into collectives was abandoned, and little progress was made toward collectivization in the next two years. (By 1955 only about a tenth of the country's peasant families had been incorporated into any sort of collective or association.) A portion of the area planted to technical crops, such as cotton, which had been overexpanded on state farms and collectives in recent years, as part of the regime's extreme autarkic policy, was given over to cereals. To reinforce the other measures tending toward the improvement of urban supplies of foodstuffs, cereals were imported in large quantities from the U.S.S.R., starting with 1955. The dissolution of the joint Soviet-Rumanian companies in 1954 and 1955 restored Rumanian control over domestic mineral resources and, regardless of the debts incurred to the U.S.S.R. on this account, probably gave the government more elbow room to carry out its new course.

The absolute level of investments declined in 1954, while the ratio of newly installed fixed assets to the volume of investment outlays during the year rose to 91 per cent, a high point for the entire decade. From 1953 to 1955, employment in industry and construction stagnated.

Thanks to increased marketings of farm products and to the drop in food exports, the government found it possible to abolish consumers' rationing at the end of 1954. Food consumption and levels of living generally rose rapidly both in the countryside and in the towns from 1954 on.

The breathing spell was interrupted in 1956 when investments rose sharply again and economic policy hardened on collectivization. But the Polish and Hungarian events of 1956 and 1957 precipitated another temporary retreat. In late 1956, the U.S.S.R. consented to waive its claims on $715 million of Rumanian debts arising from the liquidation of the joint companies. About the same time, the Party resolved to make further concessions to the peasants including the abolition of all compulsory deliveries with the exception of meat and wool.

The 1956-57 slowdown is the last on record. From 1958 on, the Rumanian Workers' Party no longer hesitated to press ahead on its twin objectives: rapid industrialization and collectivization of the countryside. By 1961, total fixed investments in the state sector and by collective farms had already more than doubled compared to 1957. At most, 15 per cent of this increase could be attributed to the wider coverage of agricultural investments due to collectivization. State farms and full-fledged collectives were cultivating 71 per cent of the country's arable land in 1961, as compared to only 28 per cent in 1957. The share of net accumulation in national income probably exceeded 25 per cent in 1961, the average mark set for the Six Year Plan (1960-65). The investment program moved forward on a wider front than during the first industrialization drive. State investments in agriculture, excluding collective farms for the sake of comparability, now took about twice as high a proportion of total state investments as in the early fifties. The relative importance of investments in residential housing also doubled. More funds were allotted to road construction, to the development of truck transportation, and to village electrification than in the past. Nevertheless, within industry, which received about half of total state investments, all but about 12 or 13 per cent of the outlays were still allotted to producer goods industries.

The extraordinarily fast growth of industrial production during these recent years was accompanied by remarkable productivity gains. Compared to a rise in gross industrial output of 61 per cent from 1957 to 1961, employment in industry went up by only 22 per cent. Capital productivity must also have risen, since the fixed assets installed in industry only increased by 37 per cent. No matter how the two inputs are weighted, the residual growth unexplained by the injection of additional labor and capital into the sector was appreciable. It is also significant that, in spite of collectivization, farm output managed to increase during this recent period. The net output of agriculture and forestry in the official national income accounts increased by 30 per cent between the years 1956-57 and 1960-61. Cereals output, which had been stagnating for some time, at last cranked up to a 10-15 per cent higher level than in 1934-38 and 1951-55. Most of the improvement was due to state farms, which have been absorbing the bulk of the available fertilizers and a disproportionate share of the machinery, selected seeds, and insecticides. On the remaining lands, there is no evidence so far that crop output has been adversely affected by collectivization. The situation with respect to animal production is not clear, inasmuch as most domestic animals were still held privately by members of associations and collective farms as well as by the few individual farms still allowed to operate in 1961. The government has prevented indiscriminate slaughtering of herds, partly by outright prohibition and partly by extending credits to collective farms to enable them to buy their members' animals at or near market value. It is still not known whether the deterioration of urban meat supplies in 1962 was due to the build-up of collective herds (as the regime claimed), to the increased meat consumption of collective peasants, to heavy exports, or to a combination of these causes.

As the state collected a larger "fund" of cereals, thanks mainly to deliveries by state farms, Rumania shifted from a net importing position (1955-57) to an increasingly significant export surplus, which reached 1,203,000 tons in 1961. This was only about half of 1910-14 exports but on a level with 1938. These exports overtaxed the country's capacity, if we may judge from the virtual rationing of bread in village shops instituted in the spring of 1962. The situation seems to have been aggravated by poor harvest results later on in the year.

Cereals make up only a small part of the impressive rise in the total volume of exports that has taken place in recent years. The value of foreign trade turnover, which had risen very slowly from 1955 to 1958, shot up by 70 per cent in the following three years. During this latter period, trade with Great Britain, Belgium, Sweden, and Switzerland more than tripled; with West Germany it rose by 133 per cent, for the first time surpassing trade with East Germany. In the meantime, the turnover with the Soviet Union expanded by only 12 per cent, its relative share declining to 40 per cent of Rumanian trade. Imports, consisting as usual mainly of iron ore, coking coals, rolled steels, textile raw materials, and industrial equipment, more than kept up with the industrial expansion. The rise in exports was due in part to a vigorous export drive in the engineering industries and in part to exports of building materials (mainly lumber products) and foodstuffs.

Although by mid-1962 the rapid pace of expansion was beginning to tell, the evidence so far is that the Communists have managed to kick off their second round of rapid industrialization without sacrificing real incomes, at least on the part of the urban population.[8]

The Rumanian leaders seem confident that they can maintain the economy's momentum without any more "breathing spells" or other retrenchments. Their planning officials argue that conditions are now much better than they were a decade ago for sustained growth. They emphasize first of all that a skilled labor force and competent managerial cadres have been trained and, second, that the investments of earlier years are now fructifying and providing opportunities for further progress. In personal interviews, I have also heard some of them take pride in the fact that in recent years they have budgeted their resources more prudently than certain of their neighbors, and that they have avoided painful readjustments, such as the Bulgarians and the Czechs have had to go through after overstraining their plans in 1959 and 1960. They would be loath to mention, although this factor also seems to have played a role in their recent successes, that the undisputed control they now exercise over their own mineral wealth—after settling Soviet claims—eventually helped to improve their foreign exchange position.

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[8] According to official statistics, real wages of employees in the state sector were 20 per cent higher in 1961 than in 1957. There is probably some overstatement in these figures, but it is obvious that some improvement must have taken place.

If the economy has indeed taken off, the Rumanian planners have accomplished a real *tour de force*. They have violated traditional principles of development. They have unbalanced the growth of interdependent sectors. They have pressed enormous amounts of capital into priority sectors and yet left virtually intact the large reservoir of underemployed labor eking out a living in agriculture. (As late as 1960 the rural population, which was slightly larger than before the war, still amounted to over two-thirds of the entire population. Strict administrative controls have had to be imposed on the rural population to prevent—or at least to limit—the spontaneous migration to the cities of peasants in search for higher incomes.)[4] More surprising still, little has been done to improve the country's infrastructure. After fifteen years of rapid development, there is not a single hard-surface road crossing the country from one end to the other, and the bulk of truck transportation is still confined to short hauls of heavy building materials; the railroad network is more or less what it was before the war; and the growth of the housing stock in the cities seriously lags behind the pace of urbanization.

Yet the country has been radically modernized in several important respects. Education and health services are a good deal more widespread than before the war: illiteracy has nearly been wiped out in the new generation, the crude death rate is now under 9 per 1,000, and the infant mortality rate has fallen to 75 per 1,000 live births—respectable achievements in a highly rural country. If prewar and postwar consumption statistics are at all accurate, the peasantry seems to be eating better on the average than in the thirties and is probably better clothed, too. In urban areas, although a large part, if not the majority, of the population is probably worse off than a quarter of a century ago, the real incomes of unskilled and semiskilled workers appear to be higher than they were in that depressed era.

Whatever we may think of all this lopsided development, we should keep in mind that the Rumanian authorities now have sufficient resources to mend the holes in their economic structure and to redress the balance of development. In the last four years the country has only begun to tap the potential benefits it stands to gain from international specialization. The productive capacity of the economy has been so far expanded that if consumers' sovereignty were now restored, better conditions would prevail for satisfying consumers' needs than before the industrialization drive started. This does not mean that Rumanian development would not have been smoother or even more effective under some other aegis or system. The benefits of different policies might have been greater or better distributed in time; but it would be idle to deny that both the regime and the nation have something to show for their labors.

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[4] Labor migration to the cities entails social costs, such as state-subsidized housing and social services, which are external to the enterprises bidding for this labor. If the wages of new entrants to the urban labor force exceed their alternative return in agriculture plus the net external diseconomies associated with the migration, then administrative measures to check the influx may be appropriate.

DISCUSSION

G. WARREN NUTTER: We could scarcely ask for a more revealing picture of economic performance of the Communist world than has been shown to us at this session. Of the two papers dealing with highly centralized economies, one has depicted spectacular successes; the other, spectacular failures. We might indeed wonder whether both speakers were describing the same economic species. But of course they were, as we see when we realize that the very successes outlined by Professor Montias for Rumania are symptoms of the deeper failures found by Professor Nove in the Soviet Union. This is made even clearer by Professor Vanek's analysis of recent performance of the Yugoslav economy.

In a highly centralized economy, the way to make headway quickly is to concentrate on easy tasks and neglect difficult ones. The trouble with this procedure is that some day the bill has to be paid, and it turns out to be higher than it would have been if problems had been properly dealt with as they arose. Neglect of some critical tasks simply because they are difficult means that all tasks gradually become less tractable. Eventually they all become too difficult for the system to manage. The marginalist approach commends itself in historic as well as static contexts.

As I understand Nove, this is what he says, though he puts it differently and illustrates it in a somewhat different way. The Soviet economy has, he says, achieved remarkable growth, but this very growth, accompanied by compounded complexities in the economy, now threatens to break down the machine that generated it. The problem has become so acute, in Nove's opinion, that there is no chance of meeting ambitious goals for the future, as outlined in various medium-range Soviet plans, without a basic overhauling of the planning and organizational structure. We might give Nove's paper the subtitle: "How success ruined the Soviet economy."

I cannot quarrel with Nove's excellent description of symptoms, but I am uneasy about his diagnosis of basic ills. When did the Soviet economy become sick? Did it have a sudden paralytic stroke in 1960 or thereabouts? Nove seems to suggest this, though it is not at all clear that he means to. Or has it suffered from muscular dystrophy all its life?

The latter view is more appealing to me. If reforms in economic structure would improve performance in the future, presumably they would also have done so in the past. While the Soviet economy was simpler in the interwar period than now, it was certainly complex enough to make centralized allocation of resources a poor substitute for the market place. The difference between then and now is surely no more than a matter of degree. Conspicuous production (to use Polanyi's descriptive phrase), neglect of critical sectors simply because measured growth is not likely to be spectacular, mismatching of plans, serious errors in investment decisions, distortions of planned output and misreporting of production at the operating level, deteriorating quality of goods in times of stress, wasteful inventories, inefficient production of intermediate products by plants consuming them, losses in shipment incurred because transport agencies are preoccupied with expeditious movement of goods—all these shortcomings so well illuminated by Nove have characterized the Soviet system from the beginning.

Indeed, the recent complaints of Khrushchev and other Soviet leaders, echoed by Nove, carry us back to the debate of the twenties and thirties on whether central planning, in its strictest sense, can work. The resemblance of recent views to those expressed more than thirty years ago by Mises and Hayek, among others, is no doubt unintended but nonetheless real. In any case, it is hard to see that much new has been added except chapter and verse. Oscar Lange once remarked that "a statue of Professor Mises ought to occupy an honorable place in the great hall of the Ministry of Socialization or of the Central Planning Board of the socialist state." ("On the Economic Theory of Socialism," *Review of Economic Studies,* October, 1936, page 53.) As things are now going, he may not have long to wait.

There is one important reason why the economy has managed to keep moving along: it has never worked the way it was intended and claimed to work. There has always been a complex network of extralegal markets of various shades of gray, tolerated by ruling authorities precisely because the system would not function otherwise. Without their "fixers" and "expediters," most plants would be strangled in red tape. Without their subsidiary shops for manufacture of needed supplies, they would starve periodically and unpredictably when allocated supplies fail to arrive. Similarly, distribution to households would have broken down without collective farm markets, "speculators," and the like.

But, if the economy would clearly benefit in most relevant respects from more decentralization, why have the needed reforms not been instituted long ago? One answer may be the deadweight of ideology, but this has not interfered seriously with more pragmatic considerations in times of severe troubles, as when NEP was introduced. Another possible answer is Stalin. This has more validity, but it reflects more than the person of Stalin himself and for this reason remains in the center of the stage in Russia.

Economic reform has, in fact, been blocked by a dilemma: how to achieve the decentralization needed to improve economic efficiency without destroying the autocratic political order. To the Soviet leadership, it is undoubtedly more important to preserve the dictatorship of the Communist Party than to achieve a more smoothly running economy. The leaders of the Party are bound to tread lightly in the field of experiments with organizational reforms as long as there is serious doubt about the political consequences.

This being so, Nove is right to avoid predictions about the likelihood of reforms over the next few years. As the Russians would say, there is a stir among the people. Something will happen, but when or how, nobody can say. One may believe, as I do, that Russia will be a future home of liberalism. But that future may be a generation or more away—or it may be less. Much depends on how the dice fall over the next few years.

As my remarks draw to a close, let me return to Nove's main point, that the ambitious medium-range plans of the Soviet Union cannot be achieved without fundamental economic reforms. With this I agree. However, there is another way out: adjust the plans to fit the system and make do. The Soviet leadership has often done this before without faltering in its stride and can be expected to do it again if the alternative—adjusting the system to fit the plans—seems too dangerous.

What, then, are the prospects for economic growth in Soviet Russia? Apparently they are good insofar as the Soviet element is removed, bad insofar as it is not. This I take to be the basic message of Nove's excellent paper.

ALEC NOVE: While I am grateful to Dr. Nutter for his generous words, I do feel that he has drawn from my arguments some conclusions far removed from any which I would accept. The Soviet economic system is not a failure, let alone a spectacular failure. It did succeed in building up a mighty industry in the face of many obstacles. It did so, it is true, at high cost and with a good deal of inefficiency in resource allocation. However, similar inefficiencies existed in the war economies of Western countries. Yet it does not follow even from the theories of Mises and Hayek that the war should have been conducted under unrestricted free-market conditions. Evidently, there exist situations in which the advantages of a free market are outweighed by certain disadvantages. War might be only one such situation. It is not for nothing that Lange used the parallel with a war economy to describe the pattern of Soviet-type industrialization in its early stages. This method of concentrating material and human resources on key sectors has surely little in common with "muscular dystrophy." Given the priority objectives of the regime, it was a source of strength.

It is my view that there is now a qualitative change, not merely one of degree, and that, in order to further the objectives of the Soviet political leadership, some major changes must be made in the system. Of course it is true that some reforms could have been undertaken earlier. It is not claimed that every mistake actually made was unavoidable. In any case there are few historical situations in any country in which reforms could be said to occur at the "right" time. But it does not follow that changes now under consideration in the U.S.S.R. could have been introduced in the thirties.

Dr. Nutter seems to me to overestimate the role of "fixers" in making the system work. Of greater importance, surely, is the fact that most enterprises do in any year pretty much as they did in the year previous, receiving materials mostly from the same suppliers and delivering goods mainly to established customers, with a resultant network of formal and informal relationships which reinforce the provisions of the plan. Fixers do exist, as do also the weaknesses analyzed in my paper. But it is going too far to attribute all the successes of Soviet-type planning to "extralegal markets," or to attribute only failures to the "Soviet element" in the system.

Finally, Dr. Nutter rightly notes the ideological and political obstacles to reform, but, in my view, underestimates the importance which the Communist Party leadership attaches to the fulfillment of its own economic program. It must somehow seek a way of reconciling its power position with greater efficiency. The Communist Party is not only a vested power-interest but an organization with far-reaching objectives, of which its economic program is one expression. There may well now exist a contradiction between the Communist Party's habits of exercizing power and the needs of a more mature industrial economy. However, the Party cannot resolve the contradiction by suppressing the needs; it may have to make some adjustments to its habits.

NICOLAS SPULBER: The purpose of this discussion is to examine economic trends in the U.S.S.R. and Eastern Europe, determine the conditions which affect growth, and evaluate the prospects for growth of these economies. There are, of course, numerous connections between the problems discussed and the tentative answers given in the three papers. I would like to point to some common ground existing between the Yugoslav and Rumanian problems since this might help somewhat to clarify the answers to the questions raised.

In the paper on Yugoslavia, Professor Vanek formulates a special hypothesis concerning the efficiency of that economy "in the light of changing [planning and administration] systems." He recalls notably that national income and industrial output grew at significantly different rates during the following three periods: 1947-52, 1952-56, and 1956-60. During the first period, under a Soviet-type, highly centralized planning system, income and industrial output grew by 2.4 and 6 per cent per year, respectively. During the second period, after the break with the bloc, adjustment to new conditions and the launching of fundamental reforms of the political and economic administration, income and industrial output grew by 8.8 and 12 per cent per annum. Finally, during the third period, when the "full impact" of the Tito-Kardelj reforms was felt, income and industrial output grew by 12.6 and 14 (?) per cent, respectively. While attributing the relatively slow rates of the first and second periods to "political tensions" with the bloc, or to the process "of gradual recuperation from the conditions of blockade"—thus avoiding any direct comment on the then prevailing system of planning—Professor Vanek attributes the "success of the third period" directly to the "new forms of economic organization." Professor Vanek adds, even, that it may be demonstrated that Paretian optimality could be reached under "a fully competitive system operating under workers' management and profit sharing" just as under a competitive capitalistic system, all "other" things being of course as usual "equal." There was, alas, a rapid deceleration in the Yugoslav growth rate from 1959 on, just as the full impact of reforms was making itself felt, but Professor Vanek disposes of the decline by assuming that it may be imputed to (1) changes in statistical definitions, (2) a drastic reform, which changed producers income tax from progressive to proportional, and (3) agricultural output declines.

I have no quarrel with the hypothesis of Paretian optimality under a "socialist competitive system." What would be helpful, it seems to me, before formulating any generalized conclusion on the efficiency of the Yugoslav "third period," is to distinguish between strategy of development and planning administration and to take account of any exogenous factors which may have accelerated the Yugoslav growth, such as, for instance, massive economic assistance.

While changing appreciably its planning administration, the Yugoslav federal government did not modify significantly in any of these periods the so-called "Soviet" strategy of development and its typical priorities: heavy industry in general, steel, machine tools, and electricity, in particular. From this point of view, the Yugoslav development has not diverged in any notable way from the developments taking place in the rest of Eastern Europe. But the Yugoslavs have been able to carry an even larger program of investment without depressing consumption to the levels prevailing in the neighboring countries, in no mean measure thanks to massive U.S. aid—totaling between 1950 and 1959 some $1.066 billion as general economic assistance, $91.5 million as assistance for special projects, and $724 million as military assistance. Under these circumstances one could, I am afraid, hazard comparatively little on the efficiency of the third period—everything not having been quite "equal" from one period to the next.

In the postwar industrialization of neighboring Rumania, Professor Montias also distinguishes three periods: 1948-52, 1952-55, and 1955-60, which cover roughly the same spans of time as the Vanek periods for Yugoslavia. According to Professor Montias' own computations, Rumanian industrial output grew between 1948 and 1960, 4.6 times as against 4.9 times claimed officially: the latter statistics are hence viewed by him as "tentatively" acceptable. Now further, according to his own computations, industrial output grew between 1948 and 1952 by 148 per cent (i.e., 25 per cent per year), between 1952 and 1955 by 24 per cent (8 per cent per year), and between 1955 and 1960 by 84 per cent (12 per cent per year). During the same time spans the Yugoslav industrial output grew by 9, 47, and also 84 per cent. Somewhat carried away by these impressive rates, Professor Montias remarks that "the Rumanian planners have accomplished a real *tour de force.* They have violated traditional principles of development. They have unbalanced the growth of interdependent sectors. They have pressed enormous amounts of capital into priority sectors; yet left virtually intact the large reservoir of underemployed labor eking out a living in agriculture." If I interpret him correctly, Professor Montias attributes the Rumanian success to the strategy of development pursued (while Professor Vanek attributes the Yugoslav success to methods, i.e., to planning procedure). Actually there was no special *tour de force* in Rumania. The Rumanians followed the Soviet blueprint and duplicated with almost absurd faithfulness everything, including Stalin's pet canal projects, the erection of a national "iron and steel base," and the development of machinery outputs which are but misconception of what industrialization should mean in a country like Rumania. Within the framework of this strategy, there was no better husbandry of resources there than in the other East European countries. In fact, the Rumanians made even more glaring mistakes than their neighbors in agriculture, where they discouraged the production of corn and then encouraged it again.

It may be interesting to note that the growth rate in Rumanian output fell precisely when the planners relaxed the harsh Stalinist "administrative" methods and that both the Yugoslav and the Rumanian performances were bettered by Bulgaria—if one accepts also "tentatively" its statistics. On a 1948 basis, Yugoslav gross industrial output rose in 1960 to 294, Rumanian gross output to 564 (Montias) or 590 (official) and Bulgarian gross output to 606—or from 1955 to 1960 by 109 per cent, far more than in either Yugoslavia or Rumania. (The rates of growth in income—which I have not quoted for simplification purposes—and in industrial output, were even more baffling for Albania, for instance.) Should one conclude from this that Stalinist planning methods are just as conducive to growth as "a fully competitive system operating under workers' management and profit sharing"? Such generalizations are far too sweeping: quite apart from the quality of the data on which they are based, they emphasize planning methods only and ignore the strategy which these methods serve, the underlying factor endowments and the specific conditions (e.g., the presence or absence of massive foreign aid) under which the strategy is carried out.

Industrialization of a similar type has certainly made headway in each of the East European countries. In all of them, including Yugoslavia, the labor force engaged in agriculture has, in absolute figures, remained by and large almost unchanged as related to prewar; but the industrial labor force has risen rapidly (e.g., in Rumania from 809,000 in 1950 to 1,241 gainfully employed in 1960; in Yugoslavia, from 582,000 in 1952 to 1,076 in 1959, etc.). In all these countries the main labor force increases have been concentrated in mining, machinery and metal working, chemicals, and woodworking. Agricultural output has remained by and large stationary, but industrial output has risen appreciably, notably in the "key" sectors of mining, iron and steel, electricity, etc. The most outstanding result of this industrialization along similar lines has, to my mind, been the formation and training in every one of these countries of an increasingly large strata of skilled workers, technicians, and managers.

The vast U.S.S.R. has aimed, since 1928, to create an autarkic basis for both rapid industrialization and a high military potential. But the faithful reproduction by small countries of strictly the same pattern of industrialization, whatever their resource endowment may be, strikes one as a methodical effort to waste resources. With microscopic (and by Western standards inefficient) duplication from one country to the next, each of these countries has raised the output of the same type of products and has persevered in an industrialization running on closely parallel autarkic lines. The awareness that only a systematic division of labor in Eastern Europe could open new and broader vistas for industrializing the area as a whole as well as for each of these countries is now increasing, though for a number of reasons which I have discussed on other occasions (*American Economic Review*, May, 1959, pages 125-27) the Communist regimes (excluding Yugoslavia, perhaps) seem both incapable and reluctant to engage resolutely on this path. Obviously, such division would lead to the closing down of many mines and many of those duplicating and relatively inefficient plants, old or new, which strive so hard today to overfulfill their targets and to push upwards their country's rate of growth.

JAPANESE ECONOMIC DEVELOPMENT

RECENT JAPANESE GROWTH IN HISTORICAL PERSPECTIVE

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I

The Japanese economy following World War II has sometimes been compared to the miraculous Phoenix. Consumed by the fires of war, the economy rose from the ashes, achieving the highest rates of growth of any country in the postwar world. Compared to the prewar years, it was noted that Japan now had higher rates of over-all growth, higher proportions of savings and investments, an apparently stable commodity price level, and a lesser dependence on foreign trade. All this must have looked pretty wonderful—especially to Americans beset by their own growth problems—and the term "economic miracle" was frequently used to describe the situation. Our faith in economic miracles is limited, and we propose to take a hard look at these recent events in the perspective of Japanese economic history. Our central topic will be simply this: what can be said about—or how can we explain—economic growth in Japan between 1953 and 1960,[1] compared to previous periods of rapid growth. And, in fact, these periods (see Table 1) can introduce our first substantive point.

The seven years of very rapid growth following postwar rehabilitation were not the only such period in Japanese development. Accordingly, we have selected three similar periods for comparison: the rapid development following the Russo-Japanese War of 1904-05, the period of speedy growth associated with World War I, and the thirties when Japan was first to recover from the Great Depression and then embarked on another boom associated with an armaments race in preparation for World War II. We find that the average rate of growth during the postwar years has been more rapid than in the selected prewar intervals, but the differences are not extreme. In both prewar and postwar years, the annual rates of growth fluctuated very widely, and peak values are quite similar. The real differences arise in trough

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[1] It is generally agreed that the years 1945-52 represented an abnormal rehabilitation period. Most observers also seem to agree that Japan had recovered at least from the more direct effects of the war by 1952-54, and therefore we concentrate on the more normal ensuing years. At this writing, information after 1960 is not yet available.

TABLE 1

GROWTH RATES OF GDP FOR SELECTED PERIODS

I 1905–12		II 1912–19		III 1931–38		IV 1953–60	
1905–06	16.5%	1912–13	−0.9%	1931–32	11.0%	1953–54	3.3%
1906–07	6.1	1913–14	1.5	1932–33	10.3	1954–55	10.3
1907–08	5.5	1914–15	1.8	1933–34	2.5	1955–56	9.0
1908–09	−0.7	1915–16	10.3	1934–35	8.4	1956–57	7.9
1909–10	−3.0	1916–17	3.2	1935–36	7.7	1957–58	3.3
1910–11	14.2	1917–18	14.1	1936–37	5.7	1958–59	17.9
1911–12	8.1	1918–19	18.8	1937–38	6.7	1959–60	13.2
Average:	6.7		7.0		7.5		9.3
Range: 16.5 to −3		18.8 to −0.9		11.0 to 2.5		17.9 to 3.3	

SOURCE: Prewar data from Hitotsubashi University, Institute of Economic Research, Rockefeller Project, Preliminary Report No. D 11 (1961); postwar data from Japan, Economic Planning Agency, *Kokumin shotoku hakusho* (White Paper on National Income), 1960 (hereafter cited as KSH 1960), pages 178–79.

values. From 1953 to 1960, the annual growth rate never dipped below 3.3 per cent (note that this is a bit higher than the average rate of growth of the U. S. between 1950-59), while in prewar years, negative entries are recorded in two of the three comparison periods. It seems to us that this type of comparison, relying on relatively short periods of rapid growth, is much more meaningful than the contrast with the long-term prewar average growth rate, which stood at roughly 4 per cent per year between 1878 and 1940. We can still recognize the unusually rapid pace of development from 1953 to 1960, but we avoid the temptation of exaggerating the differences.

Let us now try to isolate the factors which accounted for the un-

TABLE 2

DECOMPOSITION OF THE NATIONAL PRODUCT GROWTH RATE*

	G	$= n$	$+ g$	$g = g_p$	$+ g_i$	Sectoral Shares of g_p		
						A	M	S
I. 1905–12	6.5%	0.4%	6.1%	5.2%	0.9%	2.9%	0.6%	1.7%
II. 1912–19	6.7	0.1	6.6	5.9	0.7	2.5	1.8	1.6
III. 1931–38	7.4	1.2	6.2	5.3	0.9	1.2	2.0	2.1
IV. 1953–60	9.1	2.0	7.1	5.8	1.3	1.0	2.2	2.6

* G (national product), n (gainfully employed population), g (aggregate output per worker), g_p (aggregate output per worker due to increases in productivity proper), g_i (aggregate output per worker related to shifts in the industrial structure)—all stated in terms of average annual growth rates. A (agriculture), M (manufacturing), and S (services). All changes represent the average annual rate of growth between the first and last year of the period. Intermediate years are excluded. Prewar calculations refer to GDP; postwar calculations to NDP. (This also holds for Table 3.)

SOURCE: See Table 1.

usually rapid growth between 1953 and 1960. Tables 2 and 3 contain some of the relevant information.

First, a word of explanation about what the entries in the table mean and how they were derived. We retain our four previously selected periods of rapid growth, and in Table 2, for each period, decompose the growth rate of output into its component parts; that is, the growth rate of the gainfully employed population and the growth rate of aggregate output per worker. The latter term is divisible into two parts: increases in output per worker due to shifts in the industrial structure (that is, the movement of workers from lower to higher productivity occupations) which we will call the "transfer effect" and increases due to a rise in what we label "productivity proper." In turn, the growth rates of productivity proper can be observed sectorally for agriculture, manufacturing, and services.[2] In Table 3, the major components of the growth rate are shown separately for agriculture, manufacturing, and services.

TABLE 3

SECTORAL DECOMPOSITION OF THE NATIONAL PRODUCT GROWTH RATE*
(Unweighted)

	G			*g*			*n*		
	A	*M*	*S*	*A*	*M*	*S*	*A*	*M*	*S*
I. 1905–12	6.9%	5.4%	6.8%	7.4%	2.6%	4.4%	−0.5%	2.8%	2.4%
II. 1912–19	5.3	9.8	6.3	6.6	7.8	4.2	−1.3	2.0	2.1
III. 1931–38	6.9	12.0	5.1	7.2	6.8	3.9	−0.3	5.2	1.2
IV. 1953–60	3.4	11.5	9.6	5.2	7.4	5.0	−1.8	4.1	4.6

* The figures for agriculture must be treated with great care because the rates of growth are measured only in terms of the two terminal years of each period. For this reason, they are unduly distorted by bumper crops, and hide the fact that, in a trend sense, the growth rate of agricultural output and productivity has been declining since about 1905–10 and has risen a bit in the postwar period.

SOURCE: See Table 1

Now, what do the entries in the table reveal? We may begin by observing a significant acceleration in the rate of growth of the gainfully occupied population (see *n*, Table 2). This is, of course, the effect of demographic forces already in evidence during the thirties. The natural rate of increase of the Japanese population quickened in the twenties, maintained comparatively high levels during the thirties and World

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[2] In a simple two sector case, if we denote the sectoral output share by $\overline{W}_a$ and $\overline{W}_m$, and the sectoral labor share by $\overline{V}_a$ and $\overline{V}_m$, and using the other notation described in Table 2, then:

$$g_p = g_a\overline{W}_a + g_m\overline{W}_m \quad \text{and} \quad g_t = n_a(\overline{W}_a - \overline{V}_a) + n_m(\overline{W}_m - \overline{V}_m).$$

Values for g_p and g_t were calculated using the average shares of beginning and terminal years for each interval. The g_t equation reflects two types of movements—(*a*) a transfer from one sector to another (for example, from agriculture to manufacturing), and (*b*) a net increase in employment in a sector resulting from a general increase in the labor force. We refer to both as the transfer effect, or as shifts in the industrial structure.

War II, and rose to unprecedented heights in the immediate postwar period. (Since 1949, however, the pace of natural increase has been declining sharply.) These baby booms, and other forces, have altered the age structure of the population, so that in the postwar period the proportion of the working age population grew rapidly and reached an all-time high. In the short period 1953-58, that proportion rose from 62.2 per cent to 65.1 per cent of total population.[8]

Obviously the next question is: where did these increased numbers become gainfully employed? An adequate answer, focusing on 1953-60, must take into account some longer run considerations. We know that they were not going into agriculture. Throughout modern Japanese economic history, that is ever since 1868, the rate of growth of the agricultural labor force has been close to zero or negative, and during intervals of rapid growth it has usually been negative. But it has never been as sharply negative as today. (See Table 3.) Since World War II, for the first time, the absolute number working in agriculture has been declining rapidly. We are therefore confronting two economic forces: a general increase in the rate of growth of the gainfully occupied population affecting only the nonagricultural sectors, and a shift of the already employed labor force from agriculture to manufacturing and services. Both contribute to the transfer effect (g_t), and it was particularly important between 1953 and 1960. Since transfer implies movement from lower to higher productivity occupations, a considerable share of the postwar increases in the growth rate of output per man is due to this force. Our figures (Table 2) indicate that from 1953 to 1960, 18.6 per cent of the increases in output per man are assignable to shifts in the industrial structure. This is a much higher figure than prevailed for any previous period of rapid growth: 12.4 per cent for 1905-12, 10.5 per cent for 1912-19, and 13.8 per cent for 1931-38.

A glance at Table 3 indicates in somewhat greater detail the effects of transfer and net increases in the labor force. The unusual characteristic of the postwar years would appear to be high rates of labor force growth in both manufacturing and services, and especially the very sharp rise of employment in services compared to the prewar intervals, and in particular the thirties. Here one must be cautious, because towards the end of the thirties government labor allocation policies created conditions which forced workers into manufacturing while preventing a normal expansion of services.

In analyzing the over-all growth rate of output per worker, we must also look at what has happened to productivity proper. Examining this term first in aggregate form (g_p), we note that it follows less of a trend than any other component of the GDP growth rate. Productivity proper fluctuates somewhat from period to period, but always within the rather narrow range of 5.2 to 5.8 per cent per year. Things appear in a different light when we examine the sectoral shares of g_p (Table 2). Until about the time of World War I, increases in agricultural productivity played a major role in sustaining this particular rate. Since then, the weights have changed in favor of manufacturing and services, and productivity proper has continued to increase in these sectors until the present. Thus, for 1905-12, 56.4 per cent of the growth of productivity proper took place in agriculture, 11.0 per cent in manufacturing, and 32.6 per cent in services. For 1953-60, these figures are 17.0 per cent, 37.9 per cent, 45.1 per cent, respectively.

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[8] Cf. Hitotsubashi University, Institute of Economic Research, *Nihon keizai tōkei* (Japanese Economic Statistics), p. 38.

The special features of the postwar period are most easily observable in Table 3. The postwar growth rate of nonagriculture is higher than during any prewar interval, because both manufacturing and services are developing at great speed. Before World War II the gap between manufacturing and services was wider. Increases in productivity have been less important than the fact that manufacturing and services have been able to absorb workers at a rapid rate while sustaining the level of productivity growth.

Perhaps it would be helpful to summarize the argument briefly at this point. Compared to previous spurts in Japanese growth, the postwar years are distinguished by their high average level. This is related to rapid net increases in the labor force and shifts in the industrial structure, while the growth rate of productivity proper has been maintained at a high level. We must now turn to a much more difficult problem: an attempt to explain what factors within the economy have made these happenings possible.

II

There can be no doubt that in the postwar years Japan had her share of economic good luck. Terms of trade were favorable, the Korean war supplied a big boost just at the right time, and the burden of national defense was carried largely by the United States. No one will be inclined to underrate these and factors of a similar nature, but there are other elements of much greater importance for an understanding of recent rapid growth. Foremost among these is the level of investment and notably the strength of autonomous investment, in combination with the rapid development of facilitating industries, especially in the service sector. The absorption of an increased labor force, shifts in the industrial structure, the maintenance of rapid growth of labor productivity—all this was made possible because of the unprecedented levels of investment in response to the growing requirements of modern—and especially heavy—industry.

In comparing pre- and postwar capital formation, one is immediately aware of some fundamental differences. First of all, the proportion of gross investment to gross domestic expenditure now stands at an unprecedented level. The average proportion for 1953-60 was 33.2 per cent, and in 1960 it reached an almost unbelievable 38.2 per cent. In general, the trend of the capital-formation proportion has been rising in Japan, but during the earlier phases of rapid growth it was still at a much lower level: 15.1 per cent for 1905-12; 16.5 per cent for 1912-19; and 23.2 per cent for 1931-38.

What can account for the postwar level? Essentially it is due to a steep rise in the rate of private investment in plant and equipment, especially since 1956. The role of government investment—always an important magnitude—has also been altered, but here the change has been mainly compositional. Before the war, government accounted for 50 per cent of all investment if military investment is included, and for 40 per cent if it is excluded. By contrast, in 1960, government investment amounted to only 27 per cent of the total. This was not the result of an absolute decrease of public investment—in fact, the ratio of government investment to GNP has been rising virtually since the eighties—but rather the effect of the more rapid growth of private investment. Of course, it must also be stressed that public investment is today much more "productive," because defense expenditures have fallen to very low levels.

While recognizing the advantages of increased productive investment on the part of the government, it is still pretty clear that what really needs to be explained is the unprecedented vigor of the private sector. The figures indicate great private investment spurts, especially following the rather mild recessions, reaching annual rates of increase in the neighborhood of 55-60 per cent during 1956-57. It must be recognized at the outset that the postwar years were especially suitable for rapid technological progress. Japanese industry, due to the war, had been isolated for more than a decade, and there existed great opportunities to reduce rapidly a technological gap. Sometimes this has been called the "catching-up" effect, and some may feel that it is still the best explanation of the current investment wave. This is, however, a much too narrow view. Recently, for example, Japanese entrepreneurs have been concerned about possible trade liberalization and relations with the Common Market. This has quickened the introduction of new techniques and is unrelated to wartime isolation.

Catching up makes much more sense if we understand the concept in broad historical terms. Then it means the transformation of a semi-developed into a mature economy, and this is what has been happening in Japan. Underlying this transformation is a shift in the industrial structure from light to heavy industry. Productive investment by the private sector has tended to concentrate increasingly on basic heavy industries, and the dominant proportion of the technical innovations came in these sectors. For example, in 1960 the level of plant and equipment investment was 3.25 times higher than in 1955 for all production, but it was 10.5 times higher for the nonferrous metals industry, 9 times higher for the iron and steel and metals manufacturing industry, and approximately 7 times higher for the machinery, electrical machinery, and transportation equipment industry.[4]

Quite clearly, the entrepreneur did not lack incentives to promote these heavy investments. In the short run, ordinary private profit considerations can adequately explain the desire to introduce new and better techniques. Private profits were high because domestic and foreign demand was strong, productivity was rising, and wages were lagging behind productivity. The entrepreneur was merely maximizing profits. In the longer run, the government did much to bolster expectations of continued rapid growth. It compiled several long-range growth plans as guideposts for private business, and all of these anticipated a rate of expansion maintained at very high levels.

TABLE 4

INCREMENTAL CAPITAL-OUTPUT RATIO*

	AGGREGATE COR		SPECIFIED COR
	Gross	Net	
I. 1905–12	2.3	1.9	2.2
II. 1912–19	2.3	2.0	1.5
III. 1931–38	3.1	2.8	1.9
IV. 1953–60	3.7	2.8	2.7

* The aggregate capital-output ratio includes all types of investment. The specified capital-output ratio is gross, and excludes agriculture, residential construction, inventories, and military investment.

SOURCE: Prewar, based on data in Henry Rosovsky, *Capital Formation in Japan, 1868–1940* (New York: The Free Press of Glencoe, 1961), and Hitotsubashi Preliminary Report D 11. Postwar, based on KSH, 1960.

That the postwar period is distinguished by the growth of heavy industry is deserving of explicit recognition. The previous periods of rapid development—in particular the years from 1905 to 1919—were much more closely connected with the expansion of light industries and especially textiles. (In the thirties, of course, military industries were paramount.) Indeed, for 1953-60 we can observe a considerable increase in the capital-output ratio (see Table 4). This shows, we believe, that technical progress in recent years has not been "neutral"; quite the contrary, it has been capital-using. Thus, the rapidity of growth in 1953-60 was due not to a smaller capital-output ratio, but to an exceedingly high proportion of investment. It seems to us that the rise in the capital-output ratio has been caused by the emphasis on heavy industries within total investment, and also by the general shift of labor to more capital-intensive occupations—i.e., the transfer effect. Given realistic assumptions, a rising incremental capital-output ratio represents, after some time, a burden for the investor. If labor's relative share, the rate of interest, and demand conditions remain more or less unchanged, a rise in the ratio means smaller gross profits. These assumptions apply to the years under discussion and suggest that the very high levels of investment cannot be maintained indefinitely.

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[4] Japan, Economic Planning Agency, *Shihon stokku to keizai seichō* (Capital Stock and Economic Growth) (1962), p. 103.

III

An aspect of the postwar Japanese economy which has aroused perhaps the greatest degree of international interest is the alleged presence of price stability. Although we cannot, within the imposed limits, do justice to this issue, at the same time we are unable to resist the temptation to say one or two things about it. The temptation is particularly strong because the appearance of price stability in Japan would constitute the reversal of a well-established prewar trend; namely, the presence of secular inflation and particularly the presence of inflation during previous intervals of rapid growth.[5]

Naturally, it would under almost any circumstances be an error to speak of the short postwar period as a reversal of a trend. A much longer period would be required for a valid judgment. But even if we choose to regard these years as somehow representative of long-run forces, there remain difficulties. From 1945 to 1953 there was considerable price instability combined with inflation. Prices did settle down from 1954 to 1959, and since then have been climbing with quite considerable rapidity. Inflation has, once more, become an issue in Japanese domestic politics. However, even the period 1954-59 requires further qualifications. At that time, producers' prices fluctuated cyclically without any particular trend, but consumers' prices continued to creep upwards at, it is true, a very slow pace (from 100 in 1955 to 104.8 in 1959). This is still a very good showing by Japanese historical standards, and it may well be that there are elements in the postwar situation which have lessened inflationary pressure. Let us examine some of the possibilities.

One's attention naturally turns first to wages, and the evidence is straightforward. Between 1953 and 1960 wages generally lagged behind productivity, and consequently the wage-cost index declined by approximately 10 per cent. There was no real pressure from the wage side because Japan possesses an abundant supply of educated and skilled labor, strengthened, as we saw, by the population boom of the thirties and late forties, and also by the large group of returnees from a vanished empire. It is possible that the wage-productivity lag was especially pronounced in recent years, but it is equally likely that it has been in existence ever since industrialization began, and therefore cannot be viewed as a structural change. Beginning in 1960, interestingly enough, there have been complaints of labor shortages. These are still faint rumblings, but one may not exclude the possibility that, combined with more mature union activity, this particular lag has a short life expectancy. (The same argument can be applied to the favorable terms of trade; they could also turn out to be a transitory phenomenon.)

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[5] We have treated prewar "inflationary growth" in more detail elsewhere. See Kazushi Ohkawa and Henry Rosovsky, "Economic Fluctuations in Prewar Japan," *Hitotsubashi J. of Econ.*, Winter, 1962.

A factor which may have a much more lasting effect introduces the complicated question of the generation of savings within the Japanese economy. In these terms, one would argue that inflationary pressure in the postwar period has been reduced because domestic investment was financed by an adequate supply of domestic savings. This statement is correct, but it requires some amplification. Historically, we can identify a clear secular rise in the domestic-savings ratio, for example, from a gross domestic savings/domestic output ratio of 15.1 per cent in 1905-12 to one of 33.2 per cent in 1953-60. The ratio for 1931-38 stood at 23.2 per cent; so that even if the upward trend is accepted, there remains the need to explain the discrete and sudden rise of the more recent years.

One factor of considerable importance is the changed position of government after World War II. At present, during an era of very rapid growth, the shares of public and private savings maintain a very steady relationship—approximately 70 per cent private and 30 per cent public. Historically, at least in the twentieth century, this is an unusual situation. During prewar growth spurts we find sudden shifts in the share of public and private savings, together with frequent and prolonged government deficits, creating strong inflationary pressures. The ability of government in recent years to generate surpluses with which to finance the major share of a heavy investment program is due in part to a lightening of what had been a heavy military burden. But there are other, equally important, reasons. One is a more progressive system of taxation which did much to raise government surpluses, especially when the actual rate of income growth exceeded the anticipated rate. The other reason is connected with the way government and public enterprises are now operated. Many of these organizations work under conditions which automatically create surpluses for planned expansion. A most recent raise in freight rates by the National Railway Corporation provides a good illustration. The raise required Diet approval, and this was given subject to the condition that all additional revenues be spent for railroad investment.

A set of factors which influences the degree of inflationary pressure, and which concerns both the public and private sectors, is the aggregate marginal savings ratio ($\Delta S/\Delta Y$), and the degree to which savings in general are elastic with respect to the growth rate of GNP. Once again, postwar Japan exhibits unusual values. For gross domestic savings, using the established time intervals, our very tentative calculations yield the following values for $\Delta S/\Delta Y$—1905-12: 18.6; 1912-19: 25.2; 1931–38: 36.9; 1953–60: 58.4. The elasticities $\left(\frac{\Delta S/\Delta Y}{S/Y}\right)$ also show an appreciable increase—1905-12: 1.24; 1912-19: 1.59; 1931-38: 1.59; 1953-60: 1.78. Neither item of information is easily interpreted because we know only little about the components of prewar savings. This much does seem clear. The higher elasticity of savings to growth can be assigned in large part to the changed behavior of government, as previously described. Personal savings are more stable, and corporate savings are highly growth-elastic, but that was probably equally true before the war. The new level of the aggregate marginal savings ratio is, no doubt, affected by the behavior of personal savings and may be related to the over-all rapidity of postwar growth. Perhaps we are seeing the effect of a time lag: income growing rapidly while consumption levels adjust much more slowly. In Japanese economic development this would not be a novelty.

What the future holds with regard to price stability is hard to predict. In many ways, however, the situation now is more hopeful than in the past.

IV

Having been concerned with the past and the present, let us, for the sake of analytical symmetry, speculate briefly about the future. It needs to be considered in two respects: the short run and the long run.

Our best guess is that Japan will not be able to sustain the postwar average pace of some 9 per cent per year for a long time. Some of the things which have stimulated the economy in recent years may lose their force. This is certainly true of the demographic push. It is also hard to visualize investment kept at present levels. The increased capital-output ratio could change entrepreneurial incentives; and once the heavy-industry complex is well established, future technological change may proceed at a more deliberate pace. Catching up—in the short-term sense—will have been accomplished.

We should remember, however, that catching up does have a long-term meaning, and because of this there are good reasons for believing that Japan will continue to grow, perhaps at a more rapid pace than the 4 per cent prewar long-run average. In many ways, Japan is still a semideveloped country. Per capita income levels are still low, the proportion of workers in agriculture is still high, and a dual economy is still very much in evidence. We think that Japan has now entered a second industrial revolution. The first established a modern enclave and a dual economy. The task of the more recent industrial revolution is to abolish the dual economy and to bring about advanced standards of income, consumption, and industrial structure. In our analysis of the postwar years we have identified some signs of the process: shifts in the industrial structure, technological progress, and the importance of productivity gains due to transfer. We believe that this is only the beginning. Japan's path to economic maturity still contains many years of rapid growth.

Needless to say, one cannot forget that ever useful disclaimer: other things being equal. Japan's economic destiny depends very much on the activities of the outside world which we have completely ignored in this analysis. Adverse international economic conditions could easily make shambles out of our optimistic predictions. Our hope is that this will not happen.

THE PLACE OF JAPAN IN THE NETWORK OF WORLD TRADE

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Department of State

I

Japan's rapid rate of economic growth over the past decade has its counterpart, and more, in Japan's external trade. Between 1952 and 1961, when Japan's real product rose at an average annual rate of 9.7 per cent, exports grew at a 14.7 per cent annual rate and imports at 13.1 per cent. In the process the character of Japan's trade and its direction were significantly changed as against the prewar period. With its relative position in world trade now approaching the prewar (1937) level, Japan promises to become increasingly an exporter—and an importer—of a range of sophisticated manufactures. Its trade, similarly, seems likely to expand at least as rapidly with the other industrial nations as with the nonindustrial countries and Japan's more or less traditional markets and customers in Asia.

The expansion of foreign trade obviously was a necessary permissive factor in Japan's growth. Otherwise, the causal relationship surely must have worked in both directions, with the relative magnitudes arguing that domestic expansion fostered exports rather more than the reverse. The present essay will not, however, explore this subject further, since it is not very readily disassociable from the general question of the Japanese growth process. It will seek instead to describe some aspects of Japan's trade expansion, to speculate diffidently about the outlook, and to offer a word on perspectives about the Japanese accomplishment.

II

The growth of Japan's exports over the recent past was accompanied by a diversification and an upgrading of the export list.

Thus, textiles have had a marked proportionate decline among total exports and an upward shift in value per export unit. As late as 1951, the textile category accounted for almost the same share of total exports as in 1934-36, 47 per cent as against 48 per cent (with cotton cloth the dominant item, as was true before the war). Textile exports fell off in 1952 to 35 per cent of the total and thereafter declined gradually over the period to 27 per cent.

Among the textiles, the trend was strongly toward the higher value exports. Raw silk exports declined slightly in volume and rose by 5 per cent in value, while silk fabrics doubled in volume and tripled in value. Cotton fabric exports almost doubled in value with only a 28 per cent increase in volume. "New" textile products—spun rayon and woolen fabrics—grew by five and six times. The value of apparel exports increased five and one-half times. By comparison, the export returns from low-value items such as cotton and rayon filament yarn were up only 16 and 30 per cent, respectively.

The relative change in the role of textile exports reflected the remarkable expansion of exports of other manufactures. These ranged from the products of what we would call heavy industries—led by iron and steel and shipbuilding—to a variety of light manufactures. Some, as for instance stainless steel tableware, were wholly export products. Most appear to have come from industries which were growing in a booming domestic environment, with exports accompanying or following upon an increase in output to meet home demand. In all of this, thinking back to the reputation of Japanese industry for imitation and flimsy products, there was a considerable amount of innovation and of qualitative improvement of output.

Japan's Economic Planning Agency has compiled a list of twenty-four major "new" exports, defined as those which came to amount to 0.2 per cent or more of total exports between 1951 and 1961. Exports in this grouping earned $15.5 million in 1951 and $536 million (13 per cent of total exports) in 1961. The Planning Agency finds that twelve products first entered the category in 1960 and 1961, among them such items as tape recorders, stereophonic equipment, vacuum tubes, and transistors.

Expenditures on industrial research in Japan obviously are not large enough to bring about technological development at an American rate. Japanese industry has bought rights to foreign technology with some selectivity, however, and has done a good deal of independent adaptation and innovation on its own part. In shipbuilding, Japanese yards have pioneered, among a number of other things, in the super-tanker field (a recent launching featured a 132,000 ton tanker). Japanese transistor radios, exports of which rose from $200 thousand in 1953 to $151 million in 1961, can fairly be said to be a home-developed product, based on technology licensed from the United States. The Japanese camera industry has contributed its share to the development of machines so automatic that the rankest amateurs can use them and only specially trained craftsmen service them. (There is no good, cheap Japanese-produced box camera on the home market, incidentally.) A Japanese firm is now producing a truly portable television set, capable of giving an amazingly sharp picture on a five-inch screen.

It is the first of its kind to be marketed anywhere, and its sales are rocketing in Japan and abroad.

The post-1952 change in the commodity composition of exports went along with a significant shift in the direction of Japanese trade.

During the 1934-36 index period, Japan sold 64 per cent of its exports to Asia. Captive markets in Korea and Taiwan accounted for 25 per cent of all exports and Mainland China for another 18 per cent. The United States market took 16 per cent and Europe 8 per cent.

The change has been away from the Far East, which took 52 per cent of Japan's exports in 1952 and 37 per cent in 1961. Korea and Taiwan together provided less than 5.5 per cent of Japan's export market and Mainland China was a negligible factor. The United States bought almost 25 per cent of the total, and Europe's share, despite persistent and effective discrimination against Japan's exports in most European markets, was up to 14 per cent. Latin America and Africa together were at 15 per cent (10 per cent in 1934-36) and Oceania was unchanged from prewar at 3.5 per cent.

In the circumstances of the fifties, Japan could scarcely have expanded its exports to the slow-growing nearby Asian markets at the rate that was achieved over-all. Actually, the value of exports to Asia more than doubled in absolute terms, while exports to the United States increased four and one-half times and to Western Europe almost three times. Japan's share of Asian imports rose sharply, from 6.7 per cent in 1953 to 11.9 per cent in 1961. Sales of steel, industrial machinery, ships, and automotive products contributed most heavily to the expansion of Japan's exports to Asia, perhaps suggesting what the possibilities might be if the nonindustrial Asian countries were to manage to achieve a period of substantial industrial growth.

III

Throughout the postwar years Japan's import trade has been managed by the government via foreign exchange allocations. The overriding objective has been to allow for imports of raw materials and foods adequate to the nation's needs and for such additional items as have been considered essential to the functioning and growth of the economy. Only during the latter part of the period, beginning in 1959-60, were imports outside of the essential categories liberalized, and then to a distinctly limited extent.

Despite the rigors of the exchange allocation system, it is not certain that the import pattern that was allowed was radically different than would have been the case under a less strict regime. Given the domestic boom and considering Japan's absolute or near-absolute dependence on a number of foreign raw materials, importers presumably would have bid in the supplies necessary for industrial needs in any event. At the other end of the import spectrum, that is, nonfood consumer goods, it is probable that prevailing income levels would have kept imports within narrow bounds even without severe restrictive measures. Controls on capital goods imports may have been more onerous or at least more stringent in their effect. According to Planning Agency estimates, the ratio of machinery imports to total investment in fixed equipment has been lower in Japan than in any industrial country except the United States. On this showing, the Tokyo bureaucracy must have made a good many decisions about the relative merits of competing applications for machinery import licenses—a consideration that makes the pace of Japan's industrial expansion all the more astonishing.

A striking change in Japan's import pattern has been the decline in imports of rice. By 1961, rice imports had fallen from a 1954 high of 1,432,000 metric tons ($251 million) to a level of 126,000 tons (and $16.6 million). In 1954 rice imports were next to raw cotton in value. The decline thereafter was steady and, so far as the near future is concerned, presumably will not be reversed. For Japan since 1955 has had an unbroken succession of "bumper" rice crops, and these probably will continue so long as the domestic support price is set at a figure high enough to satisfy the rural voter.

The impact of Japan's expanding rice crops fell particularly upon the exports of Burma, Thailand, and Taiwan, among Japan's Asian suppliers. Interestingly, both Taiwan and Thailand were able partially to replace rice with other exports. Taiwan has even become an exporter to Japan of manufactured goods from its growing urban sector, while Thailand has become a supplier of feed grain, a new and increasing import for Japan's expanding dairy and livestock industries.

Japan's Asian neighbors did not prosper, on the whole, from the Japanese post-1952 boom. In 1952, Asian countries, including India and Pakistan, supplied 27 per cent of Japan's imports. By 1961 the figure was below 21 per cent. Of the important suppliers, only Malaya and the Philippines registered increases in value of sales to Japan at a rate comparable to Japan's threefold expansion of imports, and Burma suffered a very large absolute reduction.

In some part, this was a consequence of the slide downhill of raw materials and food prices. Japan's import price index, heavily weighted by foodstuffs and raw materials, was down to 75 in 1961, taking 1952 as equal to 100. (The export price index was also down, to 88.1, leaving a handsome improvement in Japan's terms of trade, a matter of importance to the Japanese economic recovery.)

Asian suppliers also were affected by the failure of Japan's imported raw material requirements to rise as fast as industrial output and, of course, by the decline in Japan's needs for imports of food.

"Import dependency" is a phenomenon of great interest to Japanese officials, the course of which has given them much satisfaction, thanks in part to the fall in import prices. The value of all imports as a proportion of gross product decreased over the 1952-61 period. Exchange expenditures on food were lower in 1960 than in 1952 and were up by only 9 per cent over 1952 in the high import year of 1961. The value of imports of primary raw materials, excluding petroleum, increased less rapidly than industrial output and at about the same rate as GNP over the period. Petroleum imports grew in proportion to the expansion of industrial production but, against the trend and in spite of a 40 per cent decline in exchange outlays per unit of imports, about half again as fast as GNP.

IV

Looking ahead to 1970, Japan's planners have built a "double-national income" model which calls for export growth at an average anual rate of 10 per cent per year, with the value of imports increasing at an annual 9.3 per cent. The model shows a continuing shift of exports toward "heavy and chemical" industry products and away from textiles, foods, and "light" machinery. Raw material imports increase slowly, fuel imports much more rapidly, and manufactured goods at a faster rate still.

The modelmakers' export goal, while ambitious, does not appear to be beyond Japan's reach. During the fifties Japan's exports grew at a rate more than twice as fast as the rate of growth of world trade. Even after allowing for the low point at which Japan began, this record accurately suggests that the country's competitive export position was unusually strong. And if the volume of world trade were to grow more rapidly in the sixties than in the fifties, Japan's export prospects would be very good.

At the heart of Japan's export success in the fifties was a consistently favorable cost-price situation,[1] and on a relative basis at least this might well obtain through the present decade. As we have seen, the export goods imports probably will increase slowly, mainly because of a reversal of the long decline in the volume of food purchases from abroad. Japan's comparative disadvantage in agricultural production can only worsen as labor continues to drift away from the farms and as agricultural wages rise. At the same time, the changing Japanese diet calls for items which Japanese agriculture cannot provide with any semblance of efficiency. Sugar is a small but instructive case in point, this being a product on which Japan's protective regime approaches the absurd and for which Japanese consumers pay a retail price that must be the highest in the non-Communist world. The consumption of dairy products and meats, oil seeds, cereals other than rice, tropical fruits, and coffee easily could increase at a rate faster than total imports. In this area lies the promise for an expanding complementary relationship with Southeast Asia and with Oceania.

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[1] The role of official "aids" to exports in Japan has, I believe, been exaggerated. Japan, as is true of other exporting nations, provides special financing and insurance facilities for exporters. Similarly, in company with the United States and the European industrial countries, it goes in for a variety of governmentally-sponsored trade promotion measures. There are several taxation measures that benefit exporters, the chief of which is the remission of 80 per cent of the corporate income tax for income attributable to exports. Robert Aliber, of the Committee for Economic Development, tells me that total deductions on this score from the corporate tax were about $28 million in 1961. Total exports in 1961 were more than $4 billion. In all, these aids do not add up to extraordinary incentives to exports.

V

Japan's economic performance over the recent past may not justify the term "miracle" but anyway it has been out of the ordinary. Consider the recovery from last year's scare over the balance of payments. In 1961, trade account ran $591 million against Japan, current account roughly $1 billion. Reserves, after borrowings, fell $559 million.

A general tightening of money and credit was undertaken in the summer and early fall of 1961. By January, 1962, imports had begun to fall off and exports to rise. Through September, exports were up 19 per cent and imports down 7 per cent. Trade with the United States was running nearly in balance, after a 1961 Japanese deficit of almost $700 million, with exports up to an annual rate of almost $1.4 billion, as against $1 billion in 1961. Reserves in October were $319 million above the first of the year figure. And the dismantlement of the tight money policy was begun as early as September.

This performance was closely paralleled in 1957, when the domestic boom, plus the impact of Suez, led to a near-disastrous fall in the exchange reserves. In that year it took just five months to bring the current account into the black and in less than a year the extraordinary 1958-61 expansion was under way.

These episodes suggest a remarkably resilient and responsive economy, and this I think is the case with Japan. Given a moderately favorable world economic climate, there is little reason to doubt that the Japanese will continue for some time to prosper, at home and in external trade. Now that the attention of foreign economists has been drawn to the remarkable phenomenon that Japan represents, it may be that we shall have something to learn from it.

But perspective is still useful, and this applies to Japan's foreign trade as well as to other aspects of the Japanese economy. Japan has again become a factor in international trade and is the seventh largest exporter; yet it accounts for less than 4 per cent of world exports, a lower proportion than before the war. If world trade continues to grow at the rates that prevailed during the 1950-60 period and if Japan reaches its "planned" goals, Japanese exports in 1970 will reach about 4.8 per cent of the world total. To speak of a prospective "yen shortage," even in a deprecatory sense as the *Economist* did recently, is to exaggerate immensely the present or possible productive capacity of Japan.

Actually, the rate of growth of Japan's exports has been only slightly greater than West Germany's over the 1952-61 period and, for the more recent 1957-61 period, somewhat below Italy's and just a bit higher than that of France. West Germany in 1961 exported $8.5 billion more than Japan, the United Kingdom $6.5 billion, and France $3 billion. Canada is a larger exporter and the Netherlands and Italy run neck and neck with Japan. In nearly all of these countries, as is true of most other advanced nations, exports form a larger, usually a much larger, share of GNP than in Japan.

It is possible to be skeptical, also, about the extent to which the foreign trade recovery was the result of a planned effort. The Japanese government has of course exercised broad controls over imports and less comprehensive controls over exports throughout the postwar period. The effect of these was at best mixed. Import controls have involved forfeiting some of the potential gains from favorable terms of trade (particularly in cereals but in other commodities as well) and they have been used to protect excessively the less efficient industrial sectors in Japan. They may have served in some instances to direct resources to export industries, but if so this was mainly incidental to the basic objective of limiting exchange expenditures. Government controls over exports have been almost exclusively in response to pressures from abroad for the "orderly" marketing of Japanese commodities. Export incentives were present but not to an unusual degree and in any case they were applied broadside rather than to specific export industries.

The truth is that Japan's exports were no more consciously planned than was domestic growth. Private enterprise, despite rather than because of governmental constraints, was the activating force in the total Japanese scheme of things, and it was a very lively force indeed. In its energetic pursuit of profit goals, it brought the economy sharply forward, not only in volume of output, but also in kind. This was so in export industries as in the domestic economy.

As a result, Japan is now much more than before the war a comrelated and relatively optimistic ideas with you: First, the Japanese economic miracle, far from contradicting "modern economic growth theory," makes an excellent case in point. Second, the boom can, although it need not, continue indefinitely, with the accumulation of excess capacity in the private sector playing a role Keynes assigned to "pyramid-building" in the public one.

1. An important branch of modern growth theory (for advanced countries) is represented by the Harrod-Domar model. This model embodies precisely the "knife-edge" instability best adapted to explaining the long Japanese boom. Let us consider the Domar variant, which has the merit of avoiding undue reliance on the acceleration principle. Here, most of us remember, the key factor on the expansive side is the income-creating effect of investment (Domar's α-effect, which is greater the lower is α, since α is the marginal propensity to save; the simplest Keynesian multiplier is $1/\alpha$). The key factor on the side of contraction is the output- and capacity-creating effect of investment (Domar's σ-effect, which is greater the larger is the incremental output-capital ratio σ). So long as the α-outweighs the σ-effect, the growth of investment and income is not only above its smooth exponential equilibrium path but tends to rise further above it, because a shortage of final products lowers α in the short run. This seems to put the Japanese disequilibrium case in a nutshell. (Domar, in the shadow of the American postwar depression that never came off, used contractionary disequilibrium in his illustrations; so that his model has been unfortunately associated one-sidedly with its "gloom and doom" aspect.)

2. Can upward disequilibrium be sustained? The Ohkawa-Rosovsky paper, and likewise the Shinohara book, suggests that it cannot. Accumulated excess capacity is eventually reflected in higher consumer inventories and higher values of α. Investment and income growth rates fall to or below the equilibrium path. If they fall below it, as usually happens, the contractionary or stagnation variety of disequilibrium comes into play.

Japan's "guided economy" or "new industrial order," however, is becoming increasingly safe for excess capacity. (Possibly the same is true for some Western European economies, especially the French.) When excess capacity develops, producers maintain or raise prices. When they do not reduce output, they do not increase it in line with capacity. Output is regulated and allocated between firms by cartels and/or by the Ministry of International Trade and Industry (MITI), largely on the basis of crude measures of capacity. Increase or even maintenance of market share requires increase of capacity—operative or excess capacity, it makes no difference. Increase of excess capacity is private leaf-raking and pyramid-building with a vengeance, and open to obvious objections on welfare grounds. But it is private, and therefore both "practical" and "sound," and it does maintain both employment and the growth rate.

It is not enough, we agree, to rely on the Harrod-Domar model to make the Japanese economy safe for private pyramid-building. This model assumes unlimited supplies of all types of labor and ignores the complications of international trade. Japan is developing shortages of particular skills in increasing numbers and has never dared ignore her balance of payments. At the risk of expanding unduly the scope of this comment, let me, however, suggest that postwar Japan can, if she wishes, avoid both sets of complications. Labor shortages are avoidable by diverting Japanese youth from overcrowded academic education to less developed vocational training, avoiding the trade-union restrictionism and irrelevant standard raising that plagues similar Western programs of subsidized human capital investment. As for Japan's recurrent balance-of-payments problem, we cannot yet estimate its severity, above and beyond European and American discrimination against Japanese exports. If it should amount to something more or if liberalization of restrictions is not forthcoming, Japanese reaction may take the form of strengthening Japan's own illiberal institutions, now concentrated in quasi-dumping of exports via the tax system. I have argued elsewhere ("Thoughts on the Yen-Dollar Exchange Rate," *Keizai Kenkyū,* April, 1959) for a quasi-dual currency system via exchange taxes (on imports) or subsidies (on exports) as possible next steps and will not elaborate that argument here. Suffice it to conclude that, granting the oversimplifications of the Domar-Harrod model, it not only applies to contemporary Japan but suggests indefinite extension of the boom through the private pyramid-building of excess capacity. Labor and foreign exchange shortages, along with conventional welfare considerations, will make such extension more difficult in the real world than on paper, but I suspect economic historians may be premature in concluding that extension is impossible merely because it lacks historical precedents.

WARREN S. HUNSBERGER: Mr. Trezise's excellent paper offers few opportunities for quarrel, but many points that could be fruitfully discussed at length. I might attempt to fill in some specific gaps, such as the contribution to rice self-sufficiency of the substantial shift that has occurred in Japanese eating habits, from rice to wheat, meat and dairy products. Or I might dwell on Japan's propensity to import, and express my view that imports of consumer manufactured goods would, if permitted, have been much larger in the past and will, again if permitted, rise in the coming years much faster than either the Japanese projection or Trezise suggests. But I think it most valuable to discuss some aspects of commercial policy that I believe have been crucial to Japan's recent outstanding economic success.

In commercial policy, especially American, lies a large part of the explanation for Japan's rapid export growth in recent years. To be sure, Japan's domestic economic performance, especially in savings and investment, is properly regarded as the engine driving the economy forward and upward. I agree with Ohkawa and Rosovsky that these domestic factors are more basic than the external factors on which Japan has run into extraordinary good luck. But without these external factors, Japan's record since 1945 or 1952 would have been different. As Trezise well says, expansion of foreign trade was a necessary permissive factor in economic growth.

If there was any validity to prewar complaints in Japan that the country had inadequate opportunity and required control over neighboring areas, this validity lay in the area of commercial policy. In his standard work on Japanese economic development, our chairman today stated:

> The Japanese have complained bitterly, and with reason, concerning the high tariff walls erected in prewar years against Japanese manufactures abroad. In the United States, for example, as quickly as cheap manufactures from Japan offered any competitive threat to even a small segment of an American industry, they were apt to be shut off promptly. (William W. Lockwood, *The Economic Development of Japan*, Princeton University Press, 1954, page 542.)

That has not been true in the postwar years, despite domestic producers' opposition to Japanese competition in the United States and other countries. American tariffs and other import barriers have not prevented Japanese goods—mostly competitive with domestic products—from flowing into this country in a volume that breaks all previous records for imports of manufactures, and this year is expected to reach about $1.4 billion. Nor have tariffs, quotas, exchange controls, and all the other forms of import interference, despite the high degree to which such devices have been perfected in the past three decades, prevented Japanese sales from soaring in other areas, as Trezise has well reported.

Japan's record in the American market has even more to it than Trezise has shown. In 1952 imports into the United States from Japan were $229 million. In 1961, the figure was $1,055 million. In 1952 Japan accounted for 2.1 per cent of our total imports; in 1961, 7.2 per cent. Japan is now second as a supplier of U.S. imports, following Canada, whose products are far different from the competitive goods Japan sells here. Sales in the United States constituted 23.6 per cent of total Japanese exports in this ten-year period, 1952-61 (at times nearly 30 per cent).

This record has not been achieved without a struggle, or without exceptions, some of them serious. Cotton textiles, woolen textiles, men's suits, ladies' blouses, other articles of clothing, plywood, tuna fish, and many other Japanese products have been objects of complaint by American competitors. Aside from the latest dumping charges against Japan by the United States Steel Company and earlier actions, including a patent infringement charge by the Singer Sewing Machine people, Japan has been the foreign country involved in more escape-clause applications than any other. The Tariff Commission's reports show that, in a total of 131 escape-clause cases from the start in 1947 through June, 1962, Japan was prominently involved in 24 cases dealing with twenty-two commodities. In 10 of these 24 cases the Tariff Commission found actual or threatened injury to American industry and recommended removal or modification of tariff concessions affecting Japan. The President acted to restrict imports in only 5 cases, dealing with stainless steel table flatware, clinical thermometers, Wilton rugs, cotton typewriter ribbon cloth, and sheet glass. While the economist might well object to some or all of these five restrictive actions, their effect on trade has been very minor indeed. In 1960, the year when the third of these actions took place (on September 22) and before the fourth and fifth actions, Japanese sales in the United States of the five products concerned came to only $22 million. Total U.S. imports from Japan in that year were $1,149 million.

Perhaps even more significant is the Tariff Commission's action in two separate cotton textile cases that did not involve the escape clause. In June, 1960, and again in September, 1962, the Commission decided that imports of cotton goods were not interfering with U.S. agricultural programs. The proposal had been made in the second case to impose a fee on imported cotton products so as to raise the cost of their cotton content from the world price up to the supported U.S. price.

Far more serious than formal actions by the United States to restrict imports from Japan have been the export restraints imposed by the Japanese government or by business groups in Japan. These have usually been called voluntary, although a number have been imposed only after discussions (even extended hard bargaining) between American and Japanese. What is worse for liberal trading principles, voluntary or negotiated export quotas are spreading. For instance, the new treaty of Commerce, Establishment and Navigation between the United Kingdom and Japan lists in an attached Japanese note thirteen items, several with subitems specified, on which Japan after "consultation" decided to impose restraints on exports to British territories covered by the treaty.

The most obvious, restrictive and potentially dangerous use of the quota device has occurred in cotton textiles. This device in its postwar form was invented in discussions between the United States and Japan in 1956. There had been precedents before World War II, most notably an industry-to-industry agreement in January, 1937, for Japanese export quotas on cotton textile shipments to the United States. The postwar quotas were Japanese government actions, allegedly voluntary when announced in January, 1957. These had far more commercial significance than the privately negotiated quotas of twenty years earlier, since rising tension related to the approaching Pacific war had cut the volume of Japanese goods that American customers would buy. In 1957, on the other hand, the American market was ready for a rising volume of quality cotton products from abroad. When the Japanese quotas kept our purchases from Japan below the 1956 level, American buyers simply went to Hong Kong and elsewhere. Imports into the U.S. from other areas expanded rapidly.

In 1961 the United States initiated steps to check this new inflow. Our representatives persuaded the leading countries concerned to permit quota limitations on international trade in cotton manufactures. In October, 1962, a five-year, Long-Term Arrangement went into effect at the expiration of this one-year, Short-Term Arrangement. Now, ironically under GATT auspices, importing countries may ask exporting countries to restrain exports of cotton manufactures, or, lacking satisfaction by this route, may impose import quotas.

These quota arrangements, especially those in cotton textiles, sit very poorly with many economists. They impede the necessary growth of world trade and thus far have restricted Japanese exports more severely than those of any other country. Those arrangements represent government concessions to the special interests of this industry, especially in the United States, but also in other countries where cotton textile imports have raised political problems. The United States has taken a backward step into quantitative controls on international trade in a major manufactured product. Cotton manufactures are still a major export for Japan and are potentially important exports for newly developing countries, as, one by one, they develop the capability to produce at a competitive price at least one acceptable product among the large range of products made from cotton.

Nevertheless, the Long-Term Arrangement is not wholly black. An impressive series of provisos limit the circumstances under which a quota may be demanded or imposed by an importing country and the minimum permissible quota level. No export quota may be demanded by an importing country, nor import quota imposed, unless there has been market disruption as defined by GATT. (Unfortunately, the importing country is sole arbiter on this point; as one might guess, the Japanese strongly opposed this provision.) The quota level may not be lower than actual movements during a stipulated period (the first twelve months of the fifteen-month period immediately before the importing country asks the exporting country to consult on the matter). A most encouraging feature is the compulsory escalation of quotas that remain in force. If during the Long-Term Arrangement a quota stays in effect for a second year, its level should rise, if possible. If it stays in effect beyond two years, the level must rise by at least 5 per cent a year.

The result is that trade expansion is likely, even for those cotton textile products subject to quotas. There remains, to be sure, wide latitude for the United States and other importing countries to abuse their right to declare unilaterally that market disruption has occurred. There is also room for highly restrictive quotas on other products than cotton manufactures to be negotiated bilaterally or to be imposed in panic by Japan or other exporting countries. But thus far the dangers and difficulties, even including the quota limitations on cotton goods and other products, are essentially exceptions in a situation where the general circumstances and the forces at work suggest trade expansion. The task for American policy is to make sure these remain only exceptions, to keep them limited and to bring about their progressive elimination. With the Trade Expansion Act of 1962 as legislative authority, Mr. Trezise and other officials of our government have an unusually favorable opportunity for doing so.

In Europe, too, Japan ought to be able to find a rapidly expanding market during the sixties. It is by no means yet clear what will happen in the European Economic Community, or in many other places. But Japan's phenomenal success in the American market has shown that the economic and political difficulties of a swelling inflow of competitive products need not be devastating. With vigorous economic growth in an industrial area and with selective restraint a part of Japanese export policy, there should be room in Europe for rapid growth in Japanese sales there. Reports from France and Germany during the recent visit of Prime Minister Ikeda are hopeful.

Given continued liberality in U.S. trade policy, given at least some relaxation of restrictions in Europe against Japanese goods, and given at least a moderately vigorous growth of world trade, Japan's trade prospects look bright. Without stopping to examine more of the elements in this complex picture than have been looked at in this discussion, I close by joining Trezise and the Japanese planners in judging that the projected annual increase of 10 per cent in total Japanese exports and the somewhat faster projected growth in exports to the United States seem quite within the realm of practical probability.

Hugh T. Patrick: The framework of analysis of the Ohkawa-Rosovsky paper of growth of real output in terms of increases in the labor force, transfers of labor from low to higher productivity sectors, and increases in sectoral output per worker, while in many ways illuminating, does also contain pitfalls. (I shall follow the authors' shorthand of using "productivity" for "increase in output per worker.") In the first place, this approach attributes all increases in output to labor, involving the same oversimplifications that are frequently manifested in the use of the capital-output ratio. For example, included are not only aspects which directly increase the productivity of labor, such as more capital per worker, but such factors as increased gains from trade from improved commodity terms of trade and from increased specialization, both important in the 1953-60 period for Japan.

Second, the transfer effect implies either that the economy is in disequilibrium or that there are substantial imperfections in factor markets. Imperfections in the labor market, even more so than in the capital market, are indeed important in the Japanese case. The measurement of the transfer effect is not easy, since in its purest form it requires estimates of the marginal productivities of the workers in the two sectors within which they shift. While the authors did not have time to explain their estimation procedures, one suspects that they used average rather than marginal productivity data. This requires the assumption that in any sector marginal productivity equals average productivity, or at least that the absolute difference in marginal productivities between sectors equals the difference between average productivities.

A third problem lies in the measurement of the sectoral increases in labor productivity, mainly as regards the services sector. There is first the general difficulty that where the market mechanism does not provide a good measure of output—for example, government services—output is standardly measured by the cost of inputs. Moreover, price index problems are likely to arise, because frequently one has to deflate the output series by some very general price index. I suspect that the nature of the data forced Ohkawa and Rosovsky into such a procedure. Where the spill-over of rising wages in manufacturing results in wage increases not compensated fully by productivity increases and thereby higher relative prices in the services sector, this could lead to a serious overestimation of productivity increases in the service sector. These problems of adequate price deflators and changes in relative prices of output impinge also upon the measurement of the transfer effect.

Turning to matters of substance, the historical perspective provided in this paper by comparing the 1953-60 period with earlier periods of very rapid growth is extremely useful. I was particularly struck by the constancy of aggregate labor productivity among these different periods, which I gather turns out to be essentially a coincidence, as is indicated in the sectoral data the authors present in Table 3.

As Ohkawa and Rosovsky indicate, their analytical framework does not explain for the postwar period two-thirds of the increase in output; i.e., that part termed "productivity proper." I certainly agree with the emphasis placed on the role of tremendous increases in productive investment, both in absolute terms and as a proportion of gross national expenditures, as the major determinant of increased labor productivity. I shall skirt discussing the crucial question of how and why this essentially private capital formation was generated, since it is a topic requiring more time than any of us has available today. The increased investment, with rising profit rates, has done much to generate its own demand, in consumer goods out of wages paid but even more in the producers goods sector, as reflected in the changing industrial structure. The high investment rate also allowed more rapid embodiment of the latest (and most productive) technology. Also, the laborsaving nature of investment and its associated technology has been even greater than indicated by the aggregative data, since much of the investment has been carried out by large-scale enterprise, while much of the increase in labor in the manufacturing sector has been absorbed into small firms. For example, the iron and steel industry, with its level of plant and equipment investment rising nine times between 1955-60, about three times as rapidly as that of all production, had an increase in regular employment of only 56 per cent, less than the 65 per cent increase in employment in all manufacturing. This would suggest that while the average productivity of labor in manufacturing may be rising rapidly, the marginal productivities of labor between large and small enterprises within the manufacturing sector has been widening rather than narrowing. Not only does this affect the measurement of the transfer effect, but more substantially, with greater difficulties of obtaining labor in the future and with rising wages, in order to survive small enterprises will be under even greater pressure to raise labor productivity.

While I agree that Japan has had an abundant supply of educated labor, I would not say that there has been no pressure on the wage side. After all, real wages in manufacturing, reflected in an average increase in money wages of 6.4 per cent annually between 1952 and 1960, rose more rapidly in Japan than in any other industrial nation. Rather, labor productivity has been rising even more rapidly; so that it has been easy for employers to grant substantial wage increases, more as a result of market, union, and other pressures than their own beneficence. Thus, I would place more importance on the productivity side of the unit labor cost coin in projecting to what extent the net deflationary effect of the size of wage increases will continue.

One final comment. I, too, am doubtful that the 9 per cent rate of growth can be continued indefinitely, but also optimistically expect that it will remain substantially above the 4 per cent historical rate for the foreseeable future. However, I am somewhat mistrustful of the Ohkawa-Rosovsky analysis of future growth in the broad historical terms of catching up—the transformation of a semideveloped economy into a mature one. In the first place, I am not quite certain what a mature economy is. More important, there is the implicit assumption that a mature economy grows more slowly. Perhaps it does—in that it must depend upon expansion of the technological frontier rather than adapting technology previously developed elsewhere—but it is not the inevitable or necessary case. Perhaps the obvious lesson for the United States from recent Japanese experience is that if an industrial nation wants rapid economic growth, it, too, has to devote a high proportion of its expenditures to plant and equipment investment, education, and research.

ECONOMIC DEVELOPMENT AND THE POPULATION PROBLEM

POPULATION PROBLEMS AND EUROPEAN ECONOMIC DEVELOPMENT IN THE LATE EIGHTEENTH AND NINETEENTH CENTURIES

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The period 1750-1850 saw sustained population increase in all parts of Europe—sustained in the sense that there was never a fall in absolute numbers. In many areas the rates were as high as 1 per cent per annum. For all we know, there had earlier been long periods of growth at comparable rates, but the absolute numbers were much greater in this period. We do not know enough about the century before 1750 to know how far growth was accelerating after 1750. It may be that the high rates for Russia, for example, and for parts of Germany, were of long standing, but at least in some areas there was an acceleration: in England, Wales, and Ireland, in Switzerland, and probably in West Germany.

What relation did this increase in population bear to the economic changes of the period? Was the increase fortuitous from an economic point of view? Or is it to be regarded as a response to the economic changes, and if so to which? In particular, was it a response to rising per capita incomes and greater employment opportunities? And, whether fortuitous in origin or not, was the increase in population favorable or unfavorable to economic growth and particularly to the growth of per capita income? From the point of view of current underdeveloped countries the most interesting question is how and why the increase in European population ultimately proved compatible with a sustained increase in per capita incomes. Assuming there was a population barrier to economic growth, how was it broken in the European case?

I

Population change is determined by a wide variety of influences, cultural and social as well as economic, and these have combined in many different ways at different times and places, even within the same neighborhood. Even were the evidence less fragile, it would be wildly misleading to generalize about patterns of change over a long period and a wide area. In particular, it is difficult to disentangle those influences which were exerted over a wide area within a given time period—for example, common influences on mortality—and those which characterized a particular phase of economic development, say the early stages of industrialization. Given the fragility of the evidence before 1850, the most appropriate treatment would be a detailed weighing of the possibilities region by region and decade by decade. Since this is impossible within the confines of a short paper, the best alternative may be to describe what I believe were the central lines of development more categorically than is warranted by the evidence or indeed by my own views on the matter. I shall deal first with the origins of the population increase and then consider its effects.

So far as origins are concerned, the traditional mechanisms by which population growth was kept in line with resources was capable of producing an increase of population over several decades even in the absence of any long-term change in the capacity of the area to support population. This mechanism—to put it schematically—rested on dual controls: the Malthusian preventive and positive checks. In Europe, from very early times, marriage involved the setting up of a separate household and the care of the children fell upon the husband and wife as opposed to some wider, kinship group. Thus people tended not to marry until they could expect to maintain a family at the standard of living to which they were accustomed. This prudence was reinforced in a society of peasants and artisans, where the son had to wait until his father's death before succeeding to his holding or his calling, but it existed in some degree without this institutional support. By this means marriage—the "age at marriage" and the proportion of "ever married"—was adjusted to the resources of an area. So much for the preventive checks. Where, for whatever reason, marriage habits proved inflexible in face of a change in resources in relation to population, the positive checks came into play and population was brought into line with resources by means of variations in deaths. Mortality rose, and this cut the population back to size both directly by the increase in deaths and indirectly by terminating marriages and so reducing births.

That some such mechanism as this did work in Europe before the demographic revolution is I think clear, though there were great variations in the balance between the preventive and the positive checks at different times and places. And if it had worked smoothly and swiftly, population would have grown at a rate determined by the independent growth of resources. But it is evident from the very nature of the mechanism that even if the adjustments required had been moderate and gradual, the response might have been imperfect. And in fact the adjustments the mechanism was called upon to make were often abrupt and violent. For it was a characteristic of earlier centuries that there were great short-term changes in both resources and population: on the side of resources there were fluctuations in harvest yields due to variations in the weather; on the side of population there were variations in mortality due to epidemics and to fluctuations in the severity of the endemic diseases, the last of these probably also in some measure a result of weather. These were, in the present context, fortuitous factors and according as the accidents fell out population growth might, for a period, be slower or faster than the rate of growth warranted by the long-term resource position.

It is the second case which is of present interest, where population growth was faster. By way of example I take two instances from the history of the period. A long run of favorable harvests, particularly if they coincided with other conditions favorable to low mortality, could produce an unusually large population. This seems to have happened in Sweden between 1721 and 1735, and the increase in population in this period had an echo effect in the 1750's when, in addition, because economic conditions were temporarily favorable, the age-specific marriage and birth rates were particularly high.

But a population surge could also arise as a reaction to a period of exceptionally severe mortality. When the causes of high mortality had ceased, there was a bulge of births. In the aftermath of the deadly years, the number of women who were sterile from recent pregnancy was unusually small; and marriages deferred in the bad years were now contracted. But, in addition, many young people were now able to marry earlier than had been customary, because they succeeded to holdings or occupations at an earlier age. The stock of marriages, depleted by high mortality, was now replenished by new and therefore more fertile marriages. The bulge in births arising from these causes produced fluctuations in age composition, a wavelike motion as the bulge moved forward in time. A generation or so after such a bulge, the population contained an abnormally large proportion of young adults. Now if, when the bulge came of age, conditions happened to be favorable to earlier marriage—because trade was good or harvests abundant—the coincidence of a favorable age composition and earlier marriage was capable of producing another and more considerable surge of births.

In any area where prudential restraints had maintained a low density of population or where the conventional standard of living by reference to which marriage habits were determined was high in relation to the physiological minimum, such fortuitous population surges could proceed for a considerable period at the expense of fragmentation of holdings and declining living standards, without provoking the positive checks.

Some part of the population increase in most areas can be attributed to the operation of what can broadly be termed the traditional mechanism of population change, without involving the appearance of any new influences. Insofar as this was the case, the increase can be regarded as an independent variable.

But why, it may be asked, should the fortuitous influences in this period have had the net effect of stimulating population growth over a wide area. Briefly, the answer is twofold. First, over large parts of northern Europe the late eighteenth century was freer from epidemics than the early decades and there was some reduction in the virulence of certain endemic diseases. And second, in the circumstances of the period, a fortuitous increase in population was more likely than at an earlier date to stimulate an increase in resources; this is a point which will be considered in more detail in the section on the effects of population increase.

But this is clearly not the whole explanation, for the population continued to increase into the nineteenth century, and, moreover, the rates of increase for each country no doubt disguise the revolutionary nature of the process; an increase of 1 per cent for a country as a whole probably covers areas where the increase was unprecedentedly rapid matched by areas of negligible increase. Some new factors were evidently at work. The question is how far they were fortuitous in our sense of the word.

Institutional changes imposed by the state clearly fall into this category: for example, the emancipation of the serfs in Germany, which is commonly held by German historical demographers to have relaxed restraints on marriage. So is the imposition, in the Napoleonic Civil Code, of a law of inheritance which, in the Upper Rhine at least, is believed to have increased marriages.

An improvement in medical knowledge might also be ranked as fortuitous from this point of view. But the general consensus of opinion seems now to be that, except for innoculation and vaccination, there were no additions to knowledge which had a significant effect on mortality during the century 1750-1850, and so the point need not be investigated further here.

Among the new influences of an economic nature operating on population change the one which calls most obviously for examination is industrialization. This might have three types of effect. In the first place, it increased the demand for labor and by raising wages and/or making it easier to find employment at a given wage it encouraged marriage and possibly also lowered mortality among wage-earning families. It was this type of influence which Malthus had in mind when he wrote that employment regulates "the wages of labour, on which the power of the lower classes to procure food depends; and according as the employment of the country is increasing, whether slowly or rapidly, these wages will be such as either to check or encourage early marriages, such as to enable a labourer to support only two or three or as many as five or six children." But industrialization also influenced population behavior by changing the social structure; an increase in marriage could take place, not only as a result of a change of habits within existing social groups, but as a result of a shift in the distribution of the population from social groups which customarily married late and infrequently to those where the age at marriage was lower and its incidence more frequent. Both these effects undoubtedly occurred, and in either case (but particularly in the former) one could reasonably consider the increase in population as, in some sense, called forth by contemporary industrial changes.

There are, however, some points to be noted about the effects of industrialization on population growth. In the first place, before the 1850's it is only in England that industrialization was rapid or extensive enough to have influenced the population behavior of large numbers of people. Even in England it is only after 1780 that we can reasonably suppose these influences to have been powerful; and population had already been increasing quite rapidly for a number of decades. And insofar as the increased demands for labor for industrialization were exerted in a population which was already growing for fortuitous reasons, their force would be by so much abated. Moreover, even in England in the early decades at the nineteenth century when, as is evident from the work of Dr. Krause, industrialization was a powerful stimulus to the growth of population, the response of population to the economic changes was, in some sense, excessive. For industrialization not only increased the demand for labor and redistributed population between social groups; it put the whole mechanism whereby population growth was linked to resources via marriage under pressure. Marriage habits were an effective control on population only with a certain institutional support and at a certain standard of living, and the social changes of the early nineteenth century—especially the break-up of village society, migration to the towns, and employment in the factories—put old habits under a strain they could not bear. Thus to some extent the increase of population was due to social disorganization.

In the rest of Europe, if we want to explore the possibility that population growth was a response to economic changes, we must look for changes other than industrialization. There are two principal possibilities. The first is an increase, independent of population increase, in the productivity of agriculture, due to technical progress; i.e., new rotations, the use of root crops and artificial grasses, or the increased use of the potato. Such an increase in agricultural productivity might stimulate marriage (as it is believed to have done in Ireland) and/or lower mortality by improving nutrition. In most parts of Europe, however, there were strong institutional obstacles to the introduction of the new techniques which were removed only under the pressure of population. It may well be, of course, that as the result of independent changes in agrarian structure or of technical progress, agricultural output was more responsive to a given pressure of population in the late eighteenth and early nineteenth century than at earlier times and that this was a condition of the continued growth of population. But it is not likely that independent changes in agriculture played much part in initiating the increase.

There is, however, another and more likely way in which economic developments affecting food supplies may have influenced population growth. In some parts of Europe, population growth in the later seventeenth and the early eighteenth centuries had been held back by subsistence crises, by periods of exceptionally high mortality due to harvest failure; i.e., by the Malthusian positive checks in their purest form. There were such crises in Finland, Denmark, and France, at the end of the seventeenth century. Such crises seem to disappear—or at any rate become much less frequent—after the middle of the eighteenth century. This may be the result of purely fortuitous factors; harvest yields may have been better for climatic reasons or the incidence of endemic disease (which influences the effect on mortality of harvest failure) may have changed. But there is some reason for supposing that improvements in the marketing and transport of food and the maintenance of more adequate stocks made it easier from the mid-eighteenth century on to alleviate local harvest failures. Insofar as population growth occurred because of a reduction of subsistence crises for these reasons, it could reasonably be attributed to independent economic developments. But these subsistence crises were by no means universal; they did not occur in England or Sweden in the eighteenth century, and in these countries the improvement in mortality in the later eighteenth century and early nineteenth can more plausibly be attributed to fortuitous factors.

The division between fortuitous and non-fortuitous elements clearly does violence to the complexity of the problem, and, moreover, the balance between them varied greatly from region to region. But it seems clear that even where population growth was in some sense a response to autonomous economic changes, the most significant change was not any independent increase in per capita incomes, and that over most of Europe population growth was not in fact a response to economic changes even in a more general sense. There is a case, therefore, for regarding the population growth as in some measure fortuitous.

II

We shall now consider its effects. There are two problems: Why was the growth not checked? And why did it ultimately prove compatible with an increase in per capita income?

In many areas the growth of population did provoke checks. The great sensitivity of Prussian birth, death, and marriage rates to deficient harvests in the first half of the nineteenth century is possibly a sign of this. The Irish potato famine of the 1840's, and the great Russian famines are clearly a penalty for a rate of population growth unwarranted by resources. Possibly, also, one should interpret the spread among the French peasantry in the later eighteenth century of birth control within marriage as an attempt to check a population growth thought to be too rapid, given the opportunities of creating new households. The traditional mechanisms continued to operate. But why did they never operate—except possibly in the case of Ireland—to bring population growth to a halt?

The simple explanation is that the checks were offset by the responsiveness of agriculture, so that the full force of the checks was rarely called into play. The degree of response clearly depended, among other things, on the existing ratio of land to population and on the agrarian structure, and even in the most favorably situated countries there was a lag, and the relative rise of agricultural prices from the 1760's into the second decade of the nineteenth century is most reasonably interpreted as a sign of inflexibilities in the supply of agricultural products in face of growing population. But certainly by the 1820's and 1830's the course of prices in western Europe suggests that additional food was obtainable on fairly easy terms, and population growth was maintained without heavy pressure on the standard of living.

It is not difficult to see, in general terms, why agricultural output should have been responsive even in parts of Europe which lacked supplies of unsettled land. Over large parts of Europe the system of agriculture—developed in earlier times when land was abundant—involved a high proportion of fallow. At least since the seventeenth century, methods were known—the introduction of root crops and artificial grasses—which removed the necessity for fallow, and in addition, by increasing supplies of fodder, made it possible to maintain more cattle which in turn provided more manure to raise the yield of the arable land. This was a method which required little capital and very greatly increased the productivity of land. There was also the potato, which yielded more nutrition per acre than most alternative crops.

The new techniques could not be used everywhere—there was no scope for them in the Mediterranean lands, and this is one reason for slow population growth in Italy and Spain—but where they could be used and agriculture was therefore responsive, it is no problem why population should have grown more rapidly at a more or less constant standard of living. This seems to have been the position in England in the last three decades of the eighteenth century and in Germany. What needs to be explained is why at some stage standards of living began a sustained rise. Was it because the forces independently making for increasing income were exceptionally strong—so strong that they worked despite the increase in total population? Or was it that from the 1840's on exceptional opportunities for emigration appeared and mitigated population pressure? Or was it that the increase in total population in itself generated influences which were favorable to an increase in income?

There is no lack of possible mechanisms by which an increase in population of the sort envisaged could in principle have had favorable repercussions on income. There is first of all a Ricardo-type mechanism of the kind elaborated by W. A. Lewis. An abundant supply of labor at the going wage was favorable to accumulation, on the assumption that profits were much the main source of accumulation; and in a period when there were considerable obstacles to interregional labor movements, a high rate of natural increase was a necessary condition of an abundant supply of labor. Population increase was therefore favorable to the widening of capital and so increased the opportunities of trying out any new methods which might be independently available; i.e., it led to a more rapid absorption of existing technical knowledge and therefore increased the chances of making further technical progress. Or again a growing population might stimulate investment by its effects on the demand side. Even if per capita incomes fell, there would be an increase in total demand if the rise in the population was less than the fall in per capita incomes, and this might stimulate innovation in some sectors of the economy. Then again a growing population is likely to spend a higher proportion of its income on housing and transport facilities; and while this might be an unfavorable influence in an economy held up by shortage of funds, it might have a stimulating effect where a high propensity to save had previously been frustrated by a lack of investment opportunities. A growing population might also stimulate investment by reducing the risks of entrepreneurial error. Population growth might also have stimulated invention directly; though it diminished the incentive to search for improvements which saved labor, it stimulated the search for those which saved natural resources.

Thus while the simple and initial effect of population increase was to reduce per capita income, there are a variety of ways it might have had favorable effects. The difficulty is to say how important these countervailing effects were in practice. Was the marked rise in per capita incomes in all advanced countries in the second half of the nineteenth century a rescue operation conducted by an independently generated leap in, e.g., technical progress? Or was it the response to population pressure of the kind which prompted Professor Hicks to speculate that "perhaps the whole industrial revolution of the last two hundred years has been nothing but a vast spectacular boom, largely induced by the unparalleled rise in population."

How important each of these favorable influences may have been in any given case it is clearly impossible to say, except in a very impressionistic way. Two of the more important developments of early industrialization were certainly facilitated by the high rate of population growth or at any rate by the higher level of population which this produced.

The item of technical progress of greatest long-term significance—the development of a new source of power in steam—was directly related to attempts to resolve a resource-bottleneck which would not have existed in so severe a form had the rate of population growth been slower.

Moreover, the growth of population—or more strictly the attainment of a certain density of population—made it worth while to improve and create transport facilities. One of the main sources of economic growth before the industrial revolution was the gradual extension of the market and the greater division of labor it promoted in the ways described long ago by Adam Smith. And the principal reason why this mechanism produced more rapid results in the period of the classic English industrial revolution was that a large number of canals were built. The decision to build a canal was very sensitive to the volume of traffic. Canal building speeded up in the 1790's as opposed to, say, the 1750's, not because of any technical improvements in their methods of construction or because it was easier to raise funds, but because the volume of traffic warranted it. And, comparing the 1790's with the 1750's, one difference relevant to the volume of traffic was the greater density of population. The other effects are much more speculative. It may be the case that the increase in the total population, even with a fall in per capita income, created a larger market for industrial goods. It is possible that much of the growth of the English market for the simpler industrial goods was due to a "widening" of consumption of this kind. On the other hand, it is open to anyone to argue that, with a lower rate of population growth, per capita incomes would have been higher and that, though total demand for industrial goods would have been smaller, the composition of demand would have been more favorable.

Similarly, if one regards the increased population as a source of labor, it is clear that the course of English industrialization between say 1815 and the 1850's was very greatly influenced by the ability of English industrialists to obtain additional supplies of labor more or less at the going wage. This was favorable to the great widening of capital which took place. On the other hand, it is open to anyone to argue that though a lower rate of population increase might have led to less widening of capital, it would have led to more deepening, to the more rapid invention and adoption of laborsaving devices on American lines, and to the more rapid contraction of the older types of industry.

Anyone who wished to be skeptical about the favorable effects of a high rate of population increase could point to the experience of France. In France, with its exceptional demographic history, the volume of total production increased less rapidly than in England, but it is by no means clear that per capita incomes rose less rapidly.

One is tempted to decline upon a truism. A given growth of population was likely to have more effect in stimulating investment and invention and less effect in depressing per capita incomes when it occurred in areas where land was abundant and/or land-saving techniques were available. And that where additional land was no longer available and the possibilities of land-saving improvements exhausted the reverse effect was more likely.

Pushed beyond this I should say that, even in the areas most favorably situated, the rate of increase of population was excessive or was sustained for too long a period. Without being able to specify what the optimum rates for various purposes would have been, it is difficult to avoid this general impression.

Even in England it looks as if the rapid growth rates in the early decades of the nineteenth century were more effective in depressing per capita incomes than in stimulating investment and innovation. Whether there was a rise in living standards in the early phases of British industrialization is still a matter of vigorous debate but this fact itself is perhaps a reason for supposing that any rise was modest compared with that which started in the 1850's and 1860's. And if this was the case, the cause was primarily the rapid rate of population growth.

In those parts of Europe where the influence, independent of population, making for economic growth was less vigorous, population growth was excessive in the sense that it provoked checks, both positive and preventive. And but for emigration these checks would have operated much more severely. Emigration was, of course, one of the traditional remedies for population pressure—witness the movement of the Swiss as mercenaries and settlers—but emigration from the 1840's onwards was much greater relatively as well as absolutely and one has to go back to the great tribal migrations of earlier times to find a parallel. In the absence of opportunities for overseas emigration, either death rates would have been much higher or western Europe would have adopted methods for the control of births within marriage several decades earlier than, except in the case of France, they did. In nineteenth-century Russia, where the possibilities of emigration were less accessible, there were in fact great periodic famines of a pure Malthusian type.

Moreover, if western Europe had had to rely exclusively on the possibilities of internal migration within the Continent, the rise of per capita incomes in the relatively localized areas of economic growth would have been swamped by movements of people from areas where population growth was not matched by an increase in economic opportunities. This is, to some extent, what happened in the 1820's and 1830's in the case of the Irish migration to Lancashire. And, in the absence of possibilities of overseas migration, similar movements would have occurred on a much wider scale as the railways lowered the cost of movement over land.

Between the pattern of population growth in the early stages of European industrialization and that in present-day underdeveloped countries there are some evident differences.

In the first place, the causes of population increase in the latter are much more evidently fortuitous from an economic point of view —the fall in the death rate due to improvements in medicine and public health, most of them imported. The previous discussion has emphasized the fortuitous elements in the European growth, but, in the present context, the point to be emphasized is that some part of it was related to an increase in employment opportunities.

Second, the medical improvements cause in many underdeveloped countries a much more rapid rate of natural increase than was experienced in early nineteenth-century Europe. The forces that operated in Europe in that period, from their very nature, operated slowly and moderately, whether they operated on births or on deaths. Moreover, the traditional mechanisms of European population change did impose some brakes on the growth.

It is true that Europe, too, in the later nineteenth century experienced a fall in death rates for primarily medical reasons. But the fall was modest and slow compared with that experienced in some underdeveloped countries in the recent past, and, moreover, by the time the fall occurred among European countries large parts of the Continent had already experienced several decades of increasing per capita incomes, and the habits favorable to economic growth were well established. Partly because of the level of incomes attained, but partly also because of long-established attitudes to the family, western Europeans reacted to the rapidly falling death rates by a reduction of the birth rate, and particularly by a much wider control of births within marriage.

Third, because from early times European marriage had been tied to the setting up of a separate household, the population was much sparser than it is, for example, in the Far East today. The strength of the preventive checks in Europe is one of the principal reasons for the peculiar economic history of the region. It meant in the first place that average real incomes in Europe before the industrial revolution were higher than they typically are in contemporary underdeveloped areas. The consumption not only of food but of the simpler industrial goods was higher. Thus there was much more chance that any fortuitous increase in population would at least not lead to a fall in total income. It meant, second, that there were much greater opportunities, when population pressure increased, of increasing the cultivated area. In addition, the technical nature of agriculture in northern and western Europe made it possible to achieve a considerable increase in agricultural output even without an increase in the cultivated area. As Japan shows, considerable increases in agricultural productivity can be achieved, even when land is scarce, by the use of more labor-intensive methods. But, as the Japanese experience also shows, there are limits to this process. Thus a given increase in population was less likely to reduce per capita incomes in Europe. For both reasons there was more scope for the favorable effects of population growth.

Finally, the European experience was unique because of the existence of unsettled regions of abundant resources outside Europe.

ALLOCATION CRITERIA AND POPULATION GROWTH

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Ever since the economist first began to concern himself with economic growth and the statesman to worry about its policy implications, demographic trends and our capacity to deal with them have been a central issue. Whether viewed in the context of the contemporary less developed area or from the historical perspective of economic doctrine, it should be clear that any intelligent discussion of allocative policy must be based on a clear and prior formulation of the growth problem as a whole. While the relevant general theoretical frame has not always been rigorously stated, it has been consistently clear in all such contexts that population growth, together with capital accumulation and technological change, must play a major explanatory role. Only the precise nature—and magnitude—of that role has been subject to change—never its membership in this crucial triplet of explanatory variables.

It is the major purpose of this paper to analyze the role of demographic change in the context of growth in the contemporary less developed world and to indicate the direction which the evolution of optimal allocation criteria ought consequently to take. Section I is devoted to an examination of the theoretical heritage to be drawn on in this current effort at evolving realistic allocation criteria for the less developed economy. Section II attempts to sketch the outlines of a growth-theoretic frame within which demographic change and population policy must find their rightful place. Section III concentrates on what we shall call the "positive" view of population policy, i.e., efforts to use allocative decisions to influence population growth directly, and Section IV on the "passive" view which accepts demographic trends as more or less exogenously given and deals with the evolution of allocative criteria under this constraint.

I

The various growth-theoretic formulations which travel under the umbrella of the classical system accord a major explanatory role to population growth. In fact, this system is sufficiently ambitious to attempt to accommodate population growth as a major endogenous variable responsive to other central magnitudes in the system. Briefly put, whenever capital accumulation occurs, the demand for labor forces the real wage up, if temporarily, from its subsistence minimum; this in turn induces population increases which force a return to the subsistence level of wages. The classicists thus cite the pressures of self-induced increments to the labor force as responsible for the indefinite maintenance of a biologically set minimum level of wages. Given the expected dominance of diminishing returns in the pre-eminent agricultural sector, capital accumulation and the induced growth in number jointly press against the limited natural resources base. Inevitably, the joint return to labor and capital diminishes; and with the real wage pinned down at the subsistence floor the profit rate bears the major burden of the downward adjustment. Ultimately the capitalist system then tends to grind to a halt as profits and capital accumulation are choked off and a stationary state is approached.

Thus, in the classical system, frugality and thrift, which promote savings, dominate and constitute the major engine of growth leading to both capital accumulation and population growth. The scarcity of land in the long run finally brings the process to a halt as both the ability and the incentive to invest dry up. While the classicists admit the possibility that technological change may temporarily postpone the fateful day of stagnation, its role is clearly a subordinate one and does not provide much hope in terms of a long-run solution. Nor, it should be noted, do the Malthusian prescriptions for the exercise of "preventive checks"; the policy emphasis here, if any, is on enhancing the system's over-all capacity to perform better, via laissez faire and free trade, and thus be in an improved position to accommodate demographic change.

Marx's view of the capitalist system is not radically different. Once again savings and capital accumulation dominate, but now technological change via adjustments in the organic composition of capital are elevated to a more import role. Moreover, while the notion of a subsistence wage level is retained, it is now not derived from Malthusian population theory, which Marx rejects as a "libel on the human race"; instead, with population growth exogenous, it is the twin forces of capital accumulation and technological change slanted in a laborsaving direction which continually replenish and enlarge the "reserve army" of the industrially unemployed whose bargaining power is slipping steadily.

In Marx we thus see for the first time a recognition of the overwhelming importance of the industrial sector and a de-emphasis of the agricultural sector which had received an inordinate share of attention in the classical system. Once again, however, the view is stagnationist—and once again the declining rate of profits (*inter alia*) sounds the death knell of capitalism.

During the subsequent neoclassical era interest in matters of population receded in tune with a more general de-emphasis on matters of growth. While the history of doctrine, with the notable exception of Schumpeter, was being made almost exclusively in general equilibrium and microtheory, demography was being relegated to the sociologist and technological change to the engineer. More recently, however, we have witnessed a pronounced renewal of interest in growth theory and in the readmission of population growth into the explanatory framework of economics.

The initial fillip in behalf of this tendency was, of course, provided by the stagnationist fears of the thirties and found its full expression in the post-Keynesian concern with the longer run growth prospects of the mature capitalist system. While the early Harrod-Domar formulations exhibited their Keynesian origins via an almost exclusive concern with problems of stability, the emphasis since has been shifting gradually towards the longer run and the reintegration of population growth and technological change into the explanatory apparatus.

In terms of our basic triplet of forces, exogenously given population growth and innovational activity jointly determine the rate of growth of the capital stock and the rate of profits. In this fashion the economy is pulled forward by the twin components of what Harrod rather felicitously called the "natural rate of growth." The overriding social problem of the mature capitalist society can then be defined as sustaining a high rate of capital accumulation in the face of a declining rate of profits. It should be noted that the role of the demographic variable is now reversed, however; population growth is no longer viewed as a curse contributing to stagnation and the disintegration of capitalism but emerges as a blessing helping to prevent stagnation by ensuring the absorption of full employment savings over time. In the same vein, technological change now assumes a much more dominant role than in the classical framework. Innovations are depended on to frustrate diminishing returns via the creation of new investment opportunities for the exploitation of the capitalist. Population growth, though still in a somewhat less prominent role, makes its contribution via the stimulation of investment opportunities yielded by the consumption requirements of new households.

The basic concern of this paper, to which we shall now turn, rests, of course, with population growth and allocation criteria in the quite distinct growth-theoretic context of the contemporary less developed world. In this connection it will become evident that a careful selection from among the classical, Marxist, and post-Keynesian conceptions of growth yields important building blocks in the evolution of relevant contemporary theory.

II

The generally unhappy state of our progress over the past decade and a half in moving towards a satisfactory growth-theoretic formulation with applicability to the less developed economy is a well-recognized and much-lamented fact. The realization of the explanatory significance of noneconomic or institutional variables and the dangers inherent in the mechanical transfer of theoretical constructs applicable to the developed economy has led to a general unwillingness to wave the customary magic wand of *ceteris paribus* and to a consequent timidity in our growth-theoretic formulations. As a consequence, the main attack on problems of development has proceeded inductively via a mining of the laboratory of economic history, on the one hand, and via the collection of a considerable arsenal of miscellaneous growth-theoretic ideas spawned as a by-product of contemporary planning experience, on the other. This has meant that many of our policy prescriptions, some addressing themselves specifically to the demographic problem, e.g., the big push, critical minimum effort, etc., while undoubtedly useful, have amounted, in some sense, to "shooting from the hip." To a large extent this has been inevitable since we cannot expect a moratorium on policy while the cautious search for a more general theory proceeds. We are, nevertheless, faced with the fact that to date attempts to combine some of this miscellaneous, often very insightful, knowledge within a somewhat broader analytical frame are still in their infancy.

Some progress in this direction can nevertheless be recorded, most of it, at least in this author's view, based on the insight of Arthur Lewis who translated the institutional phenomenon of disguised rural unemployment into the analytical condition of the unlimitedness of the supply of labor.[1] The classical school, it will be recalled, never fully recognized the importance of the industrial sector, while Marx was almost wholly concerned with the fate of industrial capitalism. In the context of the labor surplus, less developed economy, it is not only helpful but virtually essential to treat the economic system as composed of two sectors: a large agricultural or subsistence sector and a small but growing industrial or capitalistic sector. Once again the classical concept of a subsistence or institutionally determined real wage in agriculture becomes relevant with the industrial wage level set at equality with (or at a margin above) this agricultural wage. In this context it is the relative growth of the capitalistic sector via a transfer of labor from the agricultural subsistence sector which represents the basic social problem underlying successful development.

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[1] A. Lewis, "Development with Unlimited Supplies of Labour," *The Manchester School*, May, 1954; see also G. Ranis and J. C. H. Fei, "A Theory of Economic Development," *A.E.R.*, Sept., 1961; and S. Enke, "Economic Development with Unlimited and Limited Supplies of Labour," *Oxford Econ. Papers*, June, 1962.

It is clear that in such an economy savings must once again be viewed, as in the classical system, as the driving force behind the reallocation and development process. After all, it is the size and allocation of the total economy's limited savings fund—as supplemented by foreign aid—which will determine the rate at which agricultural labor can be freed, on the one hand, and industrial employment opportunities created, on the other. But equally important in determining the rate of industrial labor force absorption is the role of technological change—the third member of our triplet. Given the redundancy of the agricultural labor force, it is clear that the transfer of labor into productive employment will be considerably enhanced the higher the innovational intensity embodied in a given rate of capital accumulation and the more labor-using the innovational activity.

Capital accumulation and technological change thus represent the twin forces determining the rate of labor reallocation. Population growth, on the other hand, may be viewed as continuously augmenting the pool of the agriculturally underemployed. In this setting the criterion of "success" in the development effort may be stated as a rate of industrial labor absorption in excess of the rate of population growth.

The underlying allocative problem we are concerned with may then be viewed in terms of this criterion and in this growth-theoretic context. Let us briefly state it: at any moment of time the economy as a whole disposes over an extremely limited savings fund, contributed by the siphoning off of agricultural savings, on the one hand, and initially small, but growing, industrial profits, on the other. If this fund is to be allocated optimally or near-optimally—and the price of nonoptimization is understandably high—rational policy must be concerned with both blades of the scissors: the rate of growth of numbers and the smoothness and speed of the intersectoral labor reallocation process. The first of these may be called the "positive" blade since it intends to extend the traditional allocative horizon to include demographic variables as directly subject to the effects of policy instrumentation. The latter may be called the "passive" blade since it more or less accepts demographic trends as exogenously given and concentrates on the allocative problem between the two sectors under this constraint. If the "success" criterion is to be satisfied it is clear, from the theoretical point of view, that we should attempt to operate on both blades. Whether this is also practicable in the present state of our knowledge is another matter. Let us now turn to an examination, in turn, of the positive and passive allocation criteria as defined above.

III

The extensive literature of recent years dealing with the entire syndrome of investment criteria is undoubtedly familiar to the reader.[2] In this context, our positive view of the demographic allocation problem is seen to be nothing more than an attempt to broaden traditional choices in order to explicitly include the population variable. At a minimum, given equivalent social marginal productivity patterns, for example, with respect to alternative investment patterns, their secondary impact on population growth should be considered whenever possible, the obvious purpose being a relative enhancement of output and the economy's savings fund in the next period.[3] Where a considerable portion of the existing population is technologically redundant or underemployed, and hence its consumption, at every age level, exceeds its marginal product, we do not require refined optimum population theory to know that population has already exceeded its optimum and that further growth is an unmitigated evil.[4] Newborn babies obviously have a negative asset value while alternative claimants for the allocation of the economy's surplus show a significantly positive return.

Unfortunately, however, the implied attempt to restore population growth as an endogenous variable has to date been rather difficult in execution. The broadening of the allocative investment choice horizon to include the relative impact on the size and composition of the population requires a more or less adequate understanding of the functional relationship between alternative expenditure patterns and fertility rates, age specific mortality rates, etc. Admittedly, we now know a good deal about the potential impact of low-cost DDT campaigns in reducing death rates from 27 to 13 per 1,000 (e.g., Ceylon) in the course of a few years. And we have reasonably respectable information on the relationship between expenditures on environmental sanitation and infant mortality. On the other hand, our information on the cost and effectiveness of inducing fertility change is still woefully inadequate—in fact, if anything, we have had to take another look at even some of our supposedly "hard" facts based on western European experience. For example, given disposable private per capita income, urbanization and the accompanying increases in socially consumed income are traditionally assumed to result in a decline in the birth rate—at least after some time. The evidence in India to date is, at best, inconclusive on this question.

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[2] It is competently summarized in H. B. Chenery, "Comparative Advantage and Development Policy," *A. E. R.*, Mar., 1961.

[3] See Galenson and Leibenstein ("Investment Criteria, Productivity and Economic Development," *Q.J.E.*, Aug., 1955) for one such formulation.

[4] To my knowledge, the only objection to this view comes from Hirschman (*The Strategy of Economic Development*, Yale Univ. Press, 1958) who sees the likelihood of a positive relationship between severe Malthusian pressures and the economy's innovative capacity. This notion is peculiarly unconvincing where labor is already redundant and where a further deterioration of the resource base is more likely to lead to famine and apathy than to a creative response. One cannot escape the conclusion that Western (or possibly Latin-American) challenge and response mechanisms are being inappropriately applied here.

Galenson and Leibenstein,[5] to cite another example, emphasize the attractiveness of capital-intensive production functions because, *inter alia,* they tend to reduce labor's share in income and are thus expected to dampen population growth. A related study of Puerto Rican and Japanese experience,[6] indicates that fertility rates, in fact, are higher if women work under conditions in which home and industrial activities can be combined than if they work away from home in a factory-style setting. But, while the high correlation between labor intensity and cottage-style manufacturing seems to lead Jaffe and Azumi to a corroboration of the Galenson-Leibenstein hypothesis, it should also be noted that the same study reveals fertility rates for women entirely outside of the labor force to be still higher than for those working in cottage industries. Thus when we are faced with a choice between labor-intensive cottage-style and capital-intensive factory-style production—solely now with respect to their differential impact on population growth[7]—it ought to be recalled that the second alternative, given scarce capital, implies a lower degree of labor absorption and consequently more unemployed females. Thus the net differential effect on fertility is by no means established.

Such gaps in our knowledge of the underlying behavioristic relationships are even more apparent in terms of the recurrent efforts in the less developed areas to evolve a national program of family planning. The Indian experience, if no other, indicates that even when the governmental machinery is fully committed to the effort too little is known about various economic and noneconomic obstacles to legal change, to the dissemination of information and to the relative acceptance of competitive technologies of application. In the absence of reliable information on attitudinal and household behavior patterns, Indian planners have recently felt compelled to turn to sterilization as a "sure remedy." This is undoubtedly an expensive method, imbetween population growth and the rest of our system we know that the longer it takes for a given-sized agricultural surplus army to be transferred, the more likely that population growth which tends to augment that army will gain the upper hand. In terms of our basic triplet once again, our allocative criteria must ensure that, having simultaneously satisfied the investment requirements of the agricultural sector, the twin forces of capital accumulation and technological change in the industrial sector are powerful enough to permit industrial labor absorption to proceed at a rate faster than population growth. A more precise specification of these criteria may, finally, be attempted.

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[5] *Op. cit.*

[6] A. J. Jaffe and K. Azumi, "The Birth Rate and Cottage Industries in Underdeveloped Countries," *Economic Development and Cultural Change,* Oct., 1960.

[7] The same Galenson-Leibenstein hypothesis is subjected to much more serious doubts when we ask the more general question of the relation between capital intensity and reinvestment ratios. See the author's "Investment Criteria, Productivity and Economic Development," *Q.J.E.,* May, 1962.

First of all, it should be pointed out that the allocation of funds to either sector should not be viewed as necessarily restricted to investment in physical capital formation. It is often said that the agricultural laborer when released is usually not yet ready to become an industrial employee in the strictest sense. A good deal of overhead capital formation may be required to transform him into a member of what may be called an industrial labor force. This is undoubtedly true but the current swing of the pendulum towards emphasizing investment in education should not lead us to neglect the fact that the relationship between training as a cost and as a paying asset over the active lifetime of the individual concerned must be precise enough to render possible comparisons with alternative methods of enhancing labor productivity. There is evidence, for example, of a rather instinctive acceptance of literacy as a virtual precondition for raising agricultural productivity, on the one hand, and creating a trained industrial labor force, on the other. But before the economy decides to commit rather formidable resources in this context it might be well to ask whether it is, in fact, intelligence or literacy which provides the key to a successful agricultural extension program. (The author's own, if somewhat casual, impressions of Pakistan indicate that it is often governmental administrative machinery rather than the need to rely on oral communication which is the key.) On the other hand, perhaps insufficient notice has been taken to date of the potential impact of investments in health, undertaken for other reasons, on labor productivity in both sectors. Malaria weakens thousands for every one it kills; more than 90 per cent of East Pakistan's villagers suffer from debilitating intestinal parasites.

Second, the rapidity of the industrial labor absorption process is a function of the strength of the two related forces: capital accumulation and innovational activity. The higher the rate of savings which can be generated the faster, of course, the rate of labor absorption. Similarly, the greater the strength or intensity of innovations which

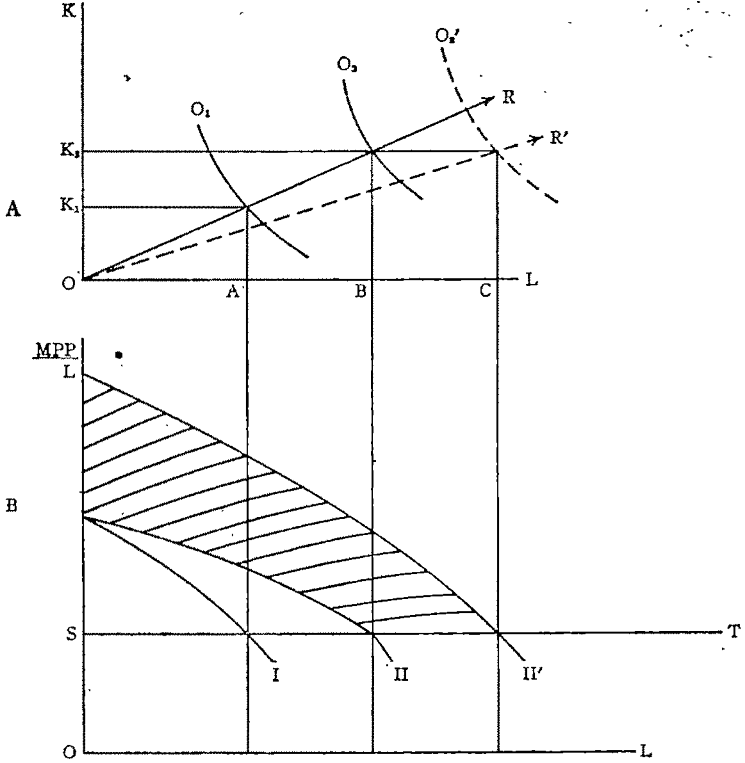

Figure 1

can be embodied in a given volume of capital formation, the greater the rate of labor absorption. Moreover, given intensity, the more innovations are slanted in a labor-using (or capital-saving) direction, the faster the process of labor absorption.

Let us illustrate the above by means of diagrams A and B. Given an industrial sector with production functions of constant returns to scale and an industrial labor supply curve pinned down at the institutional real wage level, a given rate of industrial capital accumulation simply implies an equivalent rate of labor absorption. In other words, in the absence of any technological change, given the production contour map in diagram A and the relevant marginal physical productivity of labor curves I and II in diagram B (with *ST* describing the Lewis supply curve of labor at the constant institutional level of wages *SO*) the growth path of the capitalist sector will be along the radial line *OR* with only capital-widening taking place. The rate of labor absorption and the rate of output growth ascribable to capital accumulation only are thus precisely equal to the rate of capital accumulation; any possible increase in the rate of capital accumulation will yield an equivalent increase in the rates of growth of the industrial labor force and industrial output. Thus when, in diagram A, the capital stock increases from K_1 to K_2 (and in diagram B the marginal physical productivity of labor curve shifts from I to II) labor absorption and total output go up in the same proportion; i.e., by *AB* and O_2-O_1, respectively.

Let us now admit of the likelihood that technological change plays a major role in the development process. It is, in fact, reasonable for us to expect that the less developed economy, as a late-comer to industrialization, will be in a position to borrow freely from the available known techniques (some now obsolete in the country of origin) and then to make its own adaptations on the basis of the imported technology. It seems intuitively logical (and can also be rigorously proved) that, given the economy's factor endowment, it is, moreover, the labor-using kind of innovation which is likely to be adopted in this fashion to the maximum extent possible. In diagram B the occurrence of one such innovation between any two points in time may be indicated by the fact that the marginal physical productivity of labor curve not only shifts from position I to position II as the result of capital accumulation but undergoes a further shift to position II′ as the result of innovation. (In diagram A this can be denoted by production of O'_2 units of output and a switch to the dotted radial expansion path *OR′* after the innovation.) As a result the innovation leads to an additional *BC* units of industrial labor absorption (i.e., a total of *AC* units for the period) as well as an additional increase in industrial output as indicated by the shaded area. It should, moreover, be clear that if capital accumulation over time is continuously associated with innovations, radial line *OR′* in diagram A will continue shifting towards the horizontal axis and the economy's actual growth path will exhibit a continuous tendency towards capital-shallowing or a reduction in the capital-labor ratio. From the point of view of speeding up the rate of labor absorption, therefore, as long as the unlimited supply of labor condition obtains, it is clearly desirable to have not only a maximum rate of capital accumulation but also maximum innovational intensity. Furthermore, it should be obvious that the more a given innovation is slanted in the labor-using (or capital-saving) direction the larger the gain in industrial labor absorption and in industrial output. This can be seen diagrammatically from the fact that if II′ is flatter *BC* is larger and the shaded area is larger.

Thus we see that the rate of industrial labor absorption as well as the rate of growth of industrial output depends directly on the rate of capital accumulation and innovational activity. More precisely, industrial labor and output growth are faster if the rate of capital accumulation is faster (a substantial shift from I to II), innovational intensity is high (a substantial shift from II to II′), and the innovation is biased in the labor-using direction (the II′ curve is "flat"). For a labor-surplus economy the above provides important policy guidelines for allocative decision making.

It will be noted that such advice, at least in part, may run counter to much current planning commission practice which puts a high pre-

TABLE 1

Time	Industrial Labor Force (Thousands) (1)	Industrial Capital Stock (Million Yen) (2)	Industrial K\L Ratio (3)	Rate of Growth of Industrial Labor Force (4)	Rate of Growth of Population (5)
1888	4,993	21,331.8	4.27234	3.90	1.10
1889	5,188	21,449.9	4.13452	3.80	.93
1890	5,385	21,580.7	4.00756	3.66	.70
1891	5,582	21,738.1	3.89432	3.57	.81
1892	5,781	21,897.0	3.78775	3.44	.87
1893	5,980	22,053.4	3.68786	3.39	.79
1894	6,183	22,216.5	3.59316	3.25	1.04
1895	6,384	22,417.5	3.51151	3.20	.99
1896	6,588	22,649.5	3.43799	3.26	1.10
1897	6,803	22,885.4	3.36402	2.90	1.19
1898	7,000	23,152.2	3.30746	3.09	1.07
1899	7,216	23,439.2	3.24823	3.06	1.13
1900	7,437	23,697.7	3.18646	3.08	1.17
1901	7,666	23,962.6	3.12583	2.83	1.46
1902	7,883	24,242.5	3.07529	2.89	1.29
1903	8,111	24,520.0	3.02306	2.96	1.11
1904	8,331	24,821.9	2.97946	2.52	.94
1905	8,561	25,130.4	2.93545	2.64	.83
1906	8,787	25,480.8	2.89983	2.56	1.07
1907	9,012	25,842.7	2.86759	2.46	1.21
1908	9,234	26,245.4	2.84226	2.43	1.28
1909	9,458	26,730.4	2.82622	2.35	1.34
1910	9,680	27,246.9	2.81476	2.34	1.43
1911	9,907	27,792.9	2.80538	2.29	1.44
1912	10,134	28,341.4	2.79666	2.23	1.43
1913	10,360	28,897.4	2.78932	2.22	1.38
1914	10,590	29,427.0	2.77875	2.09	1.40
1915	10,811	30,001.4	2.77508	2.08	1.25
1916	11,036	30,458.1	2.76804	2.05	1.15
1917	11,263	31,189.5	2.76920	1.98	.68
1918	11,486	31,926.5	2.77961	1.99	.73
1919	11,714	32,673.3	2.78925	5.98	1.05
1920	12,415	33,453.7	2.69462	2.00	1.22
1921	12,663	34,527.1	2.72661	1.95	1.29
1922	12,910	35,630.1	2.75988	1.93	1.70
1923	13,159	36,651.3	2.78526	1.90	.91
1924	13,409	37,677.5	2.80987	1.85	1.87
1925	13,657	38,781.6	2.83969	1.81	1.28
1926	13,904	40,067.3	2.88171	1.79	1.31
1927	14,153	41,423.9	2.92686	1.76	1.31
1928	14,402	42,783.9	2.97069	1.72	1.31
1929	14,650	44,093.9	3.00982	1.69	2.40
1930	14,898	45,710.9	3.06826		

mium on the installation of efficient modern plant and equipment. Related allocative policy is very often based partly on the naïve notion that national self-respect requires the importation of the latest technology but partly also on the belief that up-to-date factory-style methods of production are likely to be so productive in terms of output generation as to outweigh any disadvantages incurred through lower labor absorption. As a result we often see laborsaving innovations and capital-deepening in practice. Our brief analysis above, however, shows this policy to be wholly incorrect. While capital-deepening and higher labor productivity may be traditionally accepted as signs of economic progress in the mature economy this is not the case when we take into account the total resources picture of the typical less developed area. When the industrial sector is viewed as but a small capitalistic island surrounded by a sea of subsistence agriculture the wisdom of maximum resort to labor-using innovations seems incontrovertible. As long as the unlimited supply of labor condition obtains innovations should be as capital-saving as possible, from the output generation as well as the labor absorption points of view. After all, an economy must try to make maximum use of its relatively abundant resources.

Japan is usually cited as the major relevant example of a less developed economy which emerged from a labor-surplus situation, akin to that facing many contemporary less developed areas, into the company of the mature economies. It therefore seems eminently worth while to briefly examine her historical record in the light of the above conclusions. Japanese capital stock and industrial labor force figures are presented in columns 1 and 2 of Table 1.[10] The capital-labor ratios computed in column 3 give clear-cut evidence of continuous capital-shallowing in Japanese industry during the late nineteenth and early twentieth centuries, followed by a reversal of that trend after World War I. This indicates that Japan was, in fact, making full use of her potential for adopting capital-saving innovations to accelerate the redundant agricultural labor transfer and absorption process. Since we believe such policy to be a necessary, if not sufficient,[11] condition for success in the development effort, it should be no surprise to find that if we apply our own criteria of success Japan qualifies eminently. A comparison of the rates of industrial labor absorption and the rates of population growth in columns 4 and 5 of Table 1 clearly indicates that Japan was indeed able to continuously beat the Malthusian trap in the course of her development effort. A similar analysis of the performance of India—an elaboration of which is prevented by limitations of space—shows continuous capital-deepening during the fifties and a rate of growth of the industrial labor force consistently below the rate of population growth. National allocative policy with respect to intersectoral balance and with respect to the type of technology embodied in the growing industrial capital stock must thus be assigned a considerable share of the credit for success or failure in the development effort.

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[10] Capital stock figures were computed by linking the 1930 wealth estimates of Ohkawa (K. Ohkawa and Associates, *The Growth Rate of the Japanese Economy Since 1878*, Tokyo, 1957) with Rosovsky's (*Capital Formation in Japan, 1868–1940*, New York, 1961) series on capital formation through a permanent inventory method. Labor force and population data are from Ohkawa, *op. cit.*

[11] Not sufficient in the sense that if the rate of capital accumulation is too low the best allocative policies will not help.

It may therefore be suggested that our proposed view of the allocative problem with reference to demographic change has considerable applicability to contemporary labor-surplus underdeveloped countries. Efforts to improve our understanding of the impact of alternative expenditure policies on population growth should, of course, continue. But meanwhile, in the absence of a sound theoretical frame, it is difficult—and costly in terms of both physical resources and scarce administrative energies—to attempt to apply a set of positive allocative criteria; passive criteria geared to an intelligent accommodation of population pressures in the context of our above two-sector growth-theoretic frame may, therefore, well constitute a viable half-way house short of the possibility of a full return to the magnificent dynamics of another era.

DISCUSSION

William N. Parker: To an economist, Professor Habakkuk is surely one of the most satisfying generalizers and simplifiers of our knowledge of modern economic history. He exhibits in this paper the bits of order and process observable or deducible from Western population experience without creating a design too grand or a model too strict for the facts as we know them. He falls at times, it must be admitted, into the failing of every scholar who would combine caution with breadth of conclusion. What he gives with one hand he often takes away with the other. He creates the phantom of rationality and, just as we have embraced it, he makes it melt in our arms. He offers us the apple of knowledge; then turns it to ashes as our teeth sink in. To be tempted by so honest a serpent is an exasperating, yet in the end a reassuring, experience.

First, it should be clear what population phenomena Professor Habakkuk is discussing. The great fall in birth and death rates from 1850 on is ruled out by the opening sentence, though not by the title, of the paper. The discussion concerns conditions prior to this central modern phenomenon, and—since statistics accumulate where the birth rate decays—prior to the availability of much reliable information. I suspect that where Professor Habakkuk speaks of two instances—Sweden and England—one can read, *the* two instances on which long series are available. In the first case, Heckscher's attempts to relate the vital statistics to harvest data have been questioned by Ötterstrom; in the second case, Professor Habakkuk himself has been responsible for initiating a recent exceedingly tangled controversy. He has provided in this paper surely the best general statement of the bits of evidence at hand, and I allude to this embarrassing matter of data only to underscore the caution that he himself exhibits.

The trouble with simplifications, or syntheses, in areas where there is little information is, of course, that one must employ a plausible but rather empty model. The mechanism of preventive and positive checks taken together is such a model, if indeed it can be called a mechanism at all. It states that if men are born, they must die, and if they are not born, they need not die. These alternatives grow interesting if we assume, or know from observation, that over a long period: (1) per capita income is relatively constant, (2) population rose and fell, (3) many elements in economic opportunity—e.g., techniques, savings functions—are constant, and (4) the birth rate stands at less than the physiological maximum and varies between places and periods. Then the movement of population must have been co-ordinated somehow with the change in other elements in economic opportunity—weather, economic organization, trade (an element this paper seriously neglects)—to produce the stable incomes, and variations in birth rates may have helped produce the population response. But to speak of a mechanism, the theory must specify how the improvements in the economic environment might have affected economic opportunities through the system of prices or through the system of political power. Once transmitted to a population in a certain distribution, the response of the marriage rate depends upon the income elasticity of demand by the affected population for those goods which are consumed jointly, and in a fixed proportion, with the delights and consequences of marriage. Side-effects of early marriage on enterprise, innovation and labor efficiency must also be explicitly taken into account. In a traditional society with strongly fixed behavior patterns, a direct and continuous connection between two such widely separated variables as resources and marriage is not impossible. If it is observed, the chain of interactions among individuals is a long one, and the mechanism of the preventive check must look like some of the early drawings of spinning machinery. In earlier articles Professor Habakkuk has indicated something of these mechanisms, and one may hope that in his quest for ever more general levels of explanation he will find time still to pursue this more precise and narrower objective.

A second point at which issue might be joined is at Professor Habakkuk's use of the word fortuitous. Like most of us, he uses the word fortuitous for the things which, at the moment, he would rather not think about. In a narrow and short-run examination of population, a fortuitous element might be identified as any shift in the age-specific birth or death rates, producing change not inherent in the existing age structure and the existing fertility and mortality pattern. Professor Habakkuk, however, means more than this. What he is trying to determine is whether a line of influence runs from change in industrial techniques and productivity to population growth and, if so, whether it feeds back again to economic change. No such direct line appears. But surely this is again partly because neither marriages nor babies are made through the direct application of steam power. He appears to be considering as economic, and so non-fortuitous, those changes in employment opportunities, the wage rate, and the social structure—whatever that means here—which can be traced to the introduction of steam. But one wonders whether lines of connection no less roundabout might not be drawn between industrial change and some of his fortuitous elements—the emancipation of serfs, the improvement in agricultural and medical techniques. His distinction is useful as an artificial device to permit closer reasoning about interconnections, but in a fuller explanation of economic history it is unimportant. One feels here that Professor Habakkuk, having mounted successfully some slippery logs from the broken raft of economic theory, wants to ride them a while before they roll away beneath him. In his recent book on British and American technology he does indeed extend economic reasoning with some success into one of the elements he now labels fortuitous. But the ultimate job is not simply to extend reasoning based on economic assumptions about human behavior into the explanation of these noneconomic phenomena. If after all economic calculation is used to explain everything else, there will be nothing left by which to explain economic calculation. We are all seeking, I take it, a history as comprehensive as human activity with a reasoning as strict as mathematical logic.

In this comment I have accused Professor Habakkuk both of being simple and inexact and of being rigorous and narrow. His simplicity has a Malthusian and his rigor a Ricardian ring. My purpose is simply to nudge him off his classical stilts and to land him squarely in the socioeconomic soup in which the rest of us are swimming. Most of us produce here only "moonbeams from a larger lunacy," as Stephen Leacock called some of his thoughts, but all of Professor Habakkuk's work gives us hope that if he will extend himself on beyond economics he may radiate the sunlight of a larger synthesis.

PAUL DEMENY: There is much to applaud in Professor Ranis' highly stimulating paper on allocation criteria and population growth. However, to put to the best use the few minutes allotted to me I will try to limit my comments to points where I have to register disagreement or a desire for an emphasis different from his. To the latter category belongs, first of all, the case that can be made to establish the population effects of alternative allocation of resources as an at least theoretically valid investment criterion. I do not believe that this case rests on the existence of technologically redundant or unemployed labor in an economy, or, alternatively, on the presence of a segment in the total population whose consumption exceeds its marginal contribution to aggregate output. Neither do I believe that the case in any way should be based on the existence of a "Malthusian trap," as the concept is understood in the current literature, nor that to eliminate it it would be sufficient to "beat the trap"; i.e., to develop faster than the population grows. While the weight of the argument will be undoubtedly reinforced under the extreme conditions, I think that to limit the discussion to such cases unjustifiably restricts the scope of the argument and is detrimental to its proper appreciation. Population growth, after all, has a quasi-biological upper limit: a rate of increase that is, or could be, exceeded by output growth in all present-day underdeveloped countries. In the same time it can be shown that if population growth could be manipulated freely, i.e., manipulated without some positive economic cost, a gradual transition to slower rates of population increase via fertility reduction would involve practically in each of these countries important, if varying, economic advantages, provided that welfare is defined primarily in terms of per capita indices, or rather as some optimization of such indices over time. We know, however, that population trends cannot be changed in such a fashion. The theoretical case is then set for making economic sacrifices—demographic investments—in order to influence population growth in the desired direction. As a matter of classification, demographic investments might be indirect or direct. In the former case, investments in "conventional" projects, because of their expected influence on population trends, would be pushed beyond the point that would be warranted if they were demographically neutral. In the latter case, a direct attack would be made on some demographic variable, e.g., by bribing potential parents to abstain from procreation. In either case the allocation criteria for demographic investments should be controlled by the same principles as investments in a conventional project; i.e., funds should be channeled into population-influencing schemes up to the point where their efficiency in raising the relevant index of welfare ceases to be superior to that of the available alternative investments. To implement such investments, public intervention, direct or indirect, is clearly necessary: it can be justified on the basis of a difference between private and social marginal productivity. The beneficial or unfavorable consequences of different population growth rates due to alternative investment patterns are thus best interpreted in the familiar terms of external economies and diseconomies. The case for demographic investments from a purely economic standpoint is as general as the one for, say, human investments. It is probably possible to develop without sacrificing scarce resources on education and thereby continuously beat the "knowledge gap." Such a policy is also likely to be a less than optimal one.

So much for the theoretical underpinnings of the case for demographic investment. The important question is, of course: what is the operational significance of all this? The abstract justification of the population effect as an investment criterion is derived from the existence of a general economic-demographic interrelationship, but the quantitative evaluation of the importance, or even relevance, of the criterion hinges upon the knowledge of the exact nature of such an interrelationship, and this is the point where the difficulties seem to be overwhelming. Accordingly, Professor Ranis is pessimistic. He relegates the demographic factor to the limbo of independent variables with the added advice to the "economist and the planner" to shift "some of his attention" from the positive approach to the "passive approach to the demographic allocation criteria problem." The advice is somewhat puzzling, since the advocated solution, and behavior, is virtually unchallenged in modern growth theory—unfortunately so. I should add at this point, also, that since Professor Ranis' passive policy—maximization of the speed of the labor transfer process—is neither dependent on the rate of population growth, nor is it suggested that population growth will be affected by it (as compared to population growth resulting from some alternative policy), it is not clear in what way his scheme represents, as he claims it does, a "halfway house" back towards the classical dynamics which assigned population growth an integral part of the system.

Professor Ranis' rejection of the "positive approach" may well be entirely defensible. But the little defense he offers is merely declaratory. At three points in the paper he makes brief references to schemes designed to influence fertility and characterizes them at each occasion as "undoubtedly expensive" and "costly." But costly compared to what? Costly under what circumstances? Would they be still costly if their cost were reduced by 20 per cent? Or by 65 per cent? Assuming a fertility reducing program of given effectiveness, what would be the size of demographic investments that would just leave the economy on the margin of indifference between intervention and nonintervention into demographic processes? Assuming a given economic loss implied by an investment pattern when compared to an optimum solution, what would be the amount of demographic change required to offset for such a loss? I believe, perhaps naïvely, that the discussion of such questions would have been more than a futile exercise in arithmetic and speculation and would have been necessary to support, or disprove, the point Professor Ranis seems to have taken for granted. On the basis of extensive calculation and a rather involved argument, both of which defy a brief summary, I also believe that such a discussion would have established at least three points of some theoretical interest with respect to the whole spectrum of present-day underdeveloped countries.

First, the rather widespread belief that indirectly induced population effects (e.g., via faster than otherwise optimal urbanization and/or industrialization) could qualify as an investment criterion can be conclusively refuted. Second, it can be established that the economic case is very strong for foregoing, if necessary, large directly productive investments in order to achieve an early substantial reduction of fertility, thereby reaching results that support, on the basis of a different argument, some important proposition advanced by Professor Enke in a series of articles during the past few years. Third, it can be concluded that current theoretical constructions based on the idea of circular causation, such as the minimum critical effort thesis, the theory of low-level equilibrium trap, or the theory of "big push," can receive no support from the interrelationships between demographic and economic phenomena.

Admittedly, the preceding suggestions wholly ignore the question of the design, the existence, and even the feasibility of programs that may directly affect fertility. But these problems are beyond the domain of the economist, just as the study of technological processes is. Converting resource inputs to outputs taking the form of some demographic change is a matter of social engineering. Progress in this field may conceivably receive an impetus from the labors of economic analysis.

A discussion of Professor Ranis' "passive approach" to development planning would be now in order. Such an attempt, within the given time limits, could, however, merely open up a Pandora's box of all the central issues of the theory of growth in underdeveloped economies. I will make only a few impressionistic comments.

I fail to see the reason for the overwhelming importance of maximizing the speed of the labor transfer process in an underdeveloped economy. Assume that a country by mobilizing its maximum savings potential would just be able to buy and maintain the "miraculous machine" that is operated by a single worker who pushes buttons and produces national income in unlimited abundance. I submit that the machine would be purchased. The assumption is silly but I think it helps to establish a principle; namely, the definition of successful development should include some explicit reference to income maximization over time. Whether maximum speed of transfer is always a concomitant of income maximization is open to question. I think it is not. But even if it were, it seems to me, the criterion as presented puts the carriage in front of the horse. Another point I wish to make is concerned with the capital-labor ratio. While I am in full agreement with the paper's emphasis on the importance of promoting labor-intensive innovation, I do not see that the same reasons that favor the fullest possible use of labor also militate against the application of capital-intensive technology in general. One of the chief advantages of the underdeveloped countries lies precisely in their ability to adopt the most modern technology developed abroad. The thesis can easily be illustrated by a series of historical examples, among them Japan. Capital-deepening in a modern industrial sector and large-scale use of unemployed rural labor with little capital (e.g., in earth-moving operations) may go hand in hand. To characterize such a process as capital-shallowing on the basis of average capital-labor ratios seems to me less than adequate.

DOUGLASS C. NORTH:* Professor Habakkuk poses two main questions: first, he asks whether population increase during the period was induced by economic change; and, second, whether the population increase was favorable or unfavorable to economic growth.

It is difficult to find in his paper unequivocal answers to either of these questions since all the way through he hedges his bets so carefully that it is very hard to discern his position. But he appears to imply in Section I that population increase was fortuitous from an economic point of view and to suggest in Section II that population increase favorably affected economic growth during the period under study. It is not only difficult to untangle Professor Habakkuk's position but even more difficult to explore the exact relationship he had in mind in the argument, because unfortunately neither the terms nor concepts used are precise and the model in Section I is implicit rather than explicit. In order to explore the second question, it is first necessary to attempt to make the terms more precise and develop an explicit model that appears to mirror what Habakkuk had in mind.

Habakkuk suggests that there existed two standards of life: one was a physiological minimum and the other a customary standard of living maintained by marital customs and family structure. This customary standard of living was, I must conclude, one that presumed a constant living standard per capita. He then suggests that fortuitous factors, such as a period of good or bad harvest or an era of epidemics or a lack of epidemics, could lead to changes in the population not warranted by long-run changes in what he calls resources, and then—according to his argument—preventive or positive checks operate to readjust the population to the customary standards. That is, if there has been a period of abnormally good harvest through fortuitous circumstances and population increases, then the preventive checks should operate through marriage and family structure to readjust population to this long-run customary standard when the good harvests ended, or indeed if the standard of life had fallen to the physiological minimum then positive checks of the Malthusian type would act to reduce population.

The model implicit in this set of notions, as I see it, is that there exist two schedules: one a schedule of a physiological minimum between population and total output, and the other a schedule of a customary living standard which would show a relationship between total output and population such that there would be a constant per capita income and output along this schedule. Let me illustrate from the accompanying diagram the relationships as I see them.

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* I am indebted to my colleagues in our Workshop in economic history and development at the University of Washington for discussion of some of the issues involved in this paper.

Line *S* on Figure 1 represents the physiological subsistence level, and line *C*, below it, represents the customary living standard which, as indicated above, would be a constant per capita income or output. While I have some doubts about being able to express the physiological subsistence standard in this form, it does seem to be what is involved in the model here, and will serve for purposes of exposition.

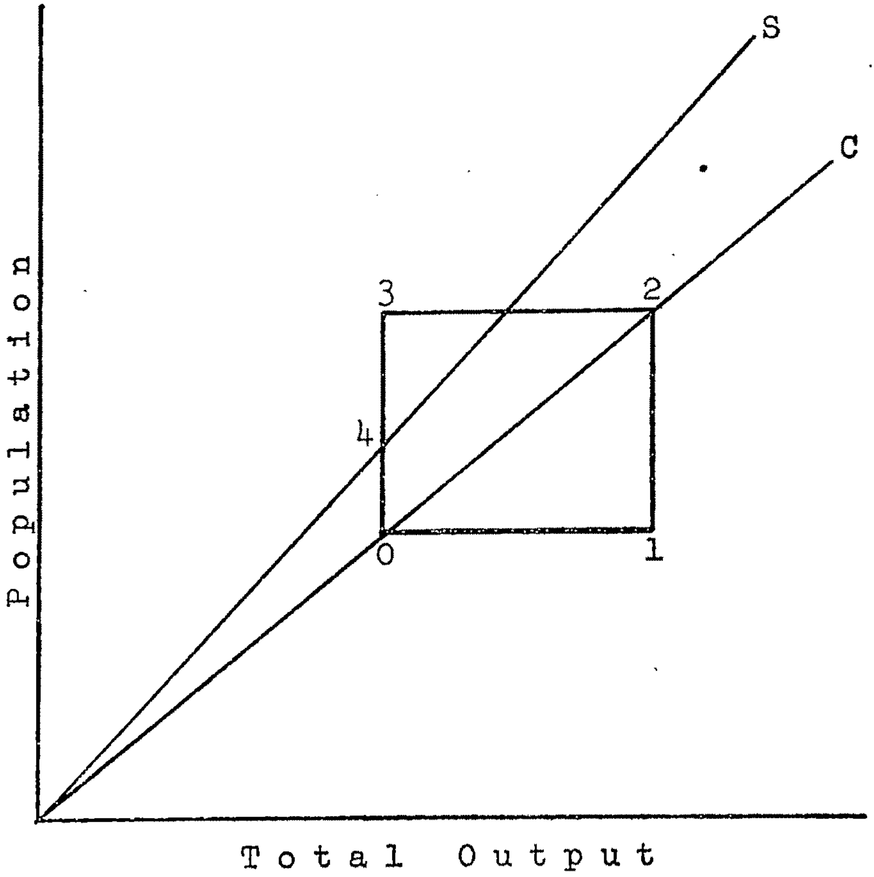

FIGURE 1

Now let us suppose that a fortuitous circumstance, in Professor Habakkuk's definition, occurred such that there are several years of good harvest leading to a temporary increase in total output from 0 on the *C* schedule to 1 which lies off the *C* schedule. As a result, population would adjust to this new level of total output, to 2 on the *C* schedule, reflecting the same level of per capita output. Thereupon, the fortuitous and by definition temporary circumstances end and total output falls back to 3, which puts it above both the C and the S schedules. At this point positive checks would tend to reduce the population to 4 on the S schedule, and still further preventive checks in terms of readjustments—such as, later marriages, smaller families, etc.—would further readjust population back down to the C schedule (although not necessarily at point 0 as indicated in Figure 1).

Similarly, there could be a reverse phenomenon in which an initial epidemic lowered population below the C schedule, and preventive checks, now operating negatively—that is, inducing earlier marriages and more births—would then readjust population back up to the C schedule.

I am certainly not competent to judge whether a model such as this did in fact operate in the centuries before 1750, as Professor Habakkuk and others have suggested. But let us look at the period from 1750 to 1850 and explore the issues in terms of Professor Habakkuk's second question, in which he tentatively suggests that the expanding population in fact induced economic growth; that is, that fortuitous circumstances initially led to population expansion and that ultimately these proved to have induced sustained economic growth. Habakkuk suggests that there were three reasons why, given the initial fortuitous expansion of population, it induced sustained growth in this period: (1) because an elastic supply of labor led under his assumption to an increase in profits and since—again under his assumptions—profits were the source of accumulation, it led to greater capital formation; (2) it led to increases in the size of markets and therefore to economies of scale; and (3) the bottlenecks induced by the pressure upon "resources" stimulated technical progress. Professor Habakkuk leaves to us which of these inconsistent consequences did in fact exert leverage for economic growth.

However, there is a more serious inconsistency in his argument. The model before 1750 implies a stable C function, but evidently after 1750 it becomes unstable for reasons that are not clear. That is, there have been fortuitous expansions in earlier periods which produced no such results (not to mention the fortuitous expansions in the rest of the world in the past century and a half which do not appear to have resulted in such response), and there is nothing in the above model to suggest such an independent role for fortuitous population change. In fact, quite the contrary, the role of population in the model is to adjust to a given per capita income. As a result any initial stimulus induced by a decline in per capita income such as the technical innovation of the steam engine would raise per capita income above the customary standard but would be followed by a population adjustment to the customary standard. Clearly, the expansion in per capita output which occurred stems from variables outside Professor Habakkuk's model. No one would deny the importance of technical change or economies of scale, but it is necessary to introduce into the analysis other variables than population growth to account for technical change and economies of scale rather than simply another fortuitous expansion of population. Professor Habakkuk does mention some other factors but not in a fashion that permits their embodiment in a rigorous explanation of economic growth.

A more promising approach would be to hypothesize that the growth of

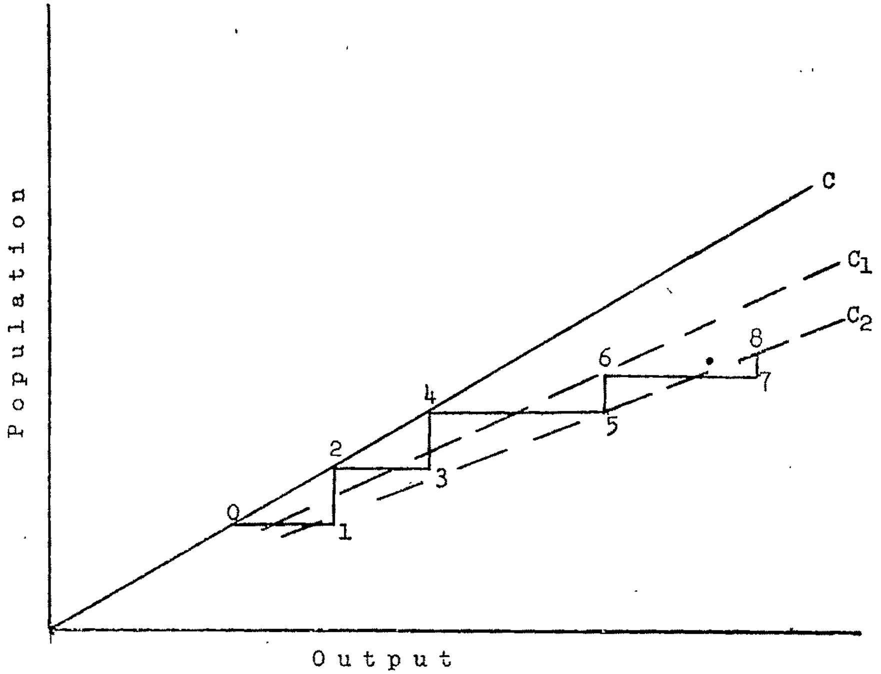

FIGURE 2

science, greater political stability, the expansion of market economies, and other factors led to an increase in capital investment (broadly conceived to include all types of productivity-raising outlays); and here I would suggest explicitly that we not confine attention to industrial revolutions or to agricultural productivity or to transport and marketing improvements but to the growing proportion of national product made up of such outlays and the consequent expansion in per capita output which was surely occuring. Even Professor Habakkuk's initial expansion in population, which he looks upon as fortuitous, was probably nothing of the kind. Instead, it was more likely induced by increases in output that were warranted in the long run as a result of productivity increases, not just those of the industrial revolution, but in agriculture, transport, and marketing as well; not just from mechanical innovations, but from returns on "widening and deepening" human knowledge and economies of scale.

If Professor Habakkuk's model does in fact mirror the pattern before 1750, then the pattern thereafter, I would suggest, is one similar to John Stuart Mill's view in which output expansion as a result of productivity increases led to rapid population expansion which for a time managed to readjust to the customary living standards, but that the growth of output occurred at such a rapid pace that the lag in population adjustment eventually led people to adjust to new customary standards. In the late eighteenth and first part of the nineteenth centuries, population adjustments did move the economy back almost to the old customary standard (accepting Professor Habakkuk's view that per capita income grew very little), but by the middle of the nineteenth century there were evident shifts in the C curve. In Figure 2 the late eighteenth and early nineteenth centuries would be represented by output increases 0 to 1 and 2 to 3, with population adjustments from 1 to 2 and 3 to 4. Thereafter, output grew so rapidly from 4 to 5 and 6 to 7 that population adjustments were only partial and new customary living standards of C_1 and C_2 resulted.

IRMA ADELMAN: Briefly stated, Professor Ranis' contention is that, in a labor surplus economy, the rate of growth of industrial output (as well as that of industrial labor usage) will be maximized by the adoption of innovations which are primarily labor intensive. Professor Ranis makes his argument without regard to the rate of over-all technological progress, the manner of financing investment, and the precise capital-labor and capital-output ratios which characterize the economy in question. Inherent in the argument is the implicit assumption that the rate of capital accumulation is independent of the technical character of innovations.

This last assumption cannot be valid in the general case. For, with wages at subsistence, investment must be financed out of non-wage income. Thus, unless output is raised by the precise amount of the increase of the labor expense resulting from an innovation (the marginal labor-intensive innovation), savings (and hence investment)will be a function of the nature of the innovation. A labor-intensive innovation which increases proprietary and entrepreneurial income will, of course, be adopted under pure profit motivation (in the absence of a superior competing innovation). On the other hand, a labor-intensive innovation which decreases profits will reduce the rate of accumulation. Whether, on balance, the net effect of adopting such a labor-intensive innovation will be to increase or decrease the rate of growth of the economy depends upon the extent to which the expansion in product resulting from a greater employment of cheap labor outweighs the loss in output which will follow from the diminished rate of accumulation.

As the capital-labor ratio is lowered, it is clear that the negative effects upon output of increased capital accumulation must begin to override the advantages of additional employment of labor. This follows because, with a given state of technical knowledge, the most productive labor-intensive innovations will be adopted first. The higher the labor-capital ratio, the more likely it becomes that the (decreased) marginal product of labor relative to that of capital resulting from the introduction of additional labor-intensive innovations will be unable to compensate for the reduction in the rate of growth of output associated with the decreased rate of investment. It follows, therefore, that, even under conditions of unlimited supply of labor at subsistence wages, there must be an optimum labor intensity of production. Utilization of additional labor-intensive innovations beyond that point must decrease the rate of growth of total output.

In general, it would appear that all that can really be demonstrated is that there is a range of capital-labor ratios within which the adoption of labor-intensive innovations that actually decrease profits and investment will increase the rate of growth of output of the economy.

Professor Ranis' argument would appear to be suggestive, however, for the efficient use of foreign aid funds and of investment capital acquired through taxes, forced savings, and foreign loans. The availability of such sources of capital would permit a greater use of labor-intensive innovations before the induced effects on accumulation decrease the rate of growth of output than would normal sources of internal investment funds. It is doubtful, however, whether such a prescription for foreign aid or government investment would be politically attractive in an environment in which national pride is intimately associated with the capital intensiveness of investment.

Incidentally, if there are scarce resources other than capital, such as managerial or entrepreneurial talent, a similar argument can be invoked to limit the range over which labor-intensive innovations ought to be adopted.

ECONOMIC EDUCATION

ECONOMIC LITERACY: WHAT ROLE FOR TELEVISION?

By JOHN R. COLEMAN
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"Economic literacy," so recent a concern for most of us, already has a touch of faddism to it. Indeed, 1962 could be the year when this new star reached its zenith before burning out; or it could be the beginning of a broader, more sustained program to improve the economic understanding of masses of Americans. The choice rests in considerable part with our professional economists.

Interest in selling economics to Americans is not new in itself. Special groups have long championed and promoted their own versions of economics, but their approaches often had more propaganda than education in them. What is relatively new is the zeal of leading economists for an objective and rational approach to their field of study in the secondary school curriculum.

A leading example of this latter emphasis is the report of the National Task Force on Economic Education.[1] A second and closely related development is the college credit course, "The American Economy," currently being shown over 241 television stations across the country. The Task Force's efforts gave much of the push to get the course on the air; their findings gave the basic list of concepts included in the course for minimum economic literacy as a part of effective citizenship; and the Task Force members served as advisers to the National Teacher of the course.[2]

What has been learned as a result of this use of television? And where do we go next to ensure that, far beyond faddism, there is a sustained forward thrust to widespread and objective economic education?

The Assumptions for "The American Economy"

Four assumptions—explicit and implicit—on which this televised course was built are important in assessing its effectiveness:

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[1] *Economic Education in the Schools* (New York: Committee for Economic Development, 1961). The Task Force members were G. L. Bach (Chairman), Arno A. Bellack, Lester V. Chandler, M. L. Frankel, Robert Aaron Gordon, Ben W. Lewis, Paul A. Samuelson, and Floyd A. Bond (Executive Secretary).

[2] But responsibility for the day-to-day content of the course rested with the National Teacher, the author of this paper. Throughout the year of developmental and filming work, he was aided by Professor Kenneth O. Alexander, of the Michigan College of Mining and Technology. Cosponsors of the course with the Task Force were Learning Resources Institute, the American Economic Association, and the Joint Council on Economic Education.

1. The course is assumed to be the equivalent of one year's introductory work in economics for nonspecialists at a quality college. Its aim is thus to go just beyond that minimum degree of economic literacy described in the National Task Force report.

2. The course surveys the same broad ground found in most of the widely selling economics textbooks. The ordinarily unacceptable answer of "both" has been given to such questions as: Should the course concentrate on micro- or macroeconomics? Should we deal with institutions or with concepts in economics? Should the emphasis be upon building tools of analysis or on applications of tools to contemporary problems? Granted that one may develop sophisticated understanding of many subjects by rigorous, disciplined study of one cornerstone in each such subject, the hazards in so doing are great. Which cornerstone in economics? What is the likelihood that the student will carry his new understanding in depth over into others of the complex areas of economics in the world about him? So long as the student is motivated to learn by a curiosity over something in his own experience, we run the risk of leaving him uninterested in economics if the bridge between his area of concern and the instructor's choice of a vital cornerstone is not built. Yet, with all of this, the television course still leaves much unsaid, as such varied correspondents as the consumer finance companies, the profit-sharing industries, the foreign exchange experts, and all the devotees of marginal analysis have been quick to point out.

3. The key target in the television classroom has been the secondary school teacher of social studies. With no live students to look at in the studios and with two red camera lights an unsatisfactory substitute, we have drawn our own image of the audience: intelligent, conscientious high school teachers who live each day with a curriculum studded with economic concepts but who in most cases have had not one formal course in economics. One in five of our lessons is conducted by a specialist in economic education[8] who addresses himself to the task of translating economic concepts and good intentions into classroom applications. The final critical measure of our success or failure in using so many resources for this course thus will be the extent to which we are helpful in getting a more informed, more rational, and more exciting approach to economics into the high schools.

But it was not our intention to teach just the economics that the high school teacher, following the Task Force recommendations, might wish to teach in the classroom. We went beyond that minimum in the belief that first-rate teaching requires that the instructor be several long jumps ahead of the best students in the class. Thus, for example,

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[8] Dr. John H. Haefner, Professor of Social Studies Education and Head of Social Studies, University High School, State University of Iowa.

160 separate days, to pursue a noncompulsory activity. To the outside world, the television teacher may insist that he is assuming continuity in his audience; in truth, he knows that he cannot safely assume that continuity and that he must repeat key points more often then he might wish.

The teaching techniques that work so well in the campus classroom are often inappropriate on television. To a greater extent than most of us realize, we teach by a raised eyebrow from one student and nods of approval or even of slumber from others. But television's feedback is necessarily so slow in coming as to lose most of its effectiveness for the course at hand, however much it may improve the next course. Moreover, in motivating the classroom student, one day we may use the technique of provoking him into independent thought through the instructor's deliberately extreme position, and next day we may hang the whole class period on a peg selected from the morning newspaper. Deliberate controversy seems unwise to me in a television course where the viewer has such limited opportunity to rebut the instructor; and news pegs suffer from severe dating by the time the televised lessons are shown. The net result is a ceaseless battle against blandness and irrelevancy—a battle in which the instructor gets only belated and scattered hints as to whether or not he has held the students' interest and deepened their understanding.

One seeming disadvantage of televised teaching of economics seems to me on closer examination to be a decided advantage. The shortness of the class period (twenty-eight minutes are cruel to professors who habitually think in fifty- or ninety-minute blocks), the limitations on what can be shown on the screen at one time, and the unevenness of the viewers' advance preparation force the television teacher to simplify beyond what he has ever done before. This is initially damaging to his picture of himself in a profession where elegance of argument rates as high as it does in economics. But there turns out to be a satisfaction in simplification itself. I sense that the guest lecturers leaving our studios with the greatest feelings of accomplishment were those who surprised themselves by their own ability to communicate that which is nearest to them in simple language. They, and the Task Force, too, have proved what we came perilously close to forgetting; namely, that economics need not always be either dull or abstruse.

I can do no better here than to echo with approval what one of the National Task Force members said of their own work:

> A superficial reader's first impression may be that the Task Force has merely summarized what is in the standard sort of college textbook in elementary economics. I hope that a more careful reading will dispel this impression. We are careful to indicate what is essential and what can be safely omitted. We put up red flags to warn the teacher when the usual way of dealing with some involved topic is likely to confuse the student. We stress

an elementary understanding of the broad and essential features of our kind of economy and of the important ways in which it differs from alternative ways of organizing economic life. We try to allow for the fact that much of what we think is necessary will be taught, if it is taught at all, in history and general social studies courses by teachers wtih a very limited background in economics.[7]

The Need for Follow-up

Let us suppose that evaluation of the television course ultimately supports our two early hunches; namely, that the numbers watching the course with some regularity are high in comparison with earlier offerings of credit courses and that the numbers following through for credit remains disappointingly small given the magnitude of the job to be done in promoting economic literacy. What conclusions can we then draw?

These results would seem to underscore the critical importance in economic education of more effective bridges between the colleges and the secondary schools. I suspect that the communities from which we will continue to draw the best evidence of a localized impact from the television course are those where affiliates of the Joint Council on Economic Education are most active. These active affiliates patiently and imaginatively worked at the task of promoting enough advance interest among the teachers in their area to ensure a follow-through once the series began. Their experience demonstrates that the day the course begins is much too late to think about utilizing it. They perform for this new course the same functions that an established college's catalogue, reputation, and information service perform for its academic offerings. Neither this nor any other televised course can sell itself for credit; nor can it be sold by nationwide advertising. Its acceptance and utilization demand hard advance work at the community level by college teachers and secondary school teachers and administrators.

Nor is this advance work all. The television course once begun can scarcely stand on its own feet, for economics fares badly when it is solely a "talking-at" subject. Basic concepts and the key policy alternatives flowing from those concepts can appropriately form the subject of lectures, in the classroom or by television. But, at some point, the student-viewer needs a chance to clarify his doubts, to test his own policy choices out loud, and to participate in the give-and-take of a well-conducted discussion session. This is the missing ingredient in television teaching. It can only be supplied by competent campus teachers who meet face to face with the viewers from time to time and who use the televised lessons as starting points for their discussions.

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[7] R. A. Gordon, *Economists and Economic Education* (Joint Council on Economic Education, 1961).

The problems in getting campus teachers to do this job well are great. The blunt truth is probably that this kind of supplementary teaching requires at least as careful advance preparation as does a conventional course without television. Yet there must be temptations to assume that television has done the core job that needs to be done and hence to use the limited campus hours to pursue the instructor's favorite refinements which were left out of the films. Indeed, what incentive is there for the campus teacher to submerge his particular specialization in the development of just that discussion and clarification which may add most to the total course impact? I submit that there can be no meaningful incentive short of shared convictions of the television and campus teachers on the objectives of the course and the importance of welding the two parts together. Such shared convictions in turn will arise only where we make it abundantly clear in our profession that we consider this job worth doing well.

But why should economists in our colleges and universities concern themselves with this promotional and supplementary educational activity?

The case for our concern with aiding the secondary school teachers in doing their jobs comes down to three propositions: (1) the interest in economics, now running at a high level in the secondary schools, will not—and should not—wither; if we remain aloof from it, we will not slow down the interest as much as we will divert it to frankly less objective sources of assistance; (2) most high school students will never enter college economics classrooms; it seems preferable to fight for a minimum of economic literacy at this stage in their lives rather than to battle against their economic illiteracy for the rest of their lives and ours; (3) those high school students who do come into our classes later will be a more exciting group to teach, even in the introductory course, if we can assume a fuller exposure in tomorrow's high school classes to key economic concepts objectively presented. Our self-interest as teachers and as citizens in a decentralized economic system then suggests the wisdom of our intelligent participation in the improvement of high school education. I stress "intelligent" participation; let no college economist assume that the high school teachers are breathlessly waiting for him to tell them how to do their job. They are experts, even as he is, and that means clashes in expertise whenever the outsider, the college teacher, enters. If we would be helpful to the high school teachers, we will have to start by knowing their problems and respecting their professional pride.

The kinds of things that we can do to capitalize on the current interest in better economic education in the high schools are numerous: learning about the existing high school curriculum in social studies and associating ourselves with the teachers' own groups working for curriculum reform; aiding in translating the Task Force report into action terms for the existing or revised curriculum; helping the teachers to work their way through the mountains of economic materials being pushed on them for classroom use; promoting reruns of the television course on a local basis, employing intensive advance work with high school teachers to enlist their participation for credit in the course and supplementation of the films by competent, imaginative, economic instruction on the campus.

The most that we can expect by way of a direct legacy from the television course on "The American Economy" are a heightened interest in and excitement about the world of economics, and a library of film materials which, judiciously used, can enrich the classroom education of today's students and the teachers of tomorrow's students. But perhaps that is legacy enough. Both the excitement and the materials can be used again and again if we so will it. The rest is up to those of us who have the biggest stake in the economic literacy of our fellow citizens.

ELEMENTARY ECONOMIC EDUCATION

By GEORGE J. STIGLER
University of Chicago

The Task Force appointed by the American Economic Association and financed by the Committee for Economic Development has reported on the problem of instruction in economics in the high school. The Task Force contained five highly esteemed economists, G. L. Bach (Chairman), Lester Chandler, R. A. Gordon, Ben Lewis, and Paul Samuelson, as well as two specialists in secondary education, Arno Bellack and M. L. Frankel. Although the American Economic Association explicitly does not endorse the report, it chose the economists and hence certified to their competence and distinction. No other body concerned with economic education has had such professional status, nor is an equally authoritative body likely to appear in this decade. The report will therefore command an audience of unusual size and deference among high school and college teachers.

This unusual status is, I believe, unfortunate. The economists composing the Task Force are leaders in American economics, of unquestionable competence and integrity. But they do not monopolize these virtues, and their special mode of selection serves to give their views an eminence which is incompatible with the doctrine of free competition (free discussion) in educational and scientific work. The views of the Task Force will receive a respectful but critical reading by professional economists; they will not receive anything like so critical a scrutiny by the people in charge of elementary economic education. If —to use an unbelievably remote possibility—I or some other professional reviewer convicted them of error, the important audience would never learn of it.

To this lament on professional sponsorship there is a clear and weighty response. If there is not an authoritative professional influence on high school instruction in economics, what other sources of influence will prevail? The answer is: a strange medley of (1) teachers and school administrators long on pedagogy and short on economics, (2) special interest groups who flood our schools with variously slanted materials, and (3) a rearguard of textbook salesmen and opinionated local school board members. And has not this strange and not always open competition led to a generally unsatisfactory neglect and/or treatment of economics in the high school?

I have no counterresponse to offer. I even believe that the Task Force's schemes are better by almost every economist's values than those which now on average prevail. It is for each person to decide whether benevolent monopoly is ever justifiable in the realm of education. My own preference is clear: of all monopolies, that of opinions is worst.

Let us abandon this question and look at two other problems which the report raises. The first concerns policy slant; the second concerns pedagogical feasibility.

Readers will know that the report has already been criticized as absurdly neutral toward communism and filled with soft welfare state views—by the conservative press. They can conjecture—as I do, since I missed the *Daily Worker* that day—that the other extreme thinks the Task Force sold out to Wall Street.

I do not propose to discuss the political slant of the report in any detail. It is fair to say that the committee which wrote the report does not include a representative of the extreme 10 per cent of economists at each end of the distribution of economists—neither fervent free-market disciples nor extreme interventionists were represented. Since the committe consisted of men who are representative of the central range of professional opinion, one can hardly call its position unrepresentative.

This does not mean that the report is in any sense "impartial." Indeed, I do not know anyone who is impartial or, if he is, how we can know it. I strongly disagree, for example, with the implicit verdict of the Task Force (and presumably of the profession) that although the problems posed by monopoly deserve extensive discussion (pages 32-36), no comparable issues are raised by public regulation (page 36). But obviously neither a Professor Bach nor I can presently persuade the other that he is wrong or—being honest men—we would have removed our differences earlier. The only legitimate complaint—and I promise not to recur to this point again—is that some (representative) views have been given a preferential status among noneconomists.

Let us turn finally to what the Task Force believes should be in the high school course in economics: "The minimal understanding needed for effective citizenship in the modern American economy" (page 22). Here full quotation is essential, but the reader will find it sufficient to sample the list. Only their explicit demands for inclusion of topics are listed, although this rule omits subjects which the Task Force believes to be very important (e.g., page 52), but probably for reasons of elegant variation chose not to introduce with a "Students should know. . . ."

> It is important that students understand the central role which markets and prices play in this process [of selecting the goods to produce and the way in which to produce them]. (Page 28.)

. . . .

Students should have a general picture of the national stock of such resources, and of the way in which this stock has grown over the year. (Page 28.)

. . . .

Students should see that the high American standard of living depends on a combination of all these factors [governing productivity]. . . . (Page 29.)

. . . .

. . . the student should understand the relationships between *saving, investment,* and *capital formation.* (Page 29.)

. . . .

Students should also understand, in broad terms, the *principle of diminishing returns.* (Page 30.)

. . . .

It is important for students to see that there are three main types of participants in economic activity. (Page 30.)

. . . .

Individual freedom of choice is central to the "private enterprise way." It is essential that students understand both what this freedom does and does not mean, and also how a private enterprise economy manages to get its business done when individuals are free to make their own economic decisions. (Page 31.)

. . . .

To comprehend the working of markets, the student must understand the main determinants of demand and supply. (Page 32.)

. . . .

Students should know something of the intent and nature of laws designed to enforce competition, and should understand something of the process by which such legislation is enforced through the courts and federal administrative agencies. (Pages 34-35.)

. . . .

Students should be familiar with these trends [in competition] in a general way and, particularly in their history courses, should have the opportunity to look briefly at the development of trusts, holding companies, and other forms of mergers over the past century. (Page 35.)

. . . .

In connection with corporations, it is desirable for students to have a grasp of a *simple* balance sheet and income statement, reflecting only the four or five major categories in each. (Page 36.)

. . . .

Students should also understand the important function performed by securities exchanges, which permit investors to buy and sell readily securities which have already been issued. (Page 36.)

. . . .

It is important for students to understand that, even in a basically free enterprise economy, governments play a significant role in setting priorities and using resources—that is, in deciding what to produce and how to produce it. (Page 37.)

. . . .

Students should have some impression of the order of magnitude of government spending, which now accounts for over a quarter of the national income. (Page 38.)

. . . .

Students should have a general impression of the major taxes currently used. (Page 39.)

. . . .

. . . *it is important that the student be introduced to the study of international economic relations.* (Page 39.)

. . . .

Students should be given a brief introduction to the concept of the international balance of payments. (Page 40.)

. . . .

One point that should be emphasized with students is the difference between the *balance of trade* and the *balance of payments.* (Page 41.)

. . . .

Students need to be introduced to the fact that many of our greatest economic problems center around how to obtain stable economic growth, avoiding the excesses of the inflationary booms and depressions. (Page 43.)

. . . .

Students should be familiar with the concept [of gross national product], although technical details should be avoided. (Page 44.)

. . . .

Students need to be impressed with the fact that this element of *dynamic interdependence* is a highly important feature of all money-using, private-enterprise economies. (Page 46.)

. . . .

Students should see that the burden imposed upon the taxpayer by a growing public debt depends largely on how rapidly the national income is rising. (Page 47.)

. . . .

Although students should be introduced to money and banking, it is too much to hope that they can master all of the intricacies about them which are found in college textbooks. (Page 48.)

. . . .

Every informed person should understand how new deposits are created when banks make loans or buy securities, and how deposits are correspondingly reduced when bank loans are repaid or the banks sell securities which they have been holding. (Page 49.)

. . . .

The Federal Reserve can control the reserves of the commercial banks which the latter must keep as backing for the public's deposits. Students need understand in only a very general way how this can be done, by raising or lowering commercial bank reserve requirements and through "open market operations." (Page 50.)

. . . .

Students need to understand that we should not expect monetary management to do things that it cannot do, and that we should not damn it for sins of which it is innocent. (Page 50.)

. . . .

But students should be able to see that if a country loses gold, it is likely (or may even be forced) to tighten credit conditions. (Page 51.)

. . . .

It is important for students to understand the vast difference between conditions here and in the underdeveloped economies, and to appreciate some of the problems such economies face. (Page 53.)

. . . .

Students should understand how money incomes arise out of the sale of productive services by individuals to businesses and governments. Equally important, they should understand that these money incomes provide the means of obtaining "real income"—that is, the goods and services that people buy with their money incomes. (Page 55.)

. . . .

Students should know in broad outline of the distribution of personal incomes in the United States, including their own place in the income structure. (Page 56.)

. . . .

Students should understand that high American wages rest fundamentally on the high productivity of American labor. (Page 57.)

. . . .

Every person should understand something of the role of unions in labor-management relations. (Page 58.)

. . . .

The "farm problem" should be understood broadly by students. (Page 59.)

. . . .

Students should be generaly familiar with these various arrangements that directly affect their personal economic security. They should also be able to reason objectively about

> the economic effects, and hence the desirability from different points of view, of these different types of security measures. (Page 60.)
>
> . . . .
>
> *Every informed American should have a general impression of how other types of economic systems operate, especially communism.* (Page 61.)
>
> . . . .
>
> In addition to being able to compare private enterprise and communism, the student should also be sensitively aware of the fact that various democratic societies—the United Kingdom, the Scandinavian countries, and India, for example—have chosen to adopt socialism in varying degrees. (Page 63.)

Amen!

This list of things a high school student should know is, of course, a representative table of contents of a textbook on elementary economics at the college level. Since high school seniors are slightly less mature and substantially less selected than college freshmen, and since high school economics teachers are appreciably less well trained in economics than the instructors in college courses, I take it as rigorously implied that if our present college courses are failing to teach students economics, the proposed high school course will fail by a greater margin.

This is in fact my thesis: The watered-down encyclopedia which constitutes the present course in beginning college economics does not teach the student how to think on economic questions. The brief exposure to each of a vast array of techniques and problems leaves with the student no basic economic logic with which to analyze the economic questions he will face as a citizen. The student will memorize a few facts, diagrams, and policy recommendations, and ten years later will be as untutored in economics as the day he entered the class.

This is a bold pronouncement and gains no authority by coming from a teacher who is many years away from his last course in elementary economics. The thesis will be rejected more completely, I believe, by the teachers of elementary economics than by the textbook writers. It is instructive that the successful textbooks keep adding subject matter to their successive editions in response to market demands.

But the thesis that our present college courses (and a fortiori the proposed high school course) fail to impart any permanently useful economic training runs against faith, not evidence. I propose the following test: Select an adequate sample of seniors (I would prefer men five years out of college), equally divided between those who have never had a course in economics and those who have had a conventional one-year course. Give them an examination on current economic problems, not on textbook questions. I predict they will not differ in their performance. I shall illustrate below the kind of question that should be asked in this test.

An introductory-terminal course in economics makes its greatest contribution to the education of students if it concentrates upon a few subjects which are developed in sufficient detail and applied to a sufficient variety of actual economic problems to cause the student to absorb the basic logic of the approach. The few subjects should be chosen with a view to their importance (which includes the frequency with which they are encountered) and to what present-day economics has to contribute to their understanding. Both criteria, I believe, argue for an immense concentration on price theory: the allocation of resources and the pricing of goods and services.

It seems almost beyond dispute that the citizen is called upon overwhelmingly—indeed, almost exclusively—to pass on policies governing the allocation of resources and the formation of prices. I consider this proposition to be self-evident in the area of state and local governmental policies; and it is hardly less true of national economic problems. I made a more or less complete count of the bills which were passed by the first session of the 81st Congress (1959) to get the following tabulation of bills with a strong or exclusively economic interest:

1. Disposal of national resources and Indian lands, at least 71, of which general bills numbered 5.
2. Regulation of economic activity: agriculture 13, merchant marine 7, housing 6, highways 6, labor 7, other (banking, air pollution, imports, etc.) 22.
3. Taxation (individual taxes) 9.
4. Tax exemption 5.
5. General appropriation acts 16.
6. Selective expenditures: salaries 12, veterans 11, other 29.
7. Monetary policy 3.

To the extent that the economist has anything to say in these areas, he uses price theory ninety-nine times for every instance in which income and employment theory has any relevance. Questions directed primarily to maintaining employment are posed so infrequently and are even then (as in the appropriation acts) so completely dominated by allocative questions that for practical purposes they are an infrequent and minor problem for the citizen.

If the elementary course were to recognize the civic pre-eminence of traditional value and distribution theory, it would spend much less time on other parts of economics. And if it recognized the immense variety of allocation problems and the impossibility of giving a mastery of detailed techniques to the one-year student, it would concentrate on the essential logic of competition and monopoly, of consumer choice, and of the theory of wages. The student would be taught, not elasticities or envelopes or oligopolistic models, but the central issues of analysis and fact in questions such as:[1]

H.R. 236 To prohibit discrimination because of age in the hiring and employment of persons by government contractors.

H.R. 1251 To authorize a five-year program of grants and scholarships for collegiate education in the field of nursing.

H.R. 2895 To grant a pension of $100 a month to all honorably discharged veterans of World War I who are over sixty-two years of age.

H.R. 5983 To enable producers to provide a supply of turkeys adequate to meet the needs of consumers, to maintain orderly marketing conditions, and to promote and expand the consumption of turkeys and turkey products.

H.R. 7714 To define, regulate, and license television service contractors.

H.R. 9038 To amend the Tariff Act of 1930 to provide for the establishment of country-by-country quotas for the importation of shrimps and shrimp products.

S. 215 To supplement the antitrust laws of the United States by requiring that corporations, in industries so heavily concentrated that monopoly or the threat of monopoly is present, file advance notice and make public justification before effectuating price increases.

I proposed above a test of whether our present elementary courses had any lasting value. The questions that ought to be used in such a test are precisely the current questions of policy. Give the student a summary page or two of the arguments and evidence presented in the discussion (in Congress and the public press) of H.R. 5983 (orderly turkeys) and let him explain benefits and costs of the scheme—with the grading based, of course, on the coherence of his argument and relevance of his evidence, not on the conclusions reached.[2]

Let us hope that the test would reveal that our present elementary courses do not have a zero, or negative, value. The test would still leave open the question: Would intense concentration on the basic logic of price theory, applied to two score of real problems, give a vastly better and more lasting training than the current encyclopedic texts? Surely, the answer is yes.

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[1] The list is from unsuccessful bills of the first session, 81st Congress, but the headlines of the newspapers will do as well.

[2] Such questions can be preceded by simpler ones such as that which Senator Symington recently flunked: If stockpiles of materials owned by the government are given directly to defense contractors rather than sold in the market, will we not be able to dispose of them without depressing their prices?

TASK FORCE TO CLASSROOM*

By LEWIS E. WAGNER
University of Illinois

The report of the National Task Force on Economic Education,[1] trumpets a "call to arms" in the service of more widespread economic understanding. Implicit in this call is the view that the time has come to redress the balance between efforts devoted to the creation of new ideas in economics and energies spent on their dissemination. The public schools, especially the high schools, are identified as the primary vehicle for "carrying the message." In the words of the report, "the primary mission of the Task Force was to describe the minimum understanding of economics essential for good citizenship and attainable by high school students, with the goal of providing helpful guidelines for high school teachers, administrators, and school boards." (Page 4.)

At one level the impact of the report is clear. It is serving as a catalyst for encouraging the careful thought and discussion which must precede individual and group action. For the immediate future, efforts to improve economic education will be influenced importantly by recommendations embodied in the report of the Task Force. The report of the Materials Evaluation Committee, the National Television Course, the design of new tests of economic understanding, the policy statement of the Committee for Economic Development on economic literacy, new plans for instructional materials on the drawing boards of commercial publishers, the substance of recent workshops, conferences, and professional meetings—all of these bear testimony to the already strong influence of this report in some quarters.

At another level—the effect on the teaching of economics in the schools—the influence of the report is less apparent. The concern of this paper is this second level: the influence of the report on classroom practice. The approach is based on two themes: first, the extent to which the statement of the Task Force provides an orientation that is operationally workable in the sense that its broad recommendations can be translated into a viable, concrete program; second, what is needed to move from recommendation to practice.

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* In the preparation of this paper, I have been helped by frequent discussions of the Task Force Report and the problems of economic education with Clark Bloom, James Calderwood, Harris Dante, Robert Darcy, John Haefner, Meno Lovenstein, Jim Reese, and Lawrence Senesh; but they, of course, bear no responsibility for the result.

[1] *Economic Education in the Schools,* Report of the National Task Force on Economic Education, Committee for Economic Development, 1961.

Nature and Orientation of the Report

The report presents a cogent statement of the economic content to be taught and the kind of understanding to be achieved. The recommendations of the Task Force, however, are in the nature of a broad statement of goals and objectives to be sought. They offer no "bill of particulars," no "tool kits," no short, simple set of instructions on "how to do it." The major purpose of the report is to provide a point of reference from which teachers and curriculum directors can proceed. The Task Force makes no pretense of developing fully the "bill of fare" they suggest. Nor do they attempt to specify how the material should be presented in the schools. In the words of the report, "each school and teacher will have to decide what should be taught in what courses, and in what ways." The report is simply a summary of objectives in terms of concepts, tools of analysis, and types of understanding to be achieved. It makes no attempt to answer the difficult and perplexing problems of how much economics should be taught, what specific topics should be taught, how they should be taught, at what grade levels they should be taught, what sequential ordering may be necessary, and how economics will be related to other subjects in the curriculum.

With only two exceptions—an emphasis on a rational, objective approach to economic issues and the importance of a high school course in economics—no clear priorities are established, either in the statement of content or the recommendations for action. Although the report does omit some topics and concepts usually found in the college principles course, it does not indicate in a manner adequate for classroom implementation whether some of the conceptual content which is included is more basic or essential than other content. With few priorities included in the report, teachers will find little assistance regarding topics which might be omitted and how much time should be devoted to those which are included. Nor is there any indication that the effective teaching of a particular concept may require students first to understand a number of other concepts. Much of the necessary elaboration and the qualification of basic concepts has been left for teachers and curriculum co-ordinators to work out for themselves or in co-operation with economists and educators actively engaged in this field. Because of the generality of the report and its failure to establish more detailed priorities, an almost endless variety of courses and study units may be interpreted by teachers as consistent with the basic framework recommended in the report. Reluctance of the Task Force to impose a rigid pattern on the schools is understandable but their own assessment of teacher preparation suggests that teachers will need considerable guidance on these matters if they are to move from the recommendations to effective classroom practice.

The overriding objective pervading the report is the goal of developing in students a rational, objective way of thinking about economic issues. On this matter I am in agreement with the recommendation of the Task Force. The report presents a distinctly first-rate statement of what is meant by rational, objective analysis (pages 14-16) which I am confident will find wide endorsement within the economic fraternity.

Another dimension of the basic orientation of the report is that the economic content appropriate for the public schools should be the minimum understandings necessary for effective citizenship and also attainable by high school students. The omission of experimental evidence and specific criteria also suggests that what is attainable by high school students can be determined on some a priori basis. Finally, the importance which the Task Force assigns to a high school course in economics should be noted as part of the report's basic orientation.

Although the report represents an important landmark in economic education, it by no means marks the end of the journey. Much remains to be done. If the generic goals of the report are to be achieved, a number of issues will need to be more thoroughly explored, and some shifts in the emphasis of the report may be necessary. It is to some of these matters of unsettled business that this paper calls attention.

Basis for Selection of Content

With considerable oversimplification, the dominant theme of the call to arms in economic education has emphasized the importance of widespread economic understanding if we are to meet our responsibilities as citizens in a democratic society. In increasing measure, it is argued, many of the pressing social problems of the day are largely economic in substance or in effect, and that economic understanding is required of each of us as a condition for our common economic and political well-being. Economic literacy is presented as a necessary part of effective and responsible citizenship. Given this justification for economic education, the problem of determining the economic content appropriate for this assignment is interpreted as a matter of identifying those economic understandings necessary for effective citizenship.

I assure you that I am for good citizenship. But we should rid ourselves of the idea that the primary task of economic education is to produce good citizens. Do any of us really know how to make good citizens? Can we specify in operational terms what constitutes responsible citizenship? Can we list the minimum economic understandings essential for good citizenship and demonstrate why these understandings are essential and those which are omitted are not? Given the standard of economic understanding established by the Task Force, I wonder how many "good citizens" we can count within our borders. To what extent is widespread economic literacy of some dimension necessary for the successful functioning of the economy? With what frequency and seriousness has public policy gone astray because of popular economic illiteracy? In principle, these are researchable questions. For the foreseeable future, however, a citizenship orientation does not provide an operationally meaningful guide for determining economic content appropriate for the public schools. This is not to say that instruction in economics has nothing to do with citizenship. Hopefully, economic understanding will contribute to good citizenship, but the contribution is more in the nature of a byproduct than a primary objective, and it is a by-product jointly produced by all subjects in the curriculum, the home, the church, and many other influences, with the respective contribution of each largely unknown.

If good citizenship is to be rejected as a viable reference point for the determination of content, what is to be substituted in its place? I submit for critical consideration a professional orientation; an orientation within which the quidelines will be the fundamental intellectual structure of the discipline of economics. Our objective should be to transmit to the student the vibrant, kicking world of the economist on the frontier of knowledge. Paraphrasing one observer on the reform of content for the public schools, "We should put the student in the place and world of the professional economist, allowing the student to experience the thrills of discovery and the disappointments of failure that are the lot of the professional."[2] Students should emerge from the study of economics endowed with a sense of the beauty and grandeur of the economic order and of the intellectual construct called economics. All of this, of course, is in keeping with the new frontiers of educational thought and recent patterns of curriculum reform in science and mathematics. Hopefully, students participating in this kind of educational experience will also go on to become good citizens.[3] There is no reason to believe that a professional orientation for determining content will prove to be inconsistent with responsible citizenship.

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[2] Evans Clinchy, "The New Education in the United States" (Educational Services Incorporated, 1961, mimeographed), p. 4.

[3] Economic understanding is an important dimension of good citizenship. If students understand economics, they will become better citizens; but understanding economics means grasping the intellectual structure of the discipline rather than a survey of the field.

In this context "fundamental intellectual structure" refers to those concepts and relationships which serve to organize and tie together seemingly disconnected facts and topics into a unified, meaningful system of thought. The wider the applicability of a concept or relationship and the more powerful it is in organizing and relating other facts and ideas, the more fundamental it becomes to the structure of the discipline and the higher the priority it deserves. These are the pervading and powerful ideas that need to be sorted out and accorded a central role in economic education. The concepts of maximization, substitution at the margin, and equilibrium may be mentioned by way of illustrating a few organizing concepts which certainly pervade our discipline but receive little or no consideration when assessed in terms of the "minimum understanding of economics essential for good citizenship."

The issue under review, however, is not the precise nature and outer dimensions of the intellectual structure of economics or even how that structure might diverge from the content outlined by the Task Force, but rather the most promising and workable frame of reference to be adopted as a first step in determining what should be taught in the schools. The position taken here is that a citizenship orientation is operationally unworkable; that what is needed is a professional orientation emphasizing the basic structure of the discipline.

Determining What Is Attainable

The report suggests not only that its outlined content is basic to effective citizenship but also that it is attainable by high school students. This raises a question regarding the extent to which the actual selection of recommended content was influenced by what the Task Force perceived the learning capacities of high school students to be.

The task of outlining basic content, whether it be basic to citizenship or to an intellectual structure, is a far different task than determining what content a given group can learn. If those who attempt to determine appropriate content establish "what they think Johnny can learn" as their orientation, they will tend to be led to include much that is peripheral to the basic structure of the discipline because it appears easy to learn and to exclude some concepts and relationships that are basic because they appear too difficult. A priori limitations to content, established in terms of criteria external to the discipline, provide a poor foundation for curriculum reform and development.

The problem of economic education is twofold: first, to determine what should be taught; second, to determine how it can be taught to students of different abilities at different grades in school. Armchair speculations about what students can learn, however well informed the speculations, are essentially predictions about future achievements. They largely sidestep the second phase of the problem. Assuming economists can agree on the basic intellectual structure of their discipline, the truly exciting challenge for the economist is to discover ways of revamping this structure so that it can be made interesting and understandable to students at various grade levels. And make no mistake, curriculum design faithful to the underlying structure of economics cannot be delegated to the public school teacher or the professor of education.[4] The economist must play an active if not major role. What students can learn is a question of fact to be explored experimentally. The constraints for this exploration should be nothing less than the basic intellectual structure of the discipline. In the words of Professor Bruner:[5]

> We [should] begin with the hypothesis that any subject can be taught effectively in some intellectually honest form to any child at any stage of development. It is a bold hypothesis and an essential one in thinking about the nature of a curriculum. No evidence exists to contradict it; considerable evidence is being amassed that supports it.

Achievements in other areas of the curriculum suggest that basic notions in economics will prove accessible to students at an early age, if they are approached through the student's experiences and conception of economic phenomena and studied through materials he can handle. As economists we pride ourselves on our allegiance to rigor, our devotion to theory, and our concern for analysis. But we also exhibit a strong propensity to equate rigor with systems of equations and geometrical design; to think of theory largely in terms of the tools and technical apparatus of the trade; to associate analysis with deductive theorizing. If we are successfully to move economic content into the public school classroom, we will need to recognize that mathematical expression is neither a prerequisite nor a guarantee of disciplined thinking; that theory is not to be identified with a particular mode of presentation or kit of tools; that analysis extends beyond the logical derivations of a set of assumptions.

It will also be necessary to recognize that "what is teachable" at a particular grade level is a dynamic concept. The introduction and proper handling of economics in the primary and intermediate grades will expand greatly the range of content that is teachable at the secondary level. Concepts and relationships which today stubbornly defy teaching may become teachable as greater attention is given to the reorganization of subject matter and the translation of basic concepts into experiences consistent with the student's way of viewing things. The effectiveness with which certain concepts are presented may also be improved by the testing of pedagogical techniques, the application of new developments in learning theory, and by increased teacher facility with the subject matter of economics. The limits to what can be learned should be set by research and experimentation, not by preconceptions as to what is attainable.

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[4] This does not mean that teachers and professors of education are unimportant and need not be involved in the project. The point is that the economist must roll up his sleeves and stay with the work rather than stand at arms length as a consultant passing judgment at the beginning and end of the effort.

[5] Jerome S. Bruner, *The Process of Education* (Harvard Univ. Press, 1961), p. 33.

Importance of a High School Course

Number one on the list of Task Force recommendations is a plea "that wherever feasible students take a high school course in economics. . . ." The Task Force recommends no particular course arrangement or teaching approach, but they do urge that stress be placed on objective, careful reasoning about economic problems, on understanding the over-all functioning of the economy, and on the major problem areas, institutions, and analytical concepts outlined in the body of the report. And they warn against superficial description and teaching that stresses memorization of trivial facts, dates, and unused lists of concepts.

Although the Task Force considers a high school course necessary, they recognize that it will not be sufficient to assure the level of understanding they recommend and they suggest that economic understanding be introduced at a number of other points in the curriculum. A high school course is definitely needed, but the problem of implementing economic education in the schools should not be approached largely in terms of courses in economics. The emphasis should be reversed and some thought should be given to the probable nature of high school courses based on the Task Force report.

The importance, usefulness, and design of a high school course in economics will be largely a function of what is done elsewhere in the curriculum. Unless students have been exposed to the intellectual structure of economics and to disciplined, analytical thinking at the elementary and junior high school levels, little can be expected from a high school course. Basic elements of monetary policy will not be accessible even to high school seniors unless they are first aware of what money is, where it comes from, how it is related to spending, and the nature and role of banks in the economy. Nor can students handle very effectively the problems and issues involved in such areas as monopoly, agriculture, and foreign trade without some grasp of the nature of a system of prices and markets and the functions this system is supposed to perform. To be sure, these topics may all be found in the content recommended by the Task Force. But students do not acquire this background by osmosis. To the extent that the bulk of this task is assigned to the high school course serious limitations will be placed on the range of topics which can be considered. More importantly, students who are spoon-fed during the early years of their educational life cannot be expected to suddenly blossom into disciplined, analytical thinkers at the twelfth grade. In order to bring the student's understanding of the structure of economics into progressively clearer focus, there must be continuous development and logical continuity within the curriculum.

Unless the problem of implementing economic education in the schools is considered in this broader context, the high school economics course is likely to become a survey course, largely independent of other work in the school curriculum. The content recommendations of the Task Force and their emphasis on the importance of a high school course, encourage this interpretation and implementation. Chapter III of the report approximates an abbreviated summary of the standard college textbook in principles of economics. Teachers are encouraged, indeed almost guided, to interpret the task of implementing economic education as largely a matter of tailoring the traditional college course to high school dimensions. Attempts to cover the range of content recommended by the Task Force in a one-semester high school course would be both naïve and futile, although such attempts will probably be made by many teachers. To the extent that this occurs, our major accomplishment will have been to export to the schools the ubiquitous weakness of the principles course; namely, attempting the impossible in terms of coverage of subject matter within the time allotment provided.

To accept the hypothesis that any topic can be taught in some intellectually honest form does not imply that everything can be taught at a given level. The art of presenting abstract subject matter for the beginning student is not merely a matter of substituting short words for long ones. It is the concept itself which must be made concrete. Unfortunately, this is often accomplished only by expanding our treatment of it. Many economic concepts are in the nature of a shorthand to the professional economist, but for the beginning student, with his own internalized structure of language and experience, they are virtually an unknown code. Consider, for example, the concept of a fully employed economy. This is a multidimensional, dynamic, and somewhat arbitrary concept involving among other things the size of the labor force, the work week, productivity, and what constitutes an acceptable level of unemployment. When the economist encounters or uses the term he is aware of all of these things for which the term stands. For the student, the concept must be made concrete by supplying in step-by-step fashion all of the subconcepts, relations, and continuity of development, translated into the internalized structure on which the student operates. Although the most promising approach is to accept the hypothesis that any economic concept or topic can be taught in some intellectually honest form in a high school economics course, it is important to keep in mind that the number of topics that can be taught may be quite limited, and that the form in which they are taught may depart from the way in which we learned economics. Hard choices must be made with full expectation of the criticism that important parts of the body economic have been ruthlessly amputated. Needless to say, this will demand the utmost in courage on the part of professional economists, including not only the courage to exclude some important topics but also the courage to include concepts which are central to the discipline of economics regardless of preconceptions about their level of difficulty.

In any realistic sense, it should be recognized that, to reach any large number of students, the bulk of economics instruction in the public schools will have to take place outside the comfortable surroundings of a high school course in economics.[6] The crux of the problem is in programming the intellectual structure of economics to fit the public school curriculum and the spectrum of student interest, experience, and maturity levels. A high school course should be included in this over-all curriculum design, but it should be considered as the capstone rather than the foundation of the program, and its design should depend upon what has gone before.

Need for Model Building

As mentioned in preceding comments, the Task Force does recognize the necessity of extending economics beyond the confines of a separate high school course in the subject. They list a number of promising opportunities encompassing courses in history, geography, civics, mathematics, business education, and blocks of work in the elementary grades. Suggestions for particular topics in these courses are offered, and the need for joint work by economists, historians, sociologists, and political scientists is mentioned. This is good advice. But the nature and problems of such efforts are scarcely suggested by the report. What seems to be needed to implement these recommendations of the Task Force is something dear to the heart of all economists; namely, model building. Let me develop this proposition using the course in United States history for purposes of illustration.

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[6] Economists should not make the mistake of boycotting their responsibilities to the schools if they do not get all the economics, formally organized and taught as economics, which they might like. Every one of the social sciences disciplines is going its own way and insisting on more time in the curriculum. This does not necessarily imply an interdisciplinary approach, but it may lead to new curriculum patterns that are not currently visible on the horizon.

First, a word about the nature of the course. The raw materials of history are names, dates, events, movements, and facts in general. The discipline of history involves the discovery, verification, ordering, and interpretation of these raw materials. Within history as a discipline these matters turn importantly on the relative weights assigned to economic, social, political, cultural, and geographical factors. The economist, however, has no special competency which qualifies him to assess the relative importance of the respective social disciplines in interpreting the course of history. Nor are useful reference points for such an assessment to be found in the partial analysis that continues to characterize the social sciences. There is no reason why the selection, ordering, and interpretation of economic topics for a course in history should correspond to the relative importance of those topics within the framework of economics as a social discipline. Explaining the structuring and operation of the economy and charting the course of our nation's history are significantly different jobs. The point is that many of the most fundamental topics in economics are omitted entirely, and many economic topics which are included are relatively unimportant for purposes of economic understanding. The nature and operation of a market system and fiscal and monetary policy are just two illustrations of topics which receive virtually no consideration. In addition to this matter of balance and emphasis in coverage, the organization of material is almost entirely chronological. The approach is predominantly descriptive and journalistic. Lastly, the course undoubtedly reflects the thought forms and values prized by historians.

In rough outline this is the nature of the medium into which we are to insert or on which we are to superimpose the fundamental structure and analysis of economics, without impinging unduly on the materials and basic objectives of the course. It may well be possible to design a course in history with a substantial emphasis upon fundamental economic analysis. Certainly the attempt should be made. For the present, however, there is a wide gap between the Task Force recommendation and effective classroom implementation.

To bridge this gap, it will be necessary to struggle with the content of both history and economics and to experiment with various strategies and instructional media for incorporating economic analysis in the American history course. One approach might be to develop materials presenting rather straightforward economic analysis on such topics as the market mechanism, growth, stability, and international trade for insertion and use at strategic points in the course. The rationale for this approach might be that such materials would provide basic background for interpreting certain recurring historical events such as tariff legislation, financial panics, and depressions. A second approach might be to select several topics which occur frequently but which are rather poorly treated in the typical textbook and to prepare materials which develop the topic in a chronological framework relevant for the history course. Or chronological periods might be selected, with a topical development by period focusing on economic developments in such areas as money, international economic relations, the role of government, agriculture, labor, business, and so on. Consideration might be given to the economic analysis of several historical events such as the passage of the Sherman Antitrust Act or the Great Depression as an appropriate strategy for incorporating economic analysis. Case studies of organizations or agencies such as the Office of Price Administration or the Standard Oil Company as an illustration of the rise of a national monopoly might be another approach worth investigating. All of this, of course, is to say nothing of the variety of instructional media that might be employed such as textbooks, narrative stories, pamphlets, programmed materials, slides, filmstrips, films, recordings, official documents, charts, posters, examinations, problems, and exercises.

This by no means exhausts the possible approaches to the problem, but hopefully it suggests that there is a genuine intellectual and professional challenge for the economist who happens to be so inclined. What is needed are specific, detailed teaching models, complete with instructional materials, not only for history but for all promising curriculum opportunities, followed by extensive experimentation and evaluation.[7] In this process evaluation is particularly important and should be made an integral part of course development rather than a terminal activity. The major purpose of evaluation should be to identify aspects of the model where revision is desirable. As one authority in the field of evaluation has emphasized:[8]

> To be useful such evidence must become available midway in curriculum development, not in the home stretch. At the end, the developer has a natural reluctance to tear open the body of materials and techniques in the hope of finding a rearrangement that will accomplish what the original materials did not. Evaluation, used to improve the course while it is still fluid, contributes more to the improvement of education than it does when it tries to appraise a product already placed on the market. Insofar as possible, evaluation should be used to understand how the course produces its effects and what parameters influence its effectiveness.

If intellectually honest economics is to find its way into the public school classroom, it is economists who will have to initiate the effort and play a major role in the necessary model building, experimentation, and evaluation.[9]

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[7] Teaching models will also be needed for such courses as government, general business, geography, problems of democracy, and blocks of work at the elementary level. Two types of models would be desirable for the economics course: one for immediate use before the total elementary and secondary curriculum is revised, and one for the kind of course that would take account of a total curriculum that had more economic education in it.

[8] Lee J. Cronbach, "Evaluation for Course Improvement" (Bureau of Educational Research, Univ. of Illinois, 1962, mimeographed).

Teacher Education

Past efforts to implement economic education in the classroom have placed considerable emphasis on teacher education. This emphasis has been based on the factual assertion that economic understanding cannot be imparted by teachers who do not themselves understand economics, coupled with the hope that limited instruction in economics (the more the better, of course) will equip them to handle the problems of course design and identifying or developing needed teaching materials. Stated more bluntly, the philosophy has been, to "give the teachers some economics and they will incorporate it into their classroom teaching."

Largely on the basis of this philosophy, a variety of programs have been developed to assist teachers already on the "firing line" in strengthening their background in economics. Summer workshops, college extension courses, and currently the national television program are examples of this effort. In addition, some colleges and universities have introduced new courses or special sections of existing courses in economics designed for students preparing to teach in the public school. It would be difficult to overestimate the value of teacher preparation in economics. And certainly these are all important and useful vehicles for transmitting economic understanding to the teacher. Hopefully, their classroom teaching will benefit from these educational experiences, although in too many instances, I am afraid, the amount of economics that filters down into the classroom is little more than a trickle, to say nothing of its quality. In some cases, I suspect, the major result may be simply teacher frustration.

The major difficulty in this whole enterprise is that the jobs of course design and the development of instructional materials require far more economic understanding and sophistication than we can ever hope to achieve in any reasonable training effort. It is simply unrealistic to think in terms of training large numbers of teachers to the point where they can independently structure the subject matter of economics for the curriculum and translate this structure into materials that match the abilities and interests of students at various grade levels. Teacher training programs are important. They should be continued. The more teachers know about economics, the better they will be able to teach it. But we cannot depend on such programs to produce effective classroom change. The major problems are conceptual and methodological. Their solutions require research and experimentation, coupled with evaluation. Until advances are made on this front, only limited gains can be expected from traditional training programs. With genuine advances in course design and accompanying materials, training efforts can be given a sharper focus, with the comforting assurance that the teacher will not be left to flounder when he returns to his classroom.[10]

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[9] This does not mean that economists can solve all the problems by themselves. Cooperative effort of economists, teachers, and educators will be needed. Teachers are in charge of the "research populations" needed for testing and experimenting, and represent a valuable source of "feedback" for improving the models. And certainly educational theory and psychology cannot be ignored in any effort to develop detailed teaching models.

Concluding Observations

All of these considerations, of course, are largely matters of approach and strategy—a frame of reference for future effort. But what of the present? The Task Force report is with us. It has been highly publicized and widely distributed. School people have evidenced interest. But they will need help—plenty of it—if they are to make a beginning in translating its recommendations into effective classroom practice. The majority of teachers do not understand the report, and they do not know what to do with it. The order of assistance needed is comparable to that provided by the county extension agent in the area of agriculture.

Perhaps a few examples will illustrate the kind of problems teachers will encounter when they attempt to implement the report. Content recommendations for the area of farm incomes and the "farm problem" are presented in two very short paragraphs comprising roughly one-third of a page (page 59). Teachers are then told that "the 'farm problem' should be understood broadly by students. It presents a good opportunity to use the simple tools outlined above to analyze a major national economic problem." The generality and brevity of this section severely limit its usefulness to teachers: they do not understand the farm problem; they will not know what tools are relevant for the analysis or how to use them; they will be at a loss on how to structure the content for a particular teaching unit; and they will not be familiar with instructional materials on the topic appropriate for their students.

At another point in the content recommendations, there is a short paragraph on the corporation as a form of business organization. It is suggested that students should know "something" about the organization of the corporation and its financing, separation of ownership and control, and the self-perpetuating tendency of management. The paragraph ends with these two sentences: "Not only is there a concentration of power and wealth in large firms, but also power within these firms is concentrated in the hands of a relatively small number of executives and large stockholders. Where this occurs, important problems of social policy are raised." (Page 36.) Teachers, however, will not know what the "something" is that students should know about corporate organization and finance, and they will not know the problems of social policy referred to.

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[10] If we are serious about improving the ability of teachers to handle economics effectively in the classroom, this also implies the development of some models for better teacher education.

Scattered throughout the content recommendations are such phrases as "know something about," "need not know in detail," "should be understood broadly," "need to understand only in a general way," "should have a rough understanding." Teachers have no basis for determining what is implied by such terms as "something," "detail," "broadly," "general way," and "rough understanding." In fact, economists working with the schools may encounter some difficulty in determining precisely what the Task Force had in mind.

These examples could be multiplied many times over, but the point should be clear: teachers are going to need help, and in large doses! It would be a mistake to regard the Task Force report and related endeavors as finished business. Rather, the challenge is to work seriously toward the general objectives set forth by the Task Force. Today, the schools are anxious to improve and strengthen their curricula. As they search for new guidelines and fresh points of reference, they want and need professional assistance on many matters. Important first steps have been taken in meeting some of these needs, but much remains to be done if the progress already achieved is to be accelerated and the effort successfully institutionalized. Promising practices must be translated into effective, continuing programs. The real pay-off is to be found in what goes on in the classroom, and on this score, conclusive evidence of significant change is both scattered and scanty. This, however, is the large and challenging task to which the Task Force would have economists, educators, classroom teachers, and the public schools address themselves.

DISCUSSION

Floyd A. Bond: This meeting is a milestone in the long history of this Association. At no time in the past would it have been possible to hold such a session. This meeting is living proof of the growing concern of the economics profession for the level of economic understanding of the whole population rather than for only that segment that studies economics in college.

The far-reaching significance of this development is still only dimly realized. The authors of these three stimulating and instructive papers have referred repeatedly to the National Task Force on Economic Education. But as short a time as three years ago, no one had heard of the National Task Force; there simply was none. Nor was anyone making plans for a nation-wide television series on "The American Economy." Neither was anyone seriously contemplating the collection and evaluation of the mountains of materials on economics which were overwhelming school teachers.

Even today I wonder how many of the members of the American Economic Association realize how much the economics profession owes to the Committee for Economic Development (CED) and the Joint Council on Economic Education (JCEE) for initiating this frontal attack on economic ignorance, for putting this Association in the forefront in this nation-wide effort, and for giving the economics profession full academic freedom, in the best academic tradition, for the content of these several programs.

Although it is only right that the economists themselves should receive full credit for their professional performances—I refer of course to the report of the National Task Force on Economic Education,[1] the report of the Material Evaluations Committee,[2] and the TV series on "The American Economy"[3]—we should at the same time not forget that it was the CED that initiated this major push forward. Without this stimulation, encouragement, and financial support, we would not be discussing the merits and limitations of the report of the National Task Force here.

In his paper, George Stigler praised the economists on the National Task Force as highly esteemed "leaders in American economics, of unquestionable competence and integrity." The fact is, of course, that they were selected for those very qualities. When CED initiated the National Task Force idea and invited the AEA to appoint the members, they were nominated by the AEA Committee on Economic Education and appointed by the 1960 AEA President, Theodore Schultz.

To those of you who are not familiar with the Task Force report, it might appear to you that at least two of the three speakers on this panel are highly critical of it. However, a careful reading of their papers suggests this is not really the case.

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[1] *Economic Education in the Schools* (Committee for Economic Development).

[2] *Study Materials for Economic Education in the Schools*, Supplementary Paper No. 12, published by the Committee for Economic Development.

[3] A "College of the Air" production, presented over the CBS Television Network.

The main trouble with the report, according to these writers, is that its authors attempted too much. They were too ambitious.

Thus, Stigler finds the topics in the report similar to "a representative table of contents of a textbook on elementary economics at the college level" and contends that "the watered-down encyclopedia which constitutes the present course in beginning college economics does not teach the student how to think on economic questions. The brief exposure to each of a vast array of techniques and problems," he continues, "leaves with the student no basic economic logic with which to analyze the economic questions he will face as a citizen."

Lew Wagner agrees. "Teachers are encouraged," he says, "indeed almost guided, to interpret the task of implementing economic education as largely a matter of tailoring the traditional college course to high school dimensions. . . . Attempts to cover the range recommended by the Task Force in a one-semester high school course would be both naïve and futile."

It is significant to note that the Task Force went to great pains to avoid the very thing of which they are accused. Indeed, they decided early in the project to organize their presentation around four main topics, to omit numerous subjects which may be found in the usual introductory college text, and above all, to avoid the temptation to write a text themselves. These four areas which occupy a total of forty-two pages were: (1) How the Economic System Uses Productive Resources in Satisfying Competing Wants (pages 27-42); (2) Economic Growth and Stability (pages 43-54); (3) The Distribution of Income (pages 55-60); (4) Communism, Socialism, and Capitalism (pages 61-63).

Jack Coleman, on the other hand, finds the coverage in the report too limited—at least by comparison with what he wants to do on the television program. "It is not our intention," he writes, "to teach just the economics that the high school teacher, following the Task Force recommendations, might wish to teach in the classroom. We went beyond that minimum in the belief that first-rate teaching requires that the instructor be several long jumps ahead of the best students in the class. . . . It has seemed far wiser," he continues, "to expect able teachers to simplify their understanding for classroom purposes than to leave them to teach on the very edge of their competencies."

And Wagner, at a number of points in his paper, criticizes the Task Force for failure to develop subjects, such as the farm problem, for example, in more detail.

There is a point here which needs explicit recognition. The more ideas to be covered in a given length of time, the less time available to develop each idea in depth. This is the hard choice that must be made given the assumption. But the Task Force did not limit itself to a single course—as some might read into Lew Wagner's paper.

Stigler would limit the content of both high school and elementary college economics to "the basic logic of price theory, applied to two score of real problems." "An introductory-terminal course in economics," he says, "makes its greatest contribution to the education of students if it concentrates upon a few subjects, which are developed in sufficient detail and applied to a sufficient variety of actual economic problems to cause the student to absorb the basic logic of the approach."

Wagner would put the emphasis on "the basic structure of the discipline," but means by this "those concepts and relationships which serve to organize and tie together seemingly disconnected facts and topics into a unified, meaningful system of thought." He would abandon "good citizenship" as the objective on the grounds it is "not operationally meaningful" as a guide, but notes that "there is no reason to believe that a professional orientation . . . will prove to be inconsistent with responsible citizenship."

Having sat through the numerous sessions at which the Task Force report was taken apart and put back together—again and again—by the Task Force itself, I think it only fair to say that the members of the Task Force were not unaware of these considerations. "What we want to emphasize," if I may be permitted to quote from the report itself, "is the need to develop in the student the ability to reason clearly and objectively about economic issues. The future citizen needs to acquire a modest amount of factual information about the economic world, but the primary obligation of the schools is to help him to develop his capacity to think clearly, objectively, and with a reasonable degree of sophistication about economic problems. Mere description of economic institutions is not what we mean by economic education."

"In this connection," the report continues, "we believe that training in economic analysis does more than just contribute to the student's understanding of how our economic system operates. As we emphasize repeatedly in these pages, the development of economic understanding involves first and above all the capacity to think *rationally* about economic issues. In this respect, the teaching of economics can make an important contribution to the student's mental development."

The report does not, as Wagner points out, indicate precisely what should be taught, where, and how. And, as Wagner also points out, the report does contain—and some would say necessarily contain—such phrases as "know something about," "need not know in detail," and "should be understood broadly."

The really significant thing, however, is that the report does exist and does serve as the most useful and most authoritative single guide yet available to public school teachers. It does tell them, and quite well I believe, what the economics profession thinks is important and, in addition, does give them some guidelines as to the relative importance of various topics that might be covered in the schools. In doing these things, it filled a real vacuum.

That a tremendously big job remains to be done should not detract from the significance of the achievements made to date.

Three years ago, there were many—including some economists—who genuinely believed that economists could not agree on enough things to make this project feasible. I know because I sat through a number of such sessions. Certainly the National Task Force report, whatever its limitations, has settled this issue once and for all. Economists can and do agree on a great many things. Not least of these is agreement on the desirability of disagreement such as that represented by the three excellent papers presented here!

CAMPBELL R. MCCONNELL: Professor Coleman's paper provides us with an authoritative assessment of the merits and demerits of television teaching and a number of compelling reasons why professional economists have a continuing obligation in economic education at the secondary level. Similarly, I find Professor Wagner's paper to be a cogent and realistic appraisal of the general problems entailed in fulfilling these various obligations and, more specifically, in implementing the recommendations of the Task Force. He makes a number of intriguing suggestions which pinpoint future avenues of development in economic education. The need for greater efforts by the professional economist in the areas of teacher training, curriculum design, and materials development is convincingly asserted. I would offer this one tentative comment. Professor Wagner suggests that a professionally oriented high school course may be more "operationally workable" than an "effective citizenship" orientation because the former provides priorities for the selection of subject matter; i.e., "the wider the applicability . . . and the more powerful [a concept] is in organizing and relating other facts and ideas, . . . the higher the priority it deserves." While this may be true, it seems to me that the professionally oriented course suggests a level of abstraction in the high school course which is more inconsistent with Professor Wagner's own assessment of the degree of economic understanding now possessed and likely to be achieved by high school teachers than would be the effective citizenship orientation of the Task Force. The successful introduction of "fundamental economic analysis" within the constraints of the high school history course might entail a similar problem.

For the most part my comments involve taking issue with some of the points made in Professor Stigler's challenging presentation. I confess at the outset a much greater willingness to question his offerings in the present subjective environment of opinion than would be the case in the cold arena of facts and analysis.

In essence Professor Stigler raises two major points. First, the subject matter proposed by the Task Force is too encyclopedic, too ambitious, to form the core of a meaningful high school or, for that matter, college course in economics. Second, there is a clear need for greater emphasis upon price theory and resource allocation if the introductory economics course is to contribute substantially to the understanding of current problems.

While I am not completely unsympathetic to his first criticism, I am compelled to make these points: (1) It is not quite correct to preface the Task Force's list of subject matter with the notation that it constitutes "what the Task Force believes should be in the high school course in economics," when, as a matter of fact, the Task Force at the outset clearly states, "We do not expect that all students in all schools will have the opportunity (and some will not have the ability) to absorb all that is suggested here. . . . The total body of material here represents an ideal which the high schools should seek to achieve for the largest number of students." (Pages 22-23.) (2) Throughout the list of subject matter proposed by the Task Force one cannot help but be impressed by the generous use of such terms as "know something about," "need to understand only in a general way," "should be understood broadly," "need not know in detail," etc. Thus, while the ideal list of subject matter proposed by the Task Force may generally correspond with that of the college introductory course, the level of analysis and understanding sought is clearly presumed to be substantially different. It is my impression that this is a reasonably logical relationship between a high school and college course in any given area and, as a matter of fact, one which exists in many fields; for example, history, political science, biology, physics, etc. (3) Without being too forceful I would offer the impression that high school and college students are increasingly better prepared to handle somewhat abstract, analytical material. Might we not be underestimating somewhat the ability of high school seniors to cope with a reasonably ambitious twelfth grade economics course? Professor Wagner's comments to the effect that our preconceptions as to what is teachable are overly pessimistic suggest that he may share this opinion. (4) Finally I am not convinced that it is less sound pedagogically to fulfill ambitious goals partially than it is to achieve more modest ones completely.

Professor Stigler's second point—the need to put substantial emphasis upon pricing and resource allocation in the introductory course—is one with which I am in agreement. But my endorsement of this point stems from quite different reasons than those suggested by Professor Stigler. His list of bills passed by the first session of the 81st Congress does little to convince me that price theory is of greater usefulness to the citizen than is employment theory in understanding current economic problems. Perhaps one sees what one wants to see, but from the terse listing provided I associate both macro- and microeconomic implications with most of the bills noted. Furthermore, might not a serious weighting problem be ignored in this listing? The macroeconomic implications of one important piece of legislation might easily dwarf a large number of bills which entail minor reallocations of resources. It is not impossible that the mere successful application of public policy under the Employment Act of 1946 might be of greater economic consequence than passing a large number of bills whose orientation is toward resource allocation.

More positively, I feel pricing and resource allocation merit considerable emphasis in the introductory-terminal course for these reasons: (1) My limited experience suggests that the concepts of microeconomics are more elusive to beginning students than are those of macroeconomics and for this simple reason merit more detailed treatment. (2) Although I would not want to defend this point at great length, it is my impression that we now possess a better understanding of those policy measures which can provide for full employment and reasonable price stability than we do of policies necessary for improved resource allocation. For example, ignoring problems of exact size and timing and waiving a host of political problems, the vast majority of economists would agree that recessions call for public deficits and inflations for surpluses. It is very doubtful there exists a similar consensus that a more vigorous application of antitrust will improve resource allocation particularly in the long run. In any event, if we actually do have a greater capacity through public policy to resolve the instability problem, then this seems an important reason for emphasizing problems of resource allocation which are as yet less susceptible to control or correction. (3) Microanalysis merits emphasis in part because of the contribution it can make to understanding the economics of employment and growth. For example, an elementary grasp of pricing and resource allocation is an important prerequisite of understanding demand-pull inflation, not to mention structural inflation and structural unemployment. And a primary reason for studying the theory of the firm is to determine what market structures are most conducive to the high rate of technological progress which is essential for full employment and economic growth. In more general terms, I think it is every bit as meaningful to view micro- and macroeconomics as complementary rather than substitute facets of subject matter. Hence, I would argue for a rough balance between micro and macro (one which I feel the Task Force has achieved), while Professor Stigler asserts the need for more price theory at the expense of employment theory. (4) It seems to me that aspect of resource allocation which is most sorely neglected at all levels of economic education is that having to do with the optimum division of resources between the private and public sectors of the economy. Price theory, as such, contributes only modestly to an understanding of this crucial allocative issue.

Finally, because the authors of principles texts will soon be in the majority, I would be remiss not to offer some reactions to Professor Stigler's and apparently every one else's (see *American Economic Review,* May, 1962, pages 474-96) dissatisfaction with "current encyclopedic texts." On the one hand—and at the risk of being taken too seriously—it is mildly amusing to find Professor Stigler (whose faith in the price mechanism is somewhat greater than mine) suggesting that the market in this particular instance has brought forth products which are uniformly unsuited to the needs of buyers. Further, might not the problem of encyclopedic courses lie partially in instructors' low propensity to discriminate in the selection of topics? The textbook inclusion of material on the farm problem, income inequality, labor problems, and even international economics, which are decried as expendable by various instructors, does not obligate anyone to actually use this material in their courses. And, finally, I sometimes wonder whether in our enthusiasm for economic education for effective citizenship we have not gone too far in tailoring the introductory college course for the terminal student and the casual interloper at the expense of the admittedly small, yet extremely important, minority who will take further work in economic analysis

AMERICAN ECONOMIC ASSOCIATION

PROCEEDINGS

OF THE

SEVENTY-FIFTH

ANNUAL

MEETING

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PITTSBURGH, PENNSYLVANIA
DECEMBER 27-29, 1962

PROCEEDINGS OF THE AMERICAN ECONOMIC ASSOCIATION

ANNUAL BUSINESS MEETING, DECEMBER 29, 1962
PENN-SHERATON HOTEL, PITTSBURGH, PENNSYLVANIA

The Seventy-fifth Annual Business Meeting of the American Economic Association was called to order in the Grant Room of the Penn-Sheraton Hotel at 5:00 p.m. by President Edward S. Mason. The minutes of the business meeting of December 29, 1961, were approved and the minutes of the Executive Committee meetings and reports of officers and committees of the Association were ratified. These reports are published in the "Proceedings" and constitute the official actions of the Association when approved at the annual meeting.

The report of the Secretary was presented by H. F. Williamson. The schedule of times and places for future annual meetings through 1969 was outlined. Attention was called to the fact that the total number of members and subscribers of the Association as of November 30, 1962, was 15,644, up 661 from the figure of 14,983 as of November 30, 1961. The Secretary also noted that the Executive Committee had authorized the publication of a new edition of the *Handbook,* which should be available by January, 1964; that arrangements had been made to reprint back numbers of the *American Economic Review;* and that members would receive copies of the *Journal of Economic Abstracts* in January, 1963.

The report of the Treasurer was presented by H. F. Williamson. Special attention was called to the fact that despite rising costs affecting the publications and other activities, the Association had a surplus at the end of the fiscal year of $279.24. The full details of the financial operations of the Association for 1962 are shown in the tables in the Treasurer's Report to the Executive Committee.

J. W. Bell reported for the Finance Committee on the changes in the Association's investment portfolio. The full report of the Finance Committee is published below.

The report of the Managing Editor, published below, was summarized by B. F. Haley, who expressed his appreciation for the effective work done by the Editorial Board and consultants which was so helpful in preparing the *American Economic Review.*

It did not seem feasible to present at the meeting reports of the representatives of the Association to various scientific associations.

Immediately following the report of the Managing Editor, the chair recognized Paul A. Samuelson, who, on behalf of the Association, presented B. F. Haley, the retiring Managing Editor, with a silver tray as a tribute to his outstanding contribution to the *American Economic Review* over the past eleven years. In his response, B. F. Haley expressed his gratitude for the cooperation of the officers and members of the Association during his term as Editor and called special attention to the fact that at no time was he ever under pressure from the officers to change the editorial policies of the *Review*. He expressed the hope that his successor would continue to receive the same co-operation and support.

The following report of the Committee on Elections and the certification of the election of new officers for the year 1963 were presented by the Secretary:

In accordance with the bylaws on election procedure, I hereby certify the results of the recent balloting and present the reports of the Nominating Committee and the Committee on Elections.

The Nominating Committee, consisting of Theodore W. Schultz, Chairman, Dorothy C. Bacon, Richard E. Caves, Rendigs T. Fels, Douglass C. North, and Robert M. Solow, presented to the Secretary the list of nominees for the respective offices:

For President-elect
George J. Stigler

For Vice-Presidents	*For Executive Committee*
Gardner Ackley	William J. Baumol
Abba P. Lerner	Evsey D. Domar
Jacob Marschak	Richard Goode
Tibor Scitovsky	William C. Hood

The Committee on Elections, consisting of Frank W. Fetter, Chairman, O. A. Smalley, and Harold F. Williamson, prepared biographical sketches of the candidates and ballots were distributed early in November. The canvass of ballots was made on December 17, 1962, and the results were filed with the Secretary.

From the report of the Committee on Elections, I have the following information:

Number of envelopes without names for identification	140
Number received too late	30
Number of defective ballots	5
Number of legal ballots	3,971
Number of returns from the mail ballot	4,146

On the basis of the canvass of the votes cast, I certify that the following persons have been duly elected to the respective offices:

President-elect (for a term of one year)
George J. Stigler
Vice-Presidents (for a term of one year)
Gardner Ackley
Abba P. Lerner
Members of the Executive Committee (for a term of three years)
William J. Baumol
Evsey D. Domar

The new officers were then inducted and President Gottfried Haberler took over the chair and called for any unfinished or new business.

There being no unfinished business to come before the meeting, the chair recognized E. T. Weiler, who presented a memorial to Arthur William Marget. It was voted to include this memorial in the "Proceedings."

The chair called for the report of the Committee on Resolutions, which was read by Fritz Machlup:

Be it resolved that this Association express its appreciation to all those who have participated in the planning, organization, and administration of its Seventy-fifth Annual Meeting. The Association is especially grateful to its President, Edward S. Mason, for his timely and challenging address; to its President-elect, Gottfried Haberler, for designing a program of unusual appeal to the membership; to the Executive Committee for assisting in the difficult task of program planning and implementation; and to the many speakers and discussants who have contributed to our edification and enlightenment.

FRITZ MACHLUP, *Chairman*
RANDALL HINSHAW
ELMO P. HOHMAN

Following the report of the Committee on Resolutions, President Haberler on behalf of the Association thanked Clyde Harrell, Chairman of the Committee on Local Arrangements, and his associates for the excellent job they did in planning the annual meeting and handling the complex problems of registration and scheduling of meeting rooms.

The meeting was adjourned at 5:45 p.m.

HAROLD F. WILLIAMSON, *Secretary*

THE FRANCIS A. WALKER AWARD

CITATION BY FRITZ MACHLUP, DECEMBER 27, 1962, on the OCCASION OF CONFERRING THE MEDAL ON JACOB VINER

This medal, which is not to be bestowed more frequently than once every five years, is to be awarded to a living economist "who has in the course of his life made a contribution of the highest distinction to economics." Awards of this medal are to be made for "contributions to the central body of economic thought and knowledge. They are not to be made for excellence in teaching, administration, or public service. Neither are they to be made for contributions to special branches of economics, except insofar as they may contribute vitally to general economic doctrine and knowledge."

The Francis A. Walker award of 1962 is made to one who, in the deliberations of the Committee on Honors and Awards, was characterized in these words: "He represents the greatest combination of theoretical keenness (with no need for fancy techniques), alertness to policy issues, and historical scholarship in both economic institutions and economic ideas. In all the fields to which he has contributed, including his specialty, international economics, his name will survive brightly as a deflator of pretentious nonsense as well as an original creator. What we all might learn from him is intellectual honesty and fearlessness, a willingness to stand firm on the unpopular side of any issue, theoretical or practical, whether that side be 'radical' or 'conservative,' 'newfangled' or 'old-fashioned.' "

There is no need to recite here the titles of his past contributions to economics; they are well known to the student of economics. But we may reveal that the medalist of 1962, though retired from his teaching duties, has by no means retired from scholarly research. We look forward to more products of his scholarship and we do so with most pleasant anticipations.

Perhaps I may finally say how happy it makes me that it is my immediate predecessor in the academic position I hold whom we are honoring today. Speaking for the Committee on Honors and Awards, I present, on behalf of the American Economic Association, the Francis A. Walker medal to Jacob Viner, Walker Professor Emeritus of Princeton University.

IN MEMORIAM

ARTHUR WILLIAM MARGET

1899-1962

Arthur W. Marget, formerly Professor of Economics at the University of Minnesota and recently retired as Chief of the Division of International Finance of the Board of Governors of the Federal Reserve System, died of a heart attack on September 5, 1962, in Guatemala City where he was serving as Regional Economic Adviser with the Agency for International Development.

His untimely death brought to a conclusion three distinct and distinguished careers. His first career as a research scholar started early. As an undergraduate at Harvard College he made some significant Biblical textual interpretations establishing the historical link between two Semitic languages. The sense of continuity that as an undergraduate he came to expect from research scholars dominated much of his thinking in economics to which he turned as a graduate student. He based his professional work on the premise that the development of knowledge, particularly economic theory, is like the building of a medieval cathedral—the work of many independent artisans each having a sense of the design for the whole structure and willing to make their contribution to the growth and beautification of the whole. His unpublished (and much used) doctoral dissertation, "The Rate of Interest," completed in 1927 at Harvard, his journal articles, particularly those dealing with the monetary aspects of the Walrasian System, and finally the fourteen hundred pages of *The Theory of Prices*, Volume I (1938) and Volume II (1942)—all testify to his strongly held view that research scholars should first know what other scholars who had preceded them had contributed to their discipline before proposing new theories. He saw the body of economic theory as a magnificent structure slowly being created out of the work of David Ricardo, John Stuart Mill, Stanley Jevons, Leon Walras, Kurt Wicksell, Alfred Marshall, A. C. Pigou, and others. He also noted the many poor theories developed by economists who had insisted on starting anew each time. It was against the backdrop of this strongly held view that Arthur Marget chose to examine in his *Theory of Prices* the monetary theories of John Maynard Keynes and to propose his own synthesis of monetary theory. It is a matter of record that the economics profession, then trying to understand the implications of the Keynesian developments, with few exceptions, largely ignored the work done by Arthur W. Marget.

In his second career, as a teacher, he served as an instructor in Harvard College from 1923 to 1927, as an Associate Professor in the University of

Minnesota from 1927 to 1930, and as a Professor of Economics from 1930 to 1943. Lecturing to large classes in business cycles and monetary theory, he left his mark on many generations of students as a talented expositor of economic doctrines. But it was as a graduate teacher of monetary theory both in and out of the classroom that Arthur Marget was most effective. He not only insisted that his students understand the growth and development of economic doctrines, but also the role of economic institutions in economic affairs. Some of his most effective teaching was done in connection with his deep interest in musicology and his enthusiasm for the opera as an art form. Together with his gracious wife, Edith Pavlo Marget, who survives him, Professor Marget entertained his graduate students on alternate Saturday nights with recorded operas and carefully prepared interpretations of the major operas. (In connection with his interest in the opera he published a number of interpretations of Wagnerian operas which were widely quoted by professional musicologists.) As a teacher he was wholly successful in conveying to his students the sense of continuity in intellectual endeavor and the importance of careful scholarship in adding to the body of knowledge.

Arthur Marget abandoned his academic career in 1943 and spent his last nineteen years as a highly successful practicing economist. He entered the Army as a Major in 1943 and was assigned to the United States Element of the Allied Commission in Austria. In 1945 he was made Chief of the Economics and Finance Division and Lieutenant Colonel, and from this position made many decisions, under difficult conditions, which kept the Austrian economy from experiencing a ruinous inflation. He participated in the Council of Foreign Ministers in London and Moscow. After serving as a consultant to the U. S. Treasury in 1948, he was made Chief of the Finance Division and Special Representative to the Economic Cooperation Administration in 1948 and 1949. And in 1950 he was appointed Director of the Division of International Finance in the Board of Governors of the Federal Reserve System, where he served with distinction for eleven years. As a practicing economist, he used the monetary theory he had developed as an academic economist, and relying on the careful use of quantitative data and his understanding of the institutional setting, he became expert in giving sound advice to those who determined economic policy. As Chief of the Division, he was primarily a practicing economist, but to his close associates he continued to be a talented scholar and an exacting teacher.

It is deeply regrettable that having partially retired with the expectation that he would have more time to enjoy his music and to pursue his interest in musicology, he was not permitted to reap the reward for a full and active life. His students and close friends entertained the hope that in this new setting he could be induced to return to scholarship in economics for which he was so talented.

MINUTES OF THE EXECUTIVE COMMITTEE MEETINGS

1. Minutes of the spring meeting held in New York City, March 23-24, 1962:

The *second meeting* of the *1962 Executive Committee* was called to order at 9:45 a.m. at Hotel Commodore, New York City, March 23, 1962. The following were present: E. S. Mason, presiding, Gottfried Haberler, B. F. Haley, C. P. Kindleberger, R. A. Musgrave, P. A. Samuelson, T. W. Schultz, Robert Triffin, W. A. Wallis, H. F. Williamson, and R. A. Young. Absent were: Ewan Clague, J. K. Galbraith, and A. G. Hart. Present as members of the Nominating Committee were: Dorothy C. Bacon, R. E. Caves, R. T. Fels, D. C. North, and R. M. Solow. Attending as guests were: Arthur Smithies and Clair Wilcox.

1. *President's Remarks* (E. S. Mason). After outlining the items of the agenda for the meeting, President Mason announced the appointment of Abe Fortas, of Arnold, Fortas and Porter, as Counsel for the Association.

2. *Minutes.* The minutes of the Executive Committee meetings of December 26 and 29, 1961, were approved.

3. *Report of the Secretary* (H. F. Williamson).

Annual Meetings. The Secretary reported that plans were well under way for the 1962 annual meeting at Pittsburgh, under the chairmanship of Clyde Harrell. The headquarters hotel will be the Penn-Sheraton. He reviewed the schedule for future meetings (Boston, 1963; Chicago, 1964; New York City, 1965; Washington, D.C., 1967), announced that arrangements had been made to have our headquarters at the New York Hilton in 1965, and raised the question of meeting place for 1966. After discussion it was VOTED to hold the 1966 annual meeting in San Francisco and to explore the possibilities of meeting in Chicago in 1968.

Membership Analysis. It was pointed out by the Secretary that the present apparent drop in memberships and subscriptions was probably incorrect, since the information from the data processing company was not complete.

Publications.

Papers and Proceedings. It was announced that the plan was to print 16,000 copies of the 1962 *Papers and Proceedings*, that the estimated total manuscript pages would be approximately one thousand, and that $20,000 had been budgeted for covering expenses.

Handbook. Following a review of alternative ways of preparing a new edition of the *Handbook*, the Secretary was instructed to explore the possibility of having the work done by a professional organization outside the office of the Secretary.

Reprinting of Out-of-Print Issues of the American Economic Review. The Secretary reported on the experience in granting permission to reprint articles from the *Review* under the policy adopted at the December meeting of the Executive Committee of charging $50 for each article. Following a discussion, it was VOTED that the Association should grant permission to reprint only after the consent of the author had been obtained and all financial arrangements for the privilege of reprinting should be made between the author and the editor-publisher.

4. *Report of the Treasurer* (H. F. Williamson). The statements of the financial condition and the income reports were reviewed and the operating results and the budget estimates for 1962 were discussed.

5. *Report of the Finance Committee* (J. W. Bell). The Treasurer, on behalf of the Committee, reported that there had been no significant changes in the Association's holdings of securities. The market fluctuations had left the value of the holdings substantially where it had been as of the date of the previous report, November 30, 1961.

6. *Report of the Managing Editor* (B. F. Haley). The Editor reported no new developments since December and that J. G. Gurley and Lorie Tarshis would serve as Acting Editor and Associate Editor during his sabbatical leave.

7. *Reports of Standing and Special Committees.*

Committee on Research and Publications (Gardner Ackley; W. L. Thorp, Acting).

Readings Series. In the absence of Mr. Thorp, the Secretary summarized a written report by the Acting Chairman, calling attention to the fact that the new financial arrangements for the editing of the two volumes, *Readings in Welfare Economics* (Tibor Scitovsky and K. J. Arrow) and *Readings in Business Cycles* (R. A. Gordon and L. R. Klein), had been approved by the Association and Richard D. Irwin, Inc.

Translation Series. No report was submitted on the "Translation Series" by the Acting Chairman. The Secretary called attention to the fact that the Association has a $25,000 grant from the Ford Foundation for the translation and publication of significant foreign books and manuscripts; that thus far no commitments had been made for this purpose; and that under the terms of the grant any unexpended balance must be returned to the Ford Foundation in 1964. Following a discussion, it was agreed that the President should urge the Committee on Research and Publications to give immediate attention to the possibility of using the grant for the translation and publication of significant foreign books and manuscripts.

Cumulative Index Project (J. P. Miller). The Secretary summarized the written report of the Chairman, indicating that the project was in its final stage and that Volume V would be ready for the printer as of early May, 1962, and raising the question of whether an additional volume, Volume VI, should be published, covering the period from 1959 through 1962-63. It was VOTED to accept the report, with thanks. It was agreed that further consideration should be given to the possibility of publishing a sixth volume of the *Index of Economic Journals.*

Committee on Economic Education (B. W. Lewis). In the absence of Mr. Lewis, the Secretary read a report from the Chairman, outlining plans for a session on economic education at the December meeting at Pittsburgh.

National Task Force on Economic Education (G. L. Bach). In the absence of Mr. Bach, the Secretary summarized the written report by the Chairman, which called special attention to the role of the Task Force in planning the national TV program, "The American Economy," to be carried by the CBS College of the Air during 1962-63.

Committee on Economic Abstracts (Arthur Smithies). The Chairman reported that the experimental issue of the *Journal of Economic Abstracts* had been enthusiastically received. He outlined the possibilities for continuing the series and putting it on a permanent, self-supporting basis. Following a discussion, it was VOTED that the Association should apply for further financial support from the Ford Foundation to continue the publication of the *Journal of Economic Abstracts.* It was agreed that every effort should be made to put the *Journal* on a self-sustaining basis.

Committee on Professional Ethics (R. D. Calkins). The President summarized a report from Mr. Calkins, covering the past activities of the Committee, calling special attention to the point that the Committee was appointed in 1959 to consider the question of professional ethics in the Association and specifically to determine whether certain charges which had been made against members and officers of the Association merited investigation. After a thorough investigation of the charges, the Committee concluded that the evidence did not offer a sufficient basis to question the integrity of those charged or to warrant any further investigation by the Committee. It was VOTED to accepted the report and to discharge the Committee, with thanks for their work.

Committee on Academic Freedom and Civil Liberties (F. M. Boddy). No report was presented. After discussion, it was agreed that the American Economic Association should go on record as endorsing the statement of principles on academic freedom and tenure (as outlined in the *Bulletin* of the American Association of University Professors, December, 1960, pages 410-11), subject to a covering letter by the President indicating that certain provisions regarding tenure as outlined in the statement of principles seemed not to be relevant to the question of academic freedom.

Committee on Honorary Members (Gottfried Haberler). The Chairman presented a report on the selection of honorary members, outlining the principles followed in their choice of suggestions, and proposing seven names for consideration. After discussion, it was VOTED to extend invitations to become honorary members to the following: Eugenio Gudin, Erik Lundberg, James E. Meade, G. Ugo Papi, Francois Perroux, Raul Prebisch and Erich Schneider.

It was VOTED to dissolve the Committee, with the understanding that it was to be reconstituted within the next three years.

Surveys of Foreign Economic Research (G. H. Hildebrand). In the absence of the Chairman, the Secretary presented the report, indicating that the Committee had rounded out its entire panel of proposed contributors and that it was expected that all papers would be ready by the end of 1962.

The Secretary was instructed to consult with the Chairman of the Committee about the possibility of distributing the surveys to members of the Association in the form of a monograph.

Committee on Honors and Awards (Clair Wilcox). The Chairman submitted the Committee's recommendations for the Francis A. Walker award for 1962. After discussion, the recipient was selected as the result of a ballot by members of the Executive Committee. It was VOTED that while the John Bates Clark medal should ordinarily be awarded every two years (except in the year that the Walker medal is awarded), under special circumstances the Clark medal may be awarded every year.

The President announced the appointment of Bernard Haley to succeed Clair Wilcox as

Chairman of the Committee on Honors and Awards for the three-year term beginning January 1, 1963.

Nominating Committee (T. W. Schultz). The Executive and Nominating Committees met as an Electoral College to consider nominees for the office of President-elect for 1963. After discussion, the nominee was selected and his acceptance obtained. Nominations for the other offices were discussed.

8. *Reports of Representatives.*

International Economic Association (Gottfried Haberler). Mr. Haberler outlined the activities of the International Economic Association, calling attention to the fact that conferences recently sponsored by the IEA have emphasized "refresher courses" for economists from underdeveloped countries and that the Ford Foundation and UNESCO have given support to such conferences. He proposed that a committee be formed, including B. F. Haley and R. M. Solow, to consider future activities of the IEA.

It was VOTED to continue paying dues of $400 a year to the IEA.

The President announced the following appointments as official representatives to the IEA: B. F. Haley (October, 1962, through December, 1964) and R. M. Solow (October, 1962, through December, 1966).

Institute of International Education (Theodore Morgan). In the absence of the Chairman, the Secretary presented his report, outlining the activities of the Institute over the past year and requesting that the system of appointing representatives to the Board of the Institute be put on a rotating basis in order to insure continuity. It was VOTED to accept the report.

Economic Internships (R. D. Calkins; F. C. Pierson). Following a discussion of the advisability of suggesting economists to participate in the political science internship program, it was VOTED not to take advantage of this opportunity for two reasons: (1) the size of the stipend was not large enough to attract the kind of individuals the Association would wish to recommend; (2) the program did not appear to be well adapted to the needs of economists.

Census Advisory Committee (Solomon Fabricant). No report.

Carnegie Travel Grant Committee (L. G. Reynolds). In the absence of the Chairman, the Secretary reported that there was a small balance remaining from the original grant. It was agreed that these funds should be expended to provide travel grants to members giving papers or participating in international meetings.

National Science Foundation (Carl Kaysen). No report.

ACLS (Frank Knight). No report.

SSRC (G. H. Hildebrand). No report.

NBER (W. L. Thorp). No report.

AAAS (K. E. Boulding). No report. It was VOTED to authorize the President to renew Mr. Boulding's appointment as our representative to the AAAS for one year, ending December 31, 1963.

9. *Unfinished and Miscellaneous Business.*

Membership Dues. Following a discussion, it was VOTED to raise the annual membership dues and subscriptions to the *AER* to $8.00 and the junior membership dues to $4.00, beginning January 1, 1963.

Information Clearing House of Service to Foreign Scholars Coming to the U. S. (IIE). After discussion, the Secretary was instructed to explore the possibilities further.

Charter Flights. The question was raised whether the Association should undertake to sponsor, on behalf of its members, charter flights to international meetings. After discussion, it was VOTED that the Association should not sponsor such flights.

10. *Program for 1962 Meeting* (Gottfried Haberler). Mr. Haberler outlined his plans for the 1962 program. It was VOTED to name the invited lecture series the "Richard T. Ely Series."

The meeting adjourned at 11:00 a.m., March 24.

2. Minutes of the Christmas meetings held in Pittsburgh, Pennsylvania, December 26 and 29, 1962:

The *third meeting* of the *1962 Executive Committee* was called to order on December 26 at 7:20 p.m. at the Penn-Sheraton Hotel, E. S. Mason presiding. Others present were: Ewan Clague, Gottfried Haberler, B. H. Haley, A. G. Hart, C. P. Kindleberger, R. A. Musgrave, T. W. Schultz, W. A. Wallis, and H. F. Williamson. Absent were: J. K. Galbraith, P. A. Samuelson, Robert Triffin, and R. A. Young. Present as guests were: G. L. Bach, W. J. Baumol, J. W. Bell, Solomon Fabricant, J. G. Gurley, G. H. Hildebrand, A. P. Lerner, G. J. Stigler, C. B. Warden, Jr., and B. W. Lewis. The meeting adjourned at 11:00 p.m.

The *first meeting* of the *1963 Executive Committee* was called to order at 7:00 p.m. on December 29, Gottfried Haberler presiding. Others present were: Ewan Clague, B. F. Haley, C. P. Kindleberger, A. P. Lerner, E. S. Mason, P. A. Samuelson, G. J. Stigler, W. A. Wallis, and H. F. Williamson Absent were: Gardner Ackley, W. J. Baumol, E. D. Domar, and Robert Triffin. Present as guests were: J. W. Bell, K. E. Boulding, J. G. Gurley, Theodore Morgan, and R. A. Young.

1. *Presidents' Remarks* (E. S. Mason and Gottfried Haberler). E. S. Mason reviewed the order of items on the agenda, calling attention to unfinished business which might call for action before year-end. These matters were taken up first. The minutes, however, follow the order of business as presented in the agenda.

2. *Minutes.* The minutes of the March 23-24, 1962, meeting were approved.

3. *Report of the Secretary* (H. F. Williamson). The Secretary presented the official figures for registration at the 1961 annual meeting in New York, the schedule of future annual meetings, membership growth and composition, publication costs of the 1962 *Papers and Proceedings,* requests to reprint, and the use of the mailing list—all more fully treated in the Secretary's Report. It was VOTED to approve and accept the Secretary's Report as presented.

The Secretary reviewed alternative proposals for arranging to reprint back numbers of the *American Economic Review.* It was VOTED to instruct the Secretary to complete a contractual arrangement for reprinting with the Johnson Reprint Corporation. The Secretary also asked for comments on the proposal circulated prior to the meeting to members of the Executive Committee to publish a new edition of the *Handbook* in January, 1964. It was VOTED to authorize the Secretary to complete arrangements for the publication of the *Handbook* under the terms of the proposed agreement.

It was VOTED to hold the spring meeting of the Executive Committee in New York City on March 29-30, 1963.

2. *Report of the Treasurer* (H. F. Williamson). The report of the Treasurer, printed in full below, was summarized. Special attention was called to the fact that the Association ended the year with a comfortable income and a small surplus. It was VOTED to accept the Report of the Treasurer as presented.

3. *Report of the Auditor* (H. F. Williamson). The Treasurer presented the Report of the Auditor prepared by David Himmelblau & Company. It was noted that, because of the lack of detailed reports on the expenditures under the special grants for the *Journal of Economic Abstracts* and the survey study of economic texts and because the tabulating service failed to furnish lists of those members and subscribers having prepaid dues or subscriptions at November 30, 1962, the Auditor could not verify the amounts involved in this section of his report. The Treasurer was instructed to try to clear up these matters and to present a supplementary report at the March meeting of the Executive Committee.

The Secretary was instructed to write a letter of appreciation to David Himmelblau & Company.

It was VOTED to return the unexpended balance of $178.30 of the amount originally allocated to the Committee on the *Papers and Proceedings* to the general funds of the Association.

4. *Report of the Finance Committee* (J. W. Bell). It was VOTED to accept the report of the Finance Committee, published below, and to reappoint the members of this Committee to serve during 1963. The Secretary was instructed to express to the members of the Committee the appreciation of the Executive Committee for their services.

5. *Report of the Managing Editor* (B. F. Haley). The Managing Editor reviewed his report, published below, calling special attention to his tribute to Miss Doris Merriam for her services to the *Review.* The President was asked to write a letter to Miss Merriam, thanking her on behalf of the Association for her services. It was VOTED to accept the Report of the Managing Editor.

The appointments of three new members of the Editorial Board, R. W. Clower, A. P. Lerner, and Albert Rees, and the budget of the *Review* for 1963 were approved.

It was VOTED to discontinue payments to the authors of book reviews in the *American Economic Review* and to raise the maximum rate for articles and communications to $75.

6. *Reports of Standing and Special Committees.*

Committee on Research and Publications (W. L. Thorp, Acting). In the absence of the Acting Chairman, a written report was presented. It was VOTED to accept the report. W. L. Thorp was requested to call a meeting of the Committee as soon as possible to consider (1) plans for translating foreign publications in the field of economics, (2) the feasibility of a sixth volume of the *Index of Economic Journals,* and (3) additional *Survey* volumes on contemporary economics.

Surveys of Foreign Economic Research (G. H. Hildebrand). The Chairman presented an oral report.

Journal of Economic Abstracts (C. B. Warden, Jr.). A progress report, published below, was distributed by C. B. Warden. He called special attention to the fact that the first issue of the *Journal* would be mailed to members and subscribers in January.

Committee on Economic Education (B. W. Lewis). The Chairman summarized the activities of the Committee (see below) calling special attention to the fact that the President should arrange to appoint successors to the members of the Committee whose terms have expired. He also noted that arrangements had been made to publish the report, *Economics in the Schools*, as a supplement to the March, 1963, issue of the *American Economic Review*.

National Task Force on Economic Education (G. L. Bach). The Chairman reported verbally (see below).

Following a discussion of the activities of the Committee on Economic Education and the Task Force, it was decided that G. L. Bach and B. W. Lewis should report to the March meeting on the possibility of co-ordinating the operations of the two committees.

Committee on Honors and Awards (Clair Wilcox). No report. The President announced that B. F. Haley's term as Chairman of the Committee was to be for the period of January, 1963, to December 31, 1966. Gottfried Haberler raised the question as to the possibility of expanding the system of awards to recognize the scholarly contributions of a greater number of the members of the Association. After considerable discussion, it was decided to refer the matter to the Committee on Honors and Awards.

Nominating Committee. The President announced that the following members were to serve on the Nominating Committee for 1963: P. A. Samuelson, Chairman, R. E. Baldwin, H. C. Eastman, E. O. Edwards, F. W. Fetter, and Ilse Mintz.

7. *Reports of AEA Representatives or Advisory Committees.*

ACLS (F. H. Knight).

SSRC (Gardner Ackley). The President announced the appointment of K. A. Fox to replace Gardner Ackley, who had resigned, as representative to the SSRC (for the term ending 1964).

NBER (W. L. Thorp).

AAAS (K. E. Boulding). It was VOTED to thank K. E. Boulding for his services as representative to the AAAS.

IEA (B. F. Haley).

IIE (Theodore Morgan). It was VOTED to authorize the President to indicate the support of the American Economic Association for the proposed grant for additional funds to carry on the operations of the Economic Institutes of the IIE.

Census Advisory Committee (Solomon Fabricant).

UNESCO (K. E. Boulding). It was VOTED to thank K. E. Boulding for his services as the AEA representative.

Joint Council on Economic Education (B. W. Lewis).

JCEE Special Publications Advisory Committee (G. L. Bach).

Reports received at the meeting and after are published below.

8. *Unfinished and Miscellaneous Business.*

Representation on the National Science Foundation Board. The President was asked to arrange for the appointment of a representative of the AEA to the National Science Foundation Board.

Group Flights. The Secretary was instructed to explore the possibilities of group flights for attendance at the annual meetings.

National Research Council. The President reported on the invitation for the Association to be represented on the National Research Council. It was VOTED to authorize the President to explore this further and to appoint such a representative.

Annuities. It was VOTED to extend the retirement coverage under TIAA to selected staff members of the Editor's and the Secretary's Offices.

Screening Committee for Prospective Foreign Graduate Students. The Secretary was instructed to explore with the Ford Foundation the possibilities of establishing committees for screening prospective foreign graduate students.

Allied Associations. E. S. Mason raised the question of reducing the number of associations meeting jointly with the American Economic Association, pointing out that with the increased attendance at the annual meetings the hotel accommodations and meeting room facilities were no longer adequate. After considerable discussion of these problems, it was VOTED that the President should write to the officers of the following associations, indicating that they should make their own arrangements for future annual meetings: Academy of Management, American Risk and Insurance Association, American Transportation Research Forum, and the Association for Education in International Business.

Annual Meeting. G. J. Stigler reported informally on his plans for the 1963 program.

The meeting adjourned at 9:00 p.m.

SSRC

W. H. Nicholls (1962)
George H. Hildebrand (1963)
Gardner Ackley (1964)

UNESCO

Kenneth E. Boulding

Committees Appointed During the Year

COMMITTEE ON ELECTIONS

Frank W. Fetter, *Chairman*
O. A. Smalley
Harold F. Williamson, Ex Officio

COMMITTEE ON LOCAL ARRANGEMENTS

Clyde Harrell, *Chairman*

FINANCE COMMITTEE

C. Wells Farnham, *Chairman*
Corliss D. Anderson
James Washington Bell
Harold F. Williamson, Ex Officio

SPECIAL PUBLICATIONS ADVISORY COMMITTEE, JCEE

George Leland Bach
Kenneth E. Boulding
Ben W. Lewis
James Reese
Leonard Silk

NOMINATING COMMITTEE

Theodore W. Schultz, *Chairman*
Dorothy C. Bacon
Richard E. Caves
Rendigs T. Fels
Douglass C. North
Robert M. Solow

Representatives of the Association on Various Occasions

CLARK UNIVERSITY, SEVENTY-FIFTH ANNIVERSARY
Edward Peragallo
COMMITTEE FOR ECONOMIC DEVELOPMENT, SEMI-ANNUAL TRUSTEES MEETING
Ewan Clague
INTER-AMERICAN STATISTICAL INSTITUTE, PAN AMERICAN UNION, FOURTH INTER-AMERICAN STATISTICAL CONFERENCE
Frank Garfield
UNIVERSITY OF MICHIGAN INSTITUTE FOR HUMAN ADJUSTMENT, CONFERENCE ON AGING AND THE ECONOMY
William Haber
LABOR-MANAGEMENT AND CAPITAL PLANNING CONFERENCE
George P. Schultz
DEPARTMENT OF STATE FOREIGN POLICY BRIEFING CONFERENCE
Harry G. Johnson
CHICAGO COUNCIL ON FOREIGN RELATIONS
James Washington Bell

INAUGURATIONS

John Henry Fischer, Columbia University, Teachers College
Harold Barger
Charles E. Shain, Connecticut College
Dorothy Bacon
Homer D. Babbidge, University of Connecticut
John Perry Miller

Richard Franklin Humphreys, Cooper Union
  Robert P. Shay
Sister Rita of the Sacred Heart, College of Great Falls
  J. L. Fischer
John Arthur Logan, Jr., Hollins College
  William A. Sandridge
Philip Guthrie Hoffman, University of Houston
  Edgar O. Edwards
John Anderson Hunter, Louisiana State University
  Ray Marshall
Lucius Holsey Pitts, Miles College
  R. Murray Havens
Glenn Lowery McConagha, Muskingam College
  Richard U. Sherman, Jr.
Robert A. L. Mortvedt, Pacific Lutheran College
  Homer H. Hamner
Ralph Gordon Hoxie, C. W. Post College
  Harold Wattel
Vernon Roger Alden, Ohio University
  Richard U. Sherman, Jr.
Lawrence Durwood Fleming, Southwestern University
  J. S. Spratt
Edward John Sabol, State University of New York, Agricultural and Technical Institute
  Benjamin Whittaker
Walker D. Wyman, Wisconsin State College
  Stuart M. Rich

Use of the Mailing List

Permission was granted to the following to use our mailing list:

ALDINE PUBLISHING COMPANY
AMERICAN ACADEMY OF POLITICAL AND SOCIAL SCIENCE
AMERICAN BANKERS ASSOCIATION
AFL-CIO
AMERICAN STATISTICAL ASSOCIATION
ATLANTIC MONTHLY
ALAS MAGAZINE
BROOKINGS INSTITUTION
CARNEGIE CORPORATION
UNIVERSITY OF CHICAGO PRESS
FORD MOTOR COMPANY
FORDHAM UNIVERSITY PRESS
FOUNDATION FOR THE STUDY OF CYCLES
GENERAL ELECTRIC COMPANY
HARPER AND BROTHERS
HILL AND KNOWLTON, INC.
INDICATOR DIGEST

INSTITUTE OF LIFE INSURANCE
INTERNATIONAL ECONOMIC ASSOCIATION
JOINT COUNCIL ON ECONOMIC EDUCATION
JOURNAL OF ECONOMIC ABSTRACTS
MACMILLAN COMPANY
NATIONAL ASSOCIATION OF MANUFACTURERS
NATIONAL GEOGRAPHIC SOCIETY
NEWSWEEK
OREGON STATE UNIVERSITY PRESS
ORGANIZATION OF ECONOMIC CO-OPERATION AND DEVELOPMENT
PENNSYLVANIA CONFERENCE OF ECONOMISTS
PHARMACEUTICAL MANUFACTURERS ASSOCIATION
PRINCETON UNIVERSITY PRESS
PRUDENTIAL INSURANCE COMPANY OF AMERICA
QUARTERLY REVIEW OF ECONOMICS AND BUSINESS
REPORTER MAGAZINE
ROOSEVELT UNIVERSITY
SHERATON CORPORATION
TRANS WORLD AIRLINES
UNION CARBIDE CORPORATION
WASHINGTON STATISTICAL ASSOCIATION
WESTERN ECONOMIC ASSOCIATION

Respectfully submitted,
HAROLD F. WILLIAMSON, *Secretary*

EXHIBIT I

Members and Subscribers*

	Totals 11/30/61	Gain or *Loss*	Totals 11/30/62
Class of membership:			
Annual	9,578	*21*	9,557
Junior	1,062	236	1,298
Family	159	*24*	135
Complimentary	77	16	93†
Life	159	23	182
Honorary	14	6	20
Total members	11,049	236	11,285
Subscribers	3,934	425	4,359
Totals	14,983	661	15,644

* These figures should not be considered definitive, since reports received from the data processing agency are not complete.

† Includes 21 who do not receive publications.

EXHIBIT II

Publication Costs

Papers and Proceedings				Handbooks		
Year*	Number of Pages	Number of Copies	Cost	Number of Pages	Number of Copies	Cost
1957	754	12,400	$16,253	548	10,100	$15,815
1958	677	12,700	15,471	32	9,300	1,434
1959	689	14,000	16,780			
1960	745	14,800	18,914			
1961	675	15,800	19,759			
1962	615	16,000	18,277			

* This is the year of publication and pertains to the meeting of the preceding year.

REPORT OF THE TREASURER FOR THE YEAR ENDING NOVEMBER 30, 1962

Table 1 indicates the comparative results of the operations of the Association for the fiscal years 1961 and 1962. It may be noted that the net operating income was $4,807 in 1962 compared with $18,090 in 1961. This decrease was principally the result of: (1) a reduction in income—chiefly in the profit on the sale of securities; (2) an increase in expenditures—particularly in the cost of accounting services and other administrative expenses and editorial and clerical salaries for the *AER*. It is expected that the accounting expenses and editorial salaries (which included a nonrecurring item in 1962) will be lower in 1963 than in 1962.

The comparative financial status for 1961 and 1962 is shown in Table 2. The fact that the unappropriated surplus at the end of the fiscal 1962 was $5,138 less than for 1961 was the result of expenditures on the *Index of Economic Journals* of $9,945 in excess of the original foundation grant for this publication.

Tables 3 and 4 give summaries of investment portfolios and return on investments for the years 1925-62.

With the raise in dues and subscriptions and further growth in the membership of the Association, the income for 1963 should show a substantial increase. It is hoped that the cost of the 1964 *Handbook* will be covered by sales to subscribers and others. It is possible, however, that the expense of publishing the *Handbook* may be higher than receipts from sales, particularly during the fiscal year of 1963.

Respectfully submitted,
HAROLD F. WILLIAMSON, *Treasurer*

TABLE 1

Comparative Results of Operations, 1961 and 1962

	11/30/61	11/30/62
Income		
Membership dues	$ 61,980	$ 63,052
Subscriptions	24,610	26,823
Sales	3,241	4,092
Advertising	19,614	20,920
Republications income	5,439	7,692
Sale of mailing list	6,338	2,830
Sundry income	816	1,662
Dues and publications income	$122,038	$127,071
Interest	$ 5,460	$ 4,838
Dividends	4,143	4,489
Less custodian fees	381*	372*
Profit on sales of securities (net)	12,712	2,578
Investment income (less fees)	$ 21,934	$ 11,533
Total income	$143,972	$138,604
Expenses		
Office salaries	$ 30,598	$ 31,239
Accounting service (IBM agency)	6,000	11,600
Other administrative expenses	8,740	10,707
Annual meeting	2,592*	7,714*
Executive Committee	1,803	1,286
Other committee expenses	827	1,249
Administrative and operating expenses	$ 45,376	$ 48,367
Review printing	$ 41,123	$ 43,780
Papers and Proceedings printing	19,759	18,277
Editorial office (*Review*):		
Contributors	1,458	2,926
Editorial and clerical salaries	17,468	20,078
Other expenses (net)	698	369
Publications	$ 80,506	$ 85,430
Total expenses	$125,882	$133,797
Net operating income or loss	$ 18,090	$ 4,807

* Denotes red.

TABLE 2

Comparative Financial Condition, 1961 and 1962

	11/30/61	11/30/62
Assets		
Cash on deposit and on hand	$ 13,469	$ 51,191
Receivables (net)	11,095	12,714
Prepaid expenses and inventories	1,911	785
Furniture and fixtures (net)	1,729	1,762
nvestments at cost:		
Bonds	169,794	125,367
Stocks	109,071	116,699
Total assets	$307,069	$308,518
Liabilities and Funds		
Accounts payable	$ 13,116	$ 11,856
Deferred income	13,767	14,323
Committee on Research and Publications	353	353
Committee on Economic Education	108	108
Committee on *Papers and Proceedings*	250	178
Outside grants:		
Index of Economic Journals (Ford)	5,417*	—
Register of economists (Ford)	1,836	1,836
Economic abstracts (Ford)	7,000	8,197
Social studies survey (Ford)	12,681	12,681
Foreign economic research translations (Ford)	25,000	25,000
Foreign economic surveys (Ford)	39,085	39,085
Survey articles (Rockefeller)	3,200	3,250
Travel grant (Carnegie)	2,239	1,579
Asia Foundation grant	2,162	1,188
Sundry	611	644
Life memberships	16,300	18,600
Total liabilities and funds	$132,291	$138,878
Surplus		
Balance at beginning of period	$156,788	$174,778
Expenditures in excess of grant, *Index of Economic Journals* (Ford), 1961, $5,417; 1962, $4,528		9,945*
Transfers from life memberships	100†	—
Net income or loss for period	18,090	4,807
Unappropriated surplus	$174,778	$169,640
Total footings	$307,069	$308,518

* Denotes red.

† Reinstated life member.

TABLE 3

INVESTMENT PORTFOLIO

YEAR	AT PAR	COST			MARKET Stocks and Bonds
	Bonds	Bonds	Stocks	Total	
1925	$ 25,000	$ 24,661		$ 24,661	
1930	31,000	32,439		32,439	
1933	33,500	32,962	$ 3,954	36,916	$ 31,522
1935	16,000	15,280	28,114	43,394	50,338
1940	25,000	22,519	41,155	63,675	60,553
1942	27,000	24,651	41,556	66,207	58,211
1945	40,000	36,705	44,955	81,661	103,574
1948	35,000	33,108	48,624	81,732	84,841
1950	35,000	33,108	51,978	85,087	104,177
1951	43,000	43,340	49,764	93,104	117,316
1952	42,000	42,312	58,934	101,246	130,836
1953	68,000	68,308	46,458	114,766	134,562
1954	61,000	61,518	38,082	99,600	132,280
1955	75,000	75,370	59,394	134,764	166,772
1956	75,000	75,370	60,237	135,607	168,337
1957	75,000	75,370	55,084	130,454	151,638
1958	75,000	75,370	67,741	143,111	175,609
1959	75,000	75,386	67,652	143,038	191,506
1959*	175,000	175,616	67,652	243,268	291,506
1960*	160,000	160,508	94,910	255,418	299,768
1961*	170,000	169,794	109,071	278,865	356,131
1962*	125,000	125,367	116,699	242,066	293,039

* Includes bonds held in temporary operating fund.

TABLE 4

RETURN ON INVESTMENTS

Year	Bonds	Stocks	Total	Rate of Return on Cost
1925	$1,350		$ 1,350*	
1930	1,695		1,695	5.22%
1933	1,679	$ 108	1,788	4.84
1935	1,022	680	1,703	3.92
1940	1,037	2,182	3,220	5.06
1942	1,306	2,186	3,492	5.28
1945	1,479	2,488	3,968	4.71
1948	1,194	2,944	4,139	5.06
1950	1,117	3,860	4,977	5.85
1951	1,026	4,607	5,633	6.05
1952	1,117	3,681	4,799	4.75
1953	1,435	3,587	5,022	4.36
1954	1,621	2,961	4,582	4.58
1955	1,750	3,002	4,752	3.53
1956	1,770	3,336	5,106	3.76
1957	1,770	3,397	5,167	3.90
1958	1,770	3,182	4,952	3.46
1959†	2,518	3,231	5,749	3.90
1959§	3,894	3,231	7,125	2.90
1960§	6,693	3,772	10,465	4.09
1961	5,460	4,143	9,603	3.44
1962	4,838	4,489	9,327	3.85

* Estimated income for year.
† Does not include income from bonds held in temporary operating fund.
§ Includes income from bonds held in temporary operating fund.

REPORT OF THE FINANCE COMMITTEE

December 22, 1962

Executive Committee
American Economic Association
Evanston, Illinois

GENTLEMEN:

Accompanying this report is a list of securities held by the Association at the end of the 1962 fiscal year. Bonds are arranged according to maturity and the common stocks alphabetically. According to our records, the securities account of the Association, which does not include the temporary operating funds, had a market value on November 30, 1961, of $296,221. During the 1962 fiscal year proceeds of common stock sales equaled $6,660 and cost of purchases of common stocks $14,070. The only bond transaction involved the maturity of $20,000 Treasury Bonds, the proceeds of which were invested in a savings account. The total market value of the securities account as of November 30, 1962, was $278,039, a decline for the year of $18,092. Adjusted for the additional investment of $7,409, the total decline in market value was $25,501, which is approximately 8 per cent of the account, or something over 13 per cent of the common stock portion, where all of the decline took place.

We call attention to the fact that the temporary operating funds referred to above declined during the course of the 1962 fiscal year from $60,000 to $55,000. We wish to suggest that these funds be separated completely from the securities account and become a matter of the responsibility and report of the Association treasurer.

A summary of sales and purchases made during the year accompanies the list of securities.

Income received from dividends and interest totaled $8,641, which will vary slightly from the auditor's report chiefly because our Committee's total does not include accrued interest on savings deposits.

The ratio of fixed-income securities to common stocks at the end of the fiscal year was about the same as the previous year.

Respectfully submitted,
C. WELLS FARNHAM, *Chairman*
CORLISS D. ANDERSON
JAMES WASHINGTON BELL
HAROLD F. WILLIAMSON

LIST OF SECURITIES HELD BY THE ASSOCIATION

Stocks

Number of Shares of Stock	Issue	Cost	Approximate Market Value 11/30/62
100	Abbott Laboratories	$ 6,133	$ 7,000
150	American Potash and Chemical Corp.	6,284	4,650
104	Castle and Cooke	3,672	2,808
300	Central & South West Corp.	2,101	12,300
150	Chain Belt Co.	6,621	5,400
50	Continental Illinois National Bank & Trust Co.	6,619	7,050
100	Deere & Co.	4,240	5,300
100	Farbenfabriken—Bayer AG	6,822	6,200
200	Gulf Oil Corp.	1,393	7,600
100	Houston Lighting and Power Co.	1,240	10,500
100	Inland Container Corp.	4,944	4,400
30	International Business Machines Corp.	9,086	11,940
100	International Nickel Co. of Canada	3,911	6,500
100	McIntyre Porcupine Mines, Ltd.	4,818	4,000
158	Monsanto Chemical Co.	5,474	7,742
150	Montgomery Ward and Co.	5,013	5,100
100	Motorola	6,746	6,300
200	Olin Mathieson Chemical Corp.	8,731	6,600
220	Peoples Gas Light and Coke Co.	3,562	11,220
100	Siemens and Halske	5,519	7,300
150	Socony Mobil Oil Co.	3,882	8,400
100	Standard Oil Co. (Ind.)	3,650	4,800
121	Wells Fargo Bank—San Francisco	4,261	7,139
150	Zenith Radio Corp.	1,977	8,100
		$116,699	$168,349

Bonds

Par Amount		Cost	Approximate Market Value 11/30/62
$20,000	Savings Deposit 4%	$ 20,000	$ 20,000
20,000	U.S. Treas. Bonds 2½% due 8/15/63	20,091	20,000
5,000	U.S. Treas. Notes 3¾% "D-1964," 5/15/64	5,000	5,050
8,000	U.S. Treas. Notes 1½%, EO-1964, 10/1/64	8,000	7,840
50,000	U.S. Treas. Bonds 3¾%, due 5/15/66	50,000	50,500
7,000	U.S. Treas. Bonds 2½%, due 12/15/72–67	7,276	6,300
	Bonds	$125,367	$109,690
	Stocks	116,699	168,349
	Total	$242,066	$278,039

TRANSACTIONS SINCE 11/30/61

Bought		Cost
30	IBM	$ 9,086.25
8/100	Gulf Oil	3.42
100	Inland Container	4,943.50
70/100	Monsanto	36.88
		$14,070.05

Sold		Selling Price
200	Outboard Marine	$ 4,060.01
200	Raymond International	2,552.90
1	Standard Oil of New Jersey	47.42
		$ 6,660.33

Matured		Proceeds
$20,000	U.S. Treas. Notes, due 2/15/62	$20,000.00

REPORT OF THE AUDITOR

December 18, 1962

Executive Committee
American Economic Association
Evanston, Illinois

Dear Sirs:

In accordance with instructions, we have examined the accounts and related records of the American Economic Association for the year ended November 30, 1962, and now submit our report thereon together with the following exhibits:

Statement of Financial Position—November 30, 1962	Exhibit 1
Statement of Income and Expense for Year Ended November 30, 1962	Exhibit 2
Special Funds—Year Ended November 30, 1962	Exhibit 3

Results from Operations

Net income for the year ended November 30, 1962, was $279 compared with net income of $12,673 for the year ended November 30, 1961, as shown in the following summary:

Particulars	Year Ended November 30 1962	1961	Increase Decrease*
Income:			
Dues	$63,052	$61,980	$ 1,072
Interest and dividends (net)	8,955	9,222	267*
Profit on sale of securities (net)	2,577	12,712	10,135*
Miscellaneous income	1,662	816	846
Total income	$76,246	$84,730	$ 8,484*
Expense:			
Publication expense	$85,430	$80,506	$ 4,924
Less—Publication income	59,527	52,903	6,624
Net publication expense	$25,903	$27,603	$ 1,700*
Administrative and other operating expenses	45,536	39,037	6,499
Appropriations	4,528	5,417	889*
Total expense	$75,967	$72,057	$ 3,910
Net income	$ 279	$12,673	$12,394*

During the current year recording of dues and subscriptions was in the hands of a tabulating service. See statement of auditors' responsibility on last page regarding accuracy of prepaid dues and subscriptions which affects the income shown in the above tabulation.

Interest on bonds owned was accounted for in accordance with stated rates;

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* Denotes red.

dividends received on stocks were compared with amounts reported in published records as dividends paid.

Net publication expense, as shown in the following summary, amounted to $25,903 for the current year compared with $27,603 for the preceding year:

	Year Ended November 30		Budgetary Estimates for
Particulars	1962	1961	Year 1962
Expenses:			
Printing of—			
Review	$43,780	$41,123	$45,000
Proceedings	18,277	19,759	20,000
Editors honorarium	8,000	7,833	8,000
Payments to contributors	2,926	1,458	2,000
Editorial clerical salaries	12,078	9,635	9,600
Editorial supplies and expense	369	698	700
Total expenses	$85,430	$80,506	$85,300
Less—Income:			
Subscriptions, other than members	$26,823	$24,610	
Sales of copies	4,092	3,241	
Advertising	20,920	19,614	
Republications	7,692	5,438	
Total income	$59,527	$52,903	
Net publication expense	$25,903	$27,603	

Charges for the cost of printing the December, 1962, issue of the *Review* and reprints had not been made by the publishers at the time of our examination. The publishers estimate the cost of the *Review* printings and reprints at $10,643; this amount is included in the expenses for the year.

Financial Position

Financial position of the Association at November 30, 1962, and 1961 is set forth in the following summary:

	November 30		Increase
Assets	1962	1961	Decrease*
Cash on deposit and on hand	$ 51,191	$ 13,469	$37,722
Receivables (net)	9,914	8,794	1,120
Advances to Foundation Project Chairmen	3,800	2,300	1,500
Prepaid expenses	785	1,911	1,126*
Equipment, furniture and fixtures (net)	1,762	1,729	33
Investments at cost—			
Bonds	125,367	169,794	44,427*
Stocks	116,699	109,071	7,628
	$309,518	$307,068	$ 2,450
Liabilities, Funds and Surplus			
Accounts payable	$ 11,856	$ 13,116	$ 1,260*
Deferred income	14,323	13,767	556
Funds	95,099	94,524	575
Life memberships	18,600	16,300	2,300
Unappropriated surplus—			
Balance at beginning of year	169,361	156,788	12,573
Net income for year	279	12,673	12,394*
Life memberships transferred to surplus	—	100*	100
	$309,518	$307,068	$ 2,450

* Denotes red.

Cash on deposit was satisfactorily reconciled with balances confirmed directly to us by the depositories.

The receivables of the Association were not confirmed by correspondence with debtors. Based upon the Association's past experience the reserve for doubtful accounts appears to be adequate to cover normal losses.

Changes in the investment account were verified by the examination of brokers invoices and other supporting data. Securities held at November 30, 1962, were confirmed directly to us by the State Bank and Trust Company of Evanston, Illinois, custodian for the Association.

Funds

The receipts and expenditures during the year for the various funds and grants are detailed in Exhibit 3. Total funds available for specific projects increased to $95,099.08 at November 30, 1962.

Statement of Auditors' Responsibility

During the course of the examination, we noted that the following balances could not be verified, for the reasons given hereunder:

(1) Fund for *Journal of Economic Abstracts* .... Committee has not furnished a report of expenditures for $1,000 advanced on November 12, 1962.	$ 9,197.45
(2) Fund for survey study of economic text books in secondary schools ..... Committee has not furnished a report of expenditures for the $2,800 advanced to them out of this fund. Last report on this project was dated July 23, 1960.	12,680.91
(3) Prepaid dues at November 30, 1962	3,151.47
(4) Prepaid subscriptions at November 30, 1962 .... The tabulating service has failed to furnish lists of those members having prepaid dues or subscriptions at November 30, 1962, so these balances could not be verified.	11,171.21

Because of the foregoing items we are unable to express an opinion as to the financial statements.

We express our appreciation for the courtesies and co-operation extended our representatives during the course of the examination.

Very truly yours,

DAVID HIMMELBLAU & CO.
Certified Public Accountants

EXHIBIT 1

AMERICAN ECONOMIC ASSOCIATION
STATEMENT OF FINANCIAL POSITION—NOVEMBER 30, 1962

Assets

CURRENT ASSETS:		
Cash on deposit and on hand—		
State Bank and Trust Company, Evanston—		
Commercial account	$ 10,204.62	
Savings account	40,926.62	
Petty cash	60.00	$ 51,191.24
Receivables—		
Review advertising	$ 6,062.50	
Accrued interest and dividends	699.08	
Publication sales, etc.	3,139.96	
Membership dues	165.00	
	$ 10,066.54	
Less—Reserve for doubtful accounts	152.55	9,913.99
Advances to Foundation Project Chairmen		
Journal of Economic Abstracts	$ 1,000.00	
Economic text books in secondary schools	2,800.00	3,800.00
Inventory of envelopes and postage		543.85
Unexpired insurance		240.65
Total current assets		$ 65,689.73
INVESTMENTS (at cost):		
Bonds (market value—$124,452.40)	$125,367.10	
Stocks (market value—$166,785.76)	116,699.26	242,066.36
EQUIPMENT, FURNITURE AND FIXTURES (less accumulated depreciation)		1,761.76
		$309,517.85

Liabilities, Funds and Surplus

CURRENT LIABILITIES:			
Accounts payable			$ 11,856.07
DEFERRED INCOME:			
Prepaid subscriptions		$ 11,171.21	
Prepaid dues		3,151.47	14,322.68
SPECIAL FUNDS (Exhibit 3)			95,099.08
LIFE MEMBERSHIPS AND SURPLUS:			
Life memberships		$ 18,600.00	
Unappropriated surplus—			
Balance November 30, 1961	$169,360.78		
Net income for year ended November 30, 1962 (Exhibit 2)	279.24	169,640.02	188,240.02
			$309,517.85

EXHIBIT 2

American Economic Association
Statement of Income and Expense
for the Year Ended November 30, 1962

Particulars			*Amount*	
Income:				
Dues—				
Regular, junior and family members			$57,547.33	
Subscribing and contributing members			5,504.50	$63,051.83
Investments—				
Interest on bonds and savings account			$ 4,837.77	
Dividends			4,488.76	
			$ 9,326.53	
Less custodian fees			371.82	8,954.71
Gain on sale of securities				2,577.51
Miscellaneous income				1,662.27
Total income				$76,246.32
Expense:				
Administrative and other expense—				
Secretary's salary		$ 8,666.66		
Office salaries		22,572.74		
Addressing service income less expense		2,829.69*		
Accounting service (IBM service)		11,600.00		
Postage		2,207.14		
Stationery and printing		2,604.78		
Insurance		168.54		
Executive Committee expense		1,285.98		
Other committee expense		1,249.47		
Annual meeting income less expense		7,713.89*		
Annuity payments		1,034.60		
Social security taxes		1,166.52		
Provision for depreciation		583.14		
Telephone and telegraph		472.34		
International Economic Association		400.00		
Office supplies		624.82		
Miscellaneous expense (net)		1,442.85	$45,536.00	
Publication expense—				
Printing of—				
Review		$43,780.14		
Proceedings		18,277.42		
Editorial honorarium		8,000.00		
Payments to contributors		2,925.50		
Editorial clerical salaries		12,077.99		
Editorial supplies and expense		369.39		
		$85,430.44		
Less—Publication income:				
Subscriptions other than members	$26,822.69			
Sales of copies	4,092.32			
Advertising	20,920.00			
Republications	7,692.42	59,527.43	25,903.01	71,439.01
				$ 4,807.31
Less—Expenditures in excess of grant on Cumulative Index Project for year ended November 30, 1962 (Exhibit 3)				4,528.07
Net income for year (Exhibit 1)				$ 279.24

* Denotes red.

EXHIBIT 3

AMERICAN ECONOMIC ASSOCIATION
STATEMENT OF SPECIAL FUNDS
FOR YEAR ENDED NOVEMBER 30, 1962

Fund	Balance November 30, 1961	Received	Expended	Balance November 30, 1962
Carnegie Corporation of New York grant for travel expenses of delegates to international meetings....	$ 2,238.64	$ —	$ 660.00	$ 1,578.64
Rockefeller Foundation grant for survey articles on recent developments	3,200.00	3,300.00	3,250.00	3,250.00
The Ford Foundation grants—				
Preparation of a special register of economists....................	1,835.50	—	—	1,835.50
Translation of books and articles of major significance into the English language..................	25,000.00	—	—	25,000.00
Preparation and publication of articles surveying economic research in foreign countries............	39,085.07	—	—	39,085.07
Survey study of economic text books in secondary schools......	12,680.91	—	—	12,680.91
Fund for *Journal of Economic Abstracts*.......................	7,000.00	10,000.00	7,802.55	9,197.45
Committee on Economic Education..	107.80	—	—	107.80
Committee on *Papers and Proceedings*	250.00	—	71.70	178.30
Committee on Professional Standards and Ethics.................	500.00	—	—	500.00
Asia Foundation...................	2,161.79	—	973.55	1,188.24
Committee on Publication and Research..........................	353.17	—	—	353.17
Sundry...........................	111.00	100.00	67.00	144.00
Total funds (Exhibit 1)........	$94,523.88	$13,400.00	$12,824.80	$95,099.08
The Ford Foundation grant for preparation of a cumulative index of economic journals (exhausted in 1961)—				
Expenditures in excess of grant for year ended November 30, 1962 (Exhibit 2)............			$ 4,528.07	

REPORT OF THE MANAGING EDITOR FOR THE YEAR ENDING DECEMBER 1962

The number of manuscripts received during 1962 was 273, approximately the same as in 1959 and 1960, and substantially less than in the peak year of 1961. Table 1 gives comparative figures for the past five years and for 1952, the first year in which I served as Managing Editor.

TABLE 1

MANUSCRIPTS SUBMITTED 1958–62 AND 1952

	1962	1961	1960	1959	1958	1952
Manuscripts received	273	305	276	279	242	190
Articles	157	179	158	180	151	133
Communications	116	126	118	99	91	57
Percentage of articles accepted	15	12	16	14	17	19

Table 2 provides the breakdown of the volume's contents between articles, review articles, communications, book reviews, etc. The allocation of space between the various sections of the *Review* has remained about the same as in recent years. The 1962 volume, however, was larger than in recent years by about 85 pages. A large part of this increase is accounted for by the fact that the volume included two survey articles instead of one (as in 1960 and 1961) aggregating 97 pages (as compared with 35 in 1960). Since the survey articles are largely financed by a grant from the Rockefeller Foundation, they are treated as net additions to the volume.

TABLE 2

SUMMARY OF CONTENTS, 1960–62

	1962		1961		1960	
	Number	Pages	Number	Pages	Number	Pages
Leading articles	18	442	20	441	20	478
Review articles	5	72	2	32	3	67
Communications:						
Original	7	65	10	66	9	49
Comments and replies	16	84	15	48	14	36
Book reviews	197	386	210	377	196	342
Classified lists:						
New books	—	68	—	70	—	62
Periodicals	—	50	—	60	—	61
Dissertations	—	33	—	41	—	28
Notes	—	52	—	33	—	42
		1,252*		1,168		1,165*

* Plus some blank pages.

The two survey articles published in 1962, the fourth and fifth of the series of eight, were: "Research on Household Behavior," by Robert Ferber, and "Monetary Theory and Policy," by H. G. Johnson. The sixth article, on "Regional Economics," by John R. Meyer, is included in the March, 1963, issue, now in press; and the seventh and eighth articles are also scheduled for publication in 1963. The Rockefeller Foundation grant, which has made these articles possible, terminates in October, 1963.

Table 3 summarizes the subject-matter distribution of articles, review articles, and communications for the past five years; the figures in parentheses give the distribution for 1962 only. The most interesting figures are those for leading articles plus original communications, since to some extent these figures indicate the areas in which the most work of publishable quality and of broad interest to economists is being done. The five fields showing the highest concentration for the last five years are as follows: Price and Allocation Theory (22), Economic Development and National Economies (16), Income and Employment Theory (15), International Economics (14), and Money and Banking (9).

TABLE 3

SUBJECT-MATTER DISTRIBUTION: ARTICLES AND COMMUNICATIONS, 1958–62 AND 1962

	Articles	Review Articles	Original Communications	Comments: Replies	Totals
General economics........	4	2	3	3	12
Price theory............	17 (4)	3 (2)	5 (3)	5	30 (9)
Income theory..........	11 (2)	2	4 (1)	11 (5)	28 (8)
History economic thought	4 (1)	—	1	2	7 (1)
Economic development, national economies.....	12 (3)	3 (2)	4	14	33 (5)
Social accounting........	3	1	1 (1)	1 (1)	6 (2)
Economic systems.......	3 (1)	—	—	—	3 (1)
Business fluctuations.....	7 (1)	1	1 (1)	1	10 (2)
Money and banking.....	5 (1)	3 (1)	4	8 (4)	20 (6)
Public finance	2	1	4	8	15
International economics..	11 (1)	2	3	4 (2)	20 (3)
Business finance.........	3	—	—	5 (2)	8 (2)
Business organization....	3 (1)	—	—	—	3 (1)
Industrial organization...	5 (1)	—	1	6 (2)	12 (3)
Land economics.........	3	1	2 (1)	—	6 (1)
Labor economics........	4 (1)	—	1	2	7 (1)
Consumption; population.	5 (1)	—	1	2	8 (1)
Unclassified............	1	—	4	1	6
	103 (18)	19 (5)	39 (7)	73 (16)	234 (46)

NOTE: The 1958–62 figure is followed in each case by the 1962 figure in parentheses.

Table 4 presents the expenditures in 1962 for the four regular issues of the *Review* in comparison with the estimated budget and with the actual expenditures in 1961. The budget figures for 1962 did not include the estimated expense of publishing two survey articles, but the actual expenditure figures for the year do include this expense. This explains the difference between actual and estimated printing and mailing costs, payments to contributors, and some of the increase in editorial assistance. The survey articles grant from the Rockefeller Foundation provided $3,250 for the two survey articles, which takes care of most of the difference between the estimated budget and the actual expenditures for 1962. Other items not allowed for in the estimated budget but included in the actual expenditures were $1,500 for salaries for the Acting Managing Editor and Acting Associate Editor who served during my three-month leave of absence from the *Review;* and a salary increase for Miss Merriam.

TABLE 4

Actual and Budgeted Expenditures

	Budget 1962	Actual 1962	Actual 1961
Printing and mailing	$46,700	$47,938.91	$44,146.91*
Editor's salary	8,000	8,000.00	8,000.00
Acting editors†		1,500.00	
Editorial assistance	9,750	10,385.05	9,750.79
Supplies	900	851.95	901.25
Contributors	2,500	3,375.00	2,921.90
	$67,850	$72,050.91	$65,720.85

* Corrected from 1961 Report.
† For three months.

Table 5 gives detailed information about printing cost by issues. The number of copies printed in 1961 averaged 15,650; in 1960, 15,000; in 1959, 14,125; in 1958, 12,975; in 1952, 10,725. The successive increases are of course mainly related to the increases in membership and subscriptions.

TABLE 5

Copies Printed, Size and Cost of Printing, 1962

	Copies Printed	Pages		Issue	Reprints	Cost Including Reprints
		Net	Gross			
March	16,000	333	400	$12,527.35	$100.74	$12,628.09
June	16,000	350	400	12,329.23	127.40	12,456.63
September	16,000	309	356	11,412.75	102.54	11,515.29
December	16,500	264	334†	10,228.90†	100.00†	11,338.90†
		1,256*	1,490	$46,498.23	$430.68	$47,938.91

* Includes four blank pages.
† Estimated.

The estimated costs for the coming year are presented in Table 6, based on a volume of 1,420 pages, including advertising (or about 1,150 pages of text, exclusive of three survey articles) and an average number of copies of 16,750. Anticipated cost of survey articles is not included, since this cost will be substantially covered by the Rockefeller Foundation grant. Anticipated cost of increased postage for mailing the *Review* is also not taken into account.

TABLE 6

RECOMMENDED BUDGET FOR 1963

Printing (including paper, postage, reprints)	$46,500
Managing editor's salary	8,000
Assistant editor	3,000
Editorial and clerical assistance	9,650
Supplies and postage	970
Contributors	2,500
	$70,620

During the year I have had the advice and assistance of the following foreign correspondents—who have been particularly helpful with regard to the selection of foreign books for listing and review:

Isaac Kerstenetzky (Brazil)
Maurice Flamant (France)
Erich Schneider (Germany)
Oscar Soberón (Mexico)
P. J. Verdoorn (Netherlands)
Bent Hansen (Sweden)
Paolo Sylos Labini (Italy)

Three members of the Board of Editors complete their three-year terms of office at this time: M. W. Reder, Tibor Scitovsky, and Robert Solow. The Association owes them a heavy debt of gratitude for the generous expenditure of time they have made in the interests of the *Review*, and I very much appreciate their constant willingness, even when other obligations pressed, to review manuscripts and offer editorial advice. I nominate as their successors on the Board, for three-year terms beginning in 1963: R. W. Clower, A. P. Lerner, and Albert Rees.

During the year I have frequently sought the aid of members of the profession in addition to the members of the Editorial Board—partly to relieve the latter of what would otherwise be an impossibly heavy burden and partly to obtain advice of specialists in particular areas not represented on the Board. The following have assisted in this way:

G. Ackley
I. Adelman
M. A. Adelman
A. Alchian
S. Alexander
E. Ames
K. Arrow
S. T. Beza
A. Bloomfield
E. C. Brown
P. Cagan
W. Capron
N. Chamberlain
A. Coale
E. Denison
E. Despres
R. Eckaus
A. Eckstein
O. Eckstein
E. O. Edwards
R. Eisner
H. S. Ellis
W. J. Fellner
R. Goode
J. G. Gurley
D. Hamberg
E. O. Heady
R. B. Heflebower
J. Hirschleifer
H. S. Houthakker
D. Humphrey
G. Jaszi
D. Jorgenson
G. Katona
J. A. Kershaw
C. Kindleberger
S. Klaman
L. R. Klein
R. J. Lampman
S. Lebergott
S. Maisel
A. Manne
J. W. Markham
G. Meier
A. Meltzer
M. Michaely
H. Minsky
F. Modigliani
J. N. Morgan
R. Powell
W. S. Salant
T. W. Schultz
C. Schultze
R. E. Seldon
E. S. Shaw
V. Smith
W. Smith
J. Spengler
P. O. Steiner
G. J. Stigler
D. Suits
R. Summers
B. C. Swerling
S. C. Tsiang
H. C. Wallich
J. F. Weston
C. R. Whittlesey
S. Winter

It is with great regret that I report the resignation of Miss Doris Merriam, assistant to the Managing Editor, who after sixteen years has given up her position in order to serve with the Peace Corps in Latin America. Needless to say, her excellent editorial ability, intelligent good judgment, and complete devotion to the task have been very much missed. However, we are fortunate indeed in finding Miss Jane Ladd, formerly on the staff of the dean of graduate study at Stanford University, willing to take over Miss Merriam's heavy responsibilities.

I greatly appreciate the helpfulness and unfailing support that I have received from successive Executive Committees of the Association over the period of approximately eleven years that I have served as Managing Editor. These relations have been most pleasant, as have also been my relations with Jim Bell and Hal Williamson.

To John G. Gurley go my best wishes for a highly successful term as the fourth managing editor of the *American Economic Review.*

Respectfully submitted,
B. F. HALEY, *Managing Editor*

REPORT OF COMMITTEE ON RESEARCH AND PUBLICATIONS

Two additional sets of readings sponsored by the Association are being prepared. One is a volume of *Readings in Welfare Economics,* edited by Kenneth Arrow and Tibor Scitovsky. The second is a new *Readings in Business Cycles,* edited by R. A. Gordon and Lawrence Klein.

With respect to the project for the translation and publication of significant foreign-language books, little progress has been made. In May, 1959, some twenty-nine books were suggested by various persons familiar with the foreign literature. A similar inquiry made in November, 1962, yielded nearly twenty titles, some of which overlapped with the earlier list. However, while there seems to be some interest in obtaining translations, no translators have presented themselves. The Committee is considering ways and means of stimulating this activity. An announcement concerning the availability of funds will appear in the *American Economic Review* for March, 1963.

The fifth volume of the *Index of Economic Journals* covered the period 1954-59. The new quarterly *Journal of Economic Abstracts* is to appear in December, 1962, and will presumably cover part of that year. Assuming that the *Journal* will provide much of the same coverage from 1963 on, it has been suggested that a sixth volume be added to the *Index* to cover the gap; namely, 1960-62. No decision has been made concerning this proposal. It depends upon availability of personnel and funds as well as an evaluation of the demand.

WILLARD L. THORP

COMMITTEE ON ECONOMIC EDUCATION

The Committee in 1962 carried out its traditional role of arranging the session on economic education for the annual meeting at Pittsburgh. In addition, the report of the subcommittee appointed to study the leading textbooks used in high school courses in economics, social studies, and American history was brought to completion and presented to the Secretary for publication as a supplement to the *Review*.

Members of the Committee are concerned to make the Committee (as presently constituted a nonoperating committee) more active and its work more effective. It is suggested that a committee which might combine the special features and the kinds of programs now undertaken by the Committee on Economic Education and the National Task Force may be the solution. There are difficulties, particularly in establishing workable lines of authority and responsibility between the Association and any such committee and keeping the burden of the committee from being too great for its members, but developments along this line should be explored.

BEN W. LEWIS, *Chairman*

REPORT OF NATIONAL TASK FORCE ON ECONOMIC EDUCATION

The report of the National Task Force on Economic Education, *Economic Education in the Schools*, was published in September, 1961. Through a grant provided by the Committee for Economic Development, a copy was sent to every high school in the United States, to a large number of leading educational officials, to leading businessmen, and to others interested in economic education in the secondary schools. A brief summary of the report was sent to a still wider group, including school board members throughout the United States. A total of about 150,000 copies was distributed.

The response to the report has been widespread and predominately very favorable. Together with the national television program, "The American Economy" (on which we comment in more detail below), this report appears to have had a widespread impact on high school teachers and officials, both in stirring up more interest in improved teaching of economics in the high schools and in providing some guidance as to directions of improvement. Hundreds of letters have been received on the report, the vast majority of them friendly. At the same time there was some very active criticism, notably from very conservative elements of the business community, which argued that the report was not sufficiently friendly to the "private enterprise system" and that it should have taken as a major goal indoctrination of high school students.

There is no way to evaluate precisely the impact of the work of the National Task Force. National educational officials at the high school level tell us that it is having a nationwide effect and is proving useful to superintendents, curriculum planners, and teachers. We know of a considerable number of concrete instances where this is true, including several larger city systems.

Following completion of its report, the National Task Force agreed to serve as the Advisory Committee to Professor John R. Coleman on "The American Economy"—the national television program designed especially to help educate high school teachers who are responsible for what economics is taught in the schools. Professor Coleman took the report of the National Task Force as a general frame of reference in designing the course. He had the advice of the National Task Force in designing the course and on a number of matters throughout the year in which the course has been put on television, though he as an individual is fully responsible for the day-to-day contents of the program. Some forty leading economists and public officials, chosen with the general advice of the National Task Force to assure reasonable balance points of view in the program, have appeared. A full description of the course appeared in the *American Economic Review* for September, 1962.

It is too early to evaluate the success of "The American Economy." Preliminary figures from CBS, the television network carrying the program, and from educational television stations which also carry it, indicate a daily watching audience of 1,000,000 to 1,250,000. This is far more than have watched any of the preceding nationwide educational television programs. The program is currently offered in every state for college credit at 388 colleges and universities. It is being used by numerous high school systems for the further training in economics of teachers in the social studies. A considerable number of business firms are encouraging employees to watch the program, including provision of textbooks and in some instances time off for the course. Some colleges are using it directly as an experiment in on-campus teaching, and some high schools are experimenting with advanced placement senior-level students who are taking the course directly. The Advisory Committee will attempt to provide a fuller report and evaluation a year hence when the program has been completed.

G. L. BACH, *Chairman*

REPORT OF THE CENSUS ADVISORY COMMITTEE

The Census Advisory Committee of the American Economic Association met with the Director and Executive Staff of the Census Bureau in March, 1962. Principal topics discussed were: plans for 1963 economic censuses; the Census Bureau running inventory of "projects for consideration"; criteria for Census Bureau projects; progress on preliminary plans for a "Census of Wealth"; availability of regional economic data; Census tabulations of foreign trade data, including their relation to domestic output and consumption; the "one-in-a-thousand" population Census sample; and the confidentiality of respondents' file copies of Census questionnaires.

In addition, the Committee set up a Technical Subcommittee on Business Cycle Developments, which met with the Director and Executive Staff of the Census Bureau in May. Among the topics discussed were: the present content and organization of the report on "Business Cycle Developments"; suggestions for improving the content and organization of the report; and consideration of the desirability of a popular guide to the report.

Members of the Technical Subcommittee on Business Cycle Developments are: Donald J. Daly, Gottfried Haberler, Bert G. Hickman (*Chairman*), Lawrence R. Klein, John P. Lewis, Geoffrey H. Moore, Arthur M. Okun, Frank E. Morris, Beryl W. Sprinkel, and Lorman C. Trueblood.

SOLOMON FABRICANT, *Chairman*

REPORT OF THE SPECIAL PUBLICATIONS ADVISORY COMMITTEE TO JOINT COUNCIL ON ECONOMIC EDUCATION

In September, 1962, a Publications Advisory Committee to the Joint Council on Economic Education was appointed by the Association, with the special task of advising on the preparation of new materials for use in teaching economics in the high schools. A major foundation has tentatively offered a substantial sum to support the preparation of such new materials under plans to be recommended by the Joint Council and the special Publications Advisory Committee. Members of the Committee are G. L. Bach (Chairman), Carnegie Institute of Technology; Kenneth Bolding, University of Michigan; Ben Lewis, Oberlin; James Reece, University of Oklahoma; and Leonard Silk, *Business Week*.

The Joint Council and the Publications Advisory Committee hope to involve several leading economists in the preparation of special teaching materials on economics in the high schools for use in both economics courses and in other courses such as American history, to supplement and improve available teaching materials which were considered by the Association's special evaluative committee on materials in the high schools to be inadequate.

Other major professional associations (for example, the American Anthropological Association, American Historical Association, American Geographical Society, and American Sociological Association) have established special groups to help in the improvement of teaching materials at the high school level. The National Science Foundation, U.S. Office of Education, and the Office of the Scientific Advisor to the President are consulting informally with these groups in the hope of encouraging a general upgrading of social science materials for use in the schools. This interest parallels comparable activities on the part of the professional societies and interested government agencies and foundations in the fields of mathematics, physics, chemistry, and the biological sciences.

G. L. BACH, *Chairman*

PROGRESS REPORT ON THE JOURNAL OF ECONOMIC ABSTRACTS

During the past year most of the conceptual, mechanical, and financial problems have been cleared away and the manuscripts of Volume I, Number 1, have been ushered successfully through page proofs in anticipation of publication the first week or so of January next.

On June 27, 1962, the Ford Foundation awarded to the American Economic Association, as proxy for the *Journal*, a grant of $40,000 to run for two years and designed to cover deficits incurred during the fledgling years. The $2.00 subscriptions should produce break-even revenues at around $8,000 paid subscriptions.

The printer is George Banta Company, Menasha, Wisconsin, the same as the *AER*. The format and style of the experimental issue has been accepted with minor modifications. The original list of 20 participating journals has been expanded to 32, of which 24 contributed to the first issue, 3 will begin with the second, and 5 are undecided.

The first issue, to be released in January, 1963, contains 24 journals, 148 abstracts, 171 authors, and will be approximately 145 pages long. Twenty-eight thousand copies will be printed and distributed at a cost of approximately $13,000. This issue will be free and available automatically to members of the AEA and the economic associations in Canada, England, India, Australia, and New Zealand. Others may have a copy on request. The *Journal* is now on a regular printing schedule and will appear the first month of each quarter.

Total money expended so far is approximately $1,300.

Thirty-five thousand copies of a descriptive leaflet are being distributed to the readerships of the contributing journals; 23,300 free copies of the first issue will go out automatically (see details of the first issue, below). Another (estimated) 4,700 will go to fill requests. In addition, special letters have been sent to 68 selected embassies in Washington, D.C., and to over 125 governmental agencies and private institutions in an attempt to reach potential readers not otherwise covered. Many of the participating editors have printed an announcement in their own journal in addition to distributing the printed leaflet.

Details of the First Issue

Number of copies automatically distributed:		
American Economic Association	15,200	
Economic Society of Australia and New Zealand	3,000	
Canadian Economic Association	1,500	
Royal Economic Society	3,100	
Indian Economic Association	300	
Econometric Society	200 (est.)	
		23,300
Estimated requests from others		4,700
Ordered printed		28,000

Estimated printing and distribution costs: $13,000.

CHARLES B. WARDEN, JR.

REPORT OF REPRESENTATIVES TO THE INTERNATIONAL ECONOMIC ASSOCIATION

During 1962 the International Economic Association held one conference in addition to the triennial meeting of the Council and the accompanying Congress.

The conference, on the subject, "The Theory of Interest Rates," was held at Royaumont at Easter. Its purpose was to bring together some of the younger economists particularly concerned with the techniques of economic analysis. The conference was organized by Frank Hahn.

The fifth (triennial) meeting of the Council of the Association was held at Vienna, August 29. This was followed (August 30 to September 6) by the Second Congress of the International Economic Association, open to all members of the constituent associations. The subject of the Congress was "Economic Development," and the membership of the American Economic Association was well represented on the program and in the meetings.

A new project undertaken in 1962, in co-operation with UNESCO, was the preparation of a textbook for students of economics in Africa. The project is part of a larger program of UNESCO to provide textbooks for students of economics in all underdeveloped countries. David Walker is serving as chief editor; and it is expected that the text will be completed by the spring of 1964.

International Economic Papers, No. 11, edited by A. T. Peacock, W. F. Stolper, Ralph Turvey, and Hans Liesner, appeared in 1962. The volume *Inflation,* proceedings of a conference of the Association in 1959 at Elsinore, was published under the editorship of D. C. Hague.

In 1963 the Easter conference will be on the subject, "Activity Analysis." In September a conference on "Economics of Education" will be held at Menthon St. Bernard (Lake Annecy).

At the meeting of the Council in Vienna the following officers and members of the Executive Committee were elected for the period 1962-65: G. U. Papi, President; E. Lundberg, Vice-President; F. Neumark, Treasurer; and E. James, D. Delivanis, E. Lipinski, B. F. Haley, V. P. Dyachenko, D. Patinkin, E. A. G. Robinson, and N. V. Sovani, members of the Executive Committee.

BERNARD F. HALEY
ROBERT M. SOLOW

REPORT OF REPRESENTATIVE TO THE AMERICAN COUNCIL OF LEARNED SOCIETIES

The American Council of Learned Societies was organized in 1919 to promote the intellectual interests of the humanities and the social sciences and to provide American representation in the newly formed International Union of Academies (Union academique internationale). Two academies, the American Philosophical Society and the American Academy of Arts and Sciences, founded in the eighteenth century, head the present list of ACLS constituent societies. Since the organization of the Social Science Research Council in 1924 the ACLS has given major attention to the humanities and borderline organizations not included in the SSRC. It should be noted, however, that the names of economists have appeared in the lists of ACLS grants and that several of the annual prizes of $10,000 have been awarded to members of the AEA.

Perhaps of greatest interest to economists are the activities of the ACLS on behalf of the intellectual professions in general, particularly through its relations with divisions or agencies of the U.S. government. Much of this work is done through joint committees, especially with the SSRC but also other bodies. The Annual Report of the ACLS for 1961-62 lists eleven such committees, with seven subcommittees, besides the Conference Board of Associated Research Councils, which includes the National Research Council (natural science) and the American Council on Education. The ACLS is also represented on three other joint committees: International Exchange of Persons, American Native Languages, and Recovery of Archaeological Remains.

The activities of the ACLS for the past year are outlined in the Annual Report of the President, Frederick Burkhardt, for 1961-62. As stated in the Preface, "Financially, it was the biggest year in the Council's history, with expenditures exceeding a million and a half dollars. Most of these funds were spent on aids to research—fellowships, grants-in-aid, travel grants and scholarly publication." One new program in linguistics was initiated, and the American Studies Program, which supports research and study of American culture in the universities of Free Europe (announced the year before), became active. Twenty-nine grants were made to foreign fellows, enabling them to spend a year in the U.S., and fifteen to European universities to assist them in establishing teaching posts in this field. Arrangements for a cultural exchange between the U.S.A. and the U.S.S.R. began with four scholars moving in each direction; in March a new agreement provided for an exchange of twelve to fourteen scholars and for a half-dozen lecturers during 1963.

Fifty-five persons received appointments as fellows of the ACLS for postdoctoral study, the stipends averaging $5,146. A new program of study fellowships was inaugurated for 1963-64 tenure. Grants-in-aid for individual research numbered ninety-six, averaging $1,224, including six publication subsidies. The report described six area programs, conducted jointly with the SSRC: Africa, Asia, Contemporary China, Latin America, Near and Middle

East, and the Slavic and East European Region. In the field of secondary education, the Council continued three programs and undertook two surveys as the basis for future action, and cosponsored two conferences: on teacher education and on school curriculum. Notably, a manuscript on the social studies, begun two years ago in co-operation with the National Council for that field, was completed and publication announced for the fall of 1962, entitled *The Social Studies and the Social Sciences.* A survey inaugurated on the present state of the social studies is to be done by Mr. Martin Mayer; and a survey was made of the ACLS constituent societies' activities related to teaching in the schools. With co-operation and under special grants, a two-day conference was held on teacher education and one of sixteen days on curriculum in the social studies and humanities.

Grants to European scholars for attendance at conferences in the U.S.A. and to Americans for such trips to Europe are important Council activities. Under the latter head, travel grants were made to eighty-three Americans for attendance at thirty meetings of varying size. A number of small conferences in this country on special topics were assisted. Among miscellaneous activities, the ACLS and the SSRC took over the Ford Foundation program of foreign area training fellowships. Work progressed on the third Continuation Volume of the *Dictionary of American Biography* and on the study of microfilming of scholarly materials was continued. The work of the Conference Board of Associated Research Councils was expanded, as were contacts with the State Department, notably the Assistant Secretary for Educational and Cultural Affairs, and also with UNESCO. In the field of American native languages, a joint committee with other associations published the *International Journal of American Linguistics.* Activities of the expanding Union academique internationale were strongly supported.

Two noteworthy resolutions were passed at the annual meeting of the ACLS, January 20 and 21, urging, respectively, the expansion under the National Defense Education Act of federal government support for Institutes for Secondary School Teachers and extension of support of higher education and research to include all the humanities and the social sciences on the same basis as mathematics, the natural sciences, and technology. Those present were urged to write their Congressmen, giving their views on the matter.

FRANK H. KNIGHT

REPORT OF REPRESENTATIVES TO THE SOCIAL SCIENCE RESEARCH COUNCIL

Much of the work of the Council that concerns economic research is conducted through special standing committees whose activities extend over several years. Below I shall report on developments in 1962, suggesting that previous reports in the "Proceedings" be consulted for continuity and perspective.

In the fall, 1961, the Committee on Agricultural Economics, under the chairmanship of Herman M. Southworth, began discussion of the need for research concerning the role of American agriculture in the world economy. In this connection, it is considering problems involving the improvement of agriculture in developing countries. It plans several research papers concerning these topics, and a number of handbooks or monographs for use by investigators in these fields. Of importance also was the publication in the *Journal of Farm Economics* (February, 1962) of "The Study of Interactions between Agriculture and the Nonfarm Economy: Local, Regional and National," by Karl A. Fox.

During 1962, the Committee on Analysis of Economic Census Data continued its review of several manuscripts deriving from studies initiated jointly with the Bureau of the Census. In April, the Yale University Press published Victor Fuchs's *Changes in the Location of Manufacturing in the United States since 1929.* Two related papers by Mr. Fuchs also appeared: "America's Changing Industrial Map," *Challenge* (February, 1962), and "The Determinants of the Redistribution of Manufacturing in the United States since 1929," *Review of Economics and Statistics* (May, 1962). Ralph L. Nelson's *Concentration in the Manufacturing Industries: A Mid-Century Report* will be published by Yale University Press in September, 1963. During 1962 the Committee also expects the completion of a manuscript by Richard H. Holton on the structure of the distributive system, and one by Richard and Nancy Ruggles on interrelationships of manufacturing prices and costs. James W. McKie is preparing two papers dealing with problems of industry classification and with relations between classifications, input-output analysis, and price indexes. Thomas Wilson has assumed responsibility for a study of large diversified firms initiated by Carl Kaysen.

The Committee on the Economy of China began its work in September, 1961, under the chairmanship of Simon Kuznets. Early in 1962 it met twice, where it decided that the need was urgent enough to justify a co-ordinated research program rather than individual grants. The Committee has worked up several proposed quantitative studies concerned with output indexes and particular industries. It also plans institutional studies of the communes, the banking and credit system, the planning mechanism, and management structures. A number of invitations have been extended to qualified scholars to undertake these investigations.

The Committee on Economic Growth, with Simon Kuznets as chairman, plans a conference in March, 1963, on demographic and economic trends in developing countries, jointly with the Population Council, Inc. In April, 1963, it will undertake another, dealing with the role of education in early stages of development, in co-operation with the Comparative Education Center at the University of Chicago. One of its earlier conferences has yielded a volume of essays edited by Richard R. Nelson, *The Rate and Direction of Inventive Activity: Economic and Social Factors,* published by Princeton University Press in March, 1962. Another has eventuated in a book edited by Abram Bergson and Simon Kuznets, *Soviet Economic Trends,* to be published by Harvard University Press early in 1963. Also deriving from Committee support is *British Economic Growth, 1688-1959: Trends and Structure,* by Phyllis Deane and W. A. Cole, published in 1962 by Cambridge University Press. The Committee also aided the preparation of N. G. Butlin's *Investment and Growth: Australia,* published by Cambridge University Press, 1962, and his *National and Domestic Productivity,* to appear in 1963. In addition, Henry Rosovsky and Kazushi Ohkawa are continuing their collaborative study of economic development in Japan. In January, 1962, the seventh of Simon Kuznets' essays on Quantitative Aspects of the Economic Growth of Nations, "The Share and Structure of Consumption," appeared as a supplement to *Economic Development and Cultural Change.*

The Committee on Economic Stability now is under the chairmanship of Bert Hickman. Those collaborating in the econometric model-building project, led by Lawrence Klein, held a second meeting, at Dartmouth, in August, 1962. The work involves two levels: an aggregative model with about 100 equations and a disaggregated one with about 300. The former model is much larger than others now in use, and it permits more effective inclusion of anticipations data. Also, government is treated where possible as an endogenous sector, while input-output analysis has been linked with models of final demand. At the 1962 meeting, single-equation least-squares estimates of aggregative relations were presented and criticized. In result, it was possible to formulate a flow diagram of the aggregative model, with summaries of the estimated equations. The next step will be to estimate the model as a whole and to undertake short-run forecasts and simulations. A preliminary statement of results will be presented at the Econometric Society in Pittsburgh at the end of 1962, and eventually a book will be prepared on the entire project. In addition to this project, the Committee has begun to concern itself with economic policy models here and abroad, and with the implications of statistical indicators and their connection with models of the business cycle.

The Committee on the Preservation and Use of Economic Data has met three times during 1962 and has undertaken exploratory talks with government officials concerned with preparation of large groups of statistical data. It is interested in increasing the availability of such statistics, and their deposit on magnetic tapes at leading universities wth computers.

Other commttees have also been active. The Committee on Mathematics in Social Science Research plans two more summer research institutes in 1963. The Committee on Population Census Monographs has seven studies under way in co-operation with the Bureau of the Census and expects to have several completed during 1963. These are described in detail in the *Proceedings* for 1961. During 1962, the Committee on Urbanization held four meetings to consider papers for inclusion in its projected monograph on the state of knowledge and research concerning urbanization, to be edited by Philip M. Hauser and Leo F. Schnore. Topics contemplated include the state of urbanization research in various disciplines, including economics; cities in less developed nations; and the growth and development of urban economy. In August, 1962, the Committee on Latin American Studies conducted an Inter-American Conference on Research and Training in Economics, at Santiago, Chile, jointly with the Instituto de Economia of the University of Chile.

Other Council developments of interest include transfer of the Foreign Area Training Fellowship Program from the Ford Foundation to a joint committee of the American Council of Learned Societies and the Social Science Research Council, directed by Schuyler C. Wallace. In addition, the Committee on International Conference Travel Grants made grants to seven economists to attend meetings of the Congress of the International Economic Association at Vienna, and to three others for meetings of the International Conference on Economic History, at Aix-en-Provence, France. Through its fellowship, grants-in-aid, and special program committees, the Council provided assistance to numerous economists, as in prior years.

Among economists participating administratively with the Council, Abram Bergson has retired from the chairmanship of the Joint Committee on Slavic Studies and has become a director-at-large. Gardner Ackley has resigned as director in summer, 1962, while William H. Nicholls retires in December. The undersigned continues as member of the Board of Directors representing the American Economic Association, serving also as a member of the Executive Committee and as Chairman, Committee on Social Science Personnel.

GEORGE H. HILDEBRAND

REPORT OF REPRESENTATIVE TO THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

A meeting was arranged at the annual meeting of the American Association for the Advancement of Science, sponsored by the American Economic Association and cosponsored by Section K of the AAAS. It was held on December 26 at 2:00 p.m. in the Tecumseh Room of the Bellevue-Stratford Hotel in Philadelphia. About fifty were present. The topic was a symposium on "The Economic Impact of Disarmament," Seymour Melman, Columbia University, presiding. Papers were:

1. *Strategy of Economic Adjustments to Disarmament*
   Emile Benoit, Columbia University
2. *Economic Impact of World Disarmament: The Quantitative Approach*
   Wassily Leontief, Harvard University
3. *The Impact of Arms Reduction on Research and Development*
   Richard R. Nelson, Council of Economic Advisers, Washington, D.C. (Read in absentia)

Discussants: Gerard Piel, Publisher, *Scientific American*
Alexander Kiefer, U. S. Arms Control and Disarmament Agency
Adolph Sturmthal, Yale University

The meeting was well reported and received good newspaper publicity.

The American Economic Association also cosponsored a meeting on December 27, at 8:00 p.m., on "Economic Growth: Some Theories and Concepts," with papers by Wilbert E. Moore, of Princeton University, and by Bert Hoselitz, of the University of Chicago, followed by the vice-presidential address of Section K, by Simon S. Kuznets, of Harvard University, on "Problems in Measuring Economic Growth."

Section K of the AAAS, with which the American Economic Association is affiliated, is growing in numbers but continues to be a weak section. However, the relationship is not taxing and seems well worth while to maintain.

KENNETH E. BOULDING

REPORT OF THE REPRESENTATIVE TO THE UNITED STATES NATIONAL COMMISSION FOR UNESCO

The various professional societies rotate terms for nominating members to the United States National Commission for UNESCO. The American Economic Association became entitled to this privilege in 1962, and Professor Kenneth E. Boulding, of the University of Michigan, was selected from a list of names proposed. Professor Boulding attended the meeting of the United States National Commission in Pittsburgh on October 12 and 13, 1962.

K. E. BOULDING

REPORT OF REPRESENTATIVE TO THE NATIONAL BUREAU OF ECONOMIC RESEARCH

The research program of the National Bureau of Economic Research covers five broad areas of economic interest: economic growth; national income, consumption, and capital formation; business cycles; financial institutions and processes; and international economic relations. Under each heading, a variety of projects are in various stages of completion.

Publications of the National Bureau fall into three categories: research reports, conference reports, and occasional or technical papers. In the first category, seven books were published during 1962, of which two related to the economy of the Soviet Union, and one was Goldsmith's *National Wealth of the United States in the Postwar Period.* Five special conference reports were published which dealt with inventive activity, the flow-of-funds approach, labor economics, investment in human beings, and philanthropy. In addition, six occasional papers were published of which three were in the field of finance.

Three new studies were begun during 1962 as follows:

A grant from the National Science Foundation was received for a study of comparative domestic and foreign prices and their relation to the balance-of-payments situation.

Grants from several companies will finance a study of the record of short-term economic forecasting as done by governmental agencies, private institutions, and individuals.

A new study in the urban mortgage financing field will examine the relationship between loan terms, borrower characteristics and loan experience on residential mortgages.

Harold M. Groves has continued to serve as Chairman of the Board, Arthur F. Burns as President, Solomon Fabricant as Director of Research, and Goeffrey H. Moore and Hal B. Lary as Associate Directors of Research. Individuals who were appointed members of the research staff on a part-time basis are: Challis A. Hall, Jr., Irving B. Kravis, Victor R. Fuchs, Albert Fishlow, and Victor Zarnowitz.

WILLARD L. THORP

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1963

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The American Economic Review, **March, **June, **September, and December

**Supplement (No. 2—May).—Sixty-eighth Annual Meeting:

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